Blacklist Kills Video Stars

The perverse career effect of 'Blacklists' on the movie workers in South Korea

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Abstract

A burgeoning literature firmly establishes that democratic backsliding leaves negative imprints in various domains. However, this empirical regularity has not been extended to the realm of culture despite ample anecdotes pointing to the detrimental effect of democratic decays on the very backbone of cultural dynamism, the freedom of expression. To fill this lacuna, the paper documents the case of deliberate infringement on freedom of expression in South Korea during its recent backsliding period (2008-2017). Using a difference-in-difference model on a original individual-level panel dataset, I report that the government's Blacklist project significantly damaged the careers of Korean movie workers, particularly those invisible from the public. The paper suggests that the consequence of a backsliding government's attempt to control public discourse can be surprisingly comprehensive because it instills voluntary sensorship in the industry.

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"*Why have you not done anything about these [anti-government] movie makers yet?*"

– Kim Ki-choon, Park Geun-hye's Chief Secretary.

1 Introduction

Does culture suffocate when democracy decays? One way in which cultural dynamism can be lost is through suppression of freedom of expression. Not only does the freedom of openly criticizing the authorities put pressure on the government to address social demands (Bailard, 2012), it also enables the legislative and judicial bodies of the government to effectively monitor the executives (Berliner, 2014). A backsliding democratic government might attempt to unshackle itself from these institutional and social constraints by restraining freedom of expression. Such a suppression of freedom of expression limits the diversity of ideas in a society. Cultural industries that flourish with a diversity of ideas, thus, are likely debilitated when democratic backsliding unfolds.

This anti-cultural consequence of democratic backsliding is hardly unheard of in recent years. Backsliding cases in Hungary (Tsioulcas, 2022) and Poland (Sethi et al., 2022), for example, suggest that arts and popular culture withers quickly upon governments' de-platforming artists deemed inconsistent with the nationalist ideologies. On a more general level, a large body of literature documents the damage that non-democratic regimes wrought upon cultural industries in the twentieth century (Merziger et al., 2019; D'Antonio, 2020).

However, systematic investigations into this subject are rare. Although the burgeoning literature on the consequences of democratic backsliding has gained significant traction in recent years, the empirical domains of this body of research seem confined to the economy (Nelson and Witko, 2022; Szikra and Öktem, 2022), public health (Wigley et al., 2020; Son and Bellinger, 2022), foreign policy (Rüland, 2021; Tschantret, 2020) and bureaucracy (Bauer and Becker, 2020). In these studies, the limitation of freedom of expression, which subsequently stifles culture, is often regarded as a mere unfortunate byproduct of backsliding. Despite valuable descriptive studies on Turkey (Öztürk, 2020) and Eastern European countries (Cendic and Gosztonyi, 2020), there seems to be a glaring gap

in the literature in terms of providing a clear theorization and conducting rigorous empirical analysis to understand whether and how democratic backsliding infringes upon freedom of expression.

To fill this lacuna, the paper examines the case of 'Blacklist' in South Korea. During the recent democratic backsliding period of the country (2008–2017),[1] the government created secret lists identifying allegedly anti-government or left-leaning figures in popular culture. These included poets, writers, celebrities, academics, and movie workers. Comprehensive investigations confirmed that the authorities purposefully used these lists to suppress dissident voices (Noh, 2018). As an example of blatant attack on freedom of expression, the Blacklist case offers a rare empirical space where the consequences of backsliding on popular culture can be examined systematically.

Of particular interest in this paper is assessing whether, how, and to what extent the Blacklist had adverse effects on individuals in the Korean cultural industries. I posit that being blacklisted has deleterious career effects on the victims through two mechanisms: 1) directly via government operations and 2) indirectly through self-censorship in the industry that Blacklist induced. The latter mechanism suggests that the scope of the effect was much wider than what the government operations alone could have achieved.

Using an original panel data set of Korean movie workers, I present difference-in-difference estimates consistent with this empirical expectation. The estimates point to dramatically diverging career paths of the blacklisted and those who were not. The negative effect was substantially stronger on the non-actor movie workers than on the actors because the careers of the latter are more publicly visible than those of the former. The suppression of freedom of expression grows more blatant when the reputational and business costs involved are smaller.

The paper contributes to our understanding of democratic backsliding in several ways. First, it expands the empirical purview of the backsliding literature to the cultural realm.

[1] It is worth acknowledging the perspectives challenging this identification of the end period of backsliding in South Korea, be it epistemologically (e.g., Hur and Yeo, 2023) or ontologically (e.g., Shin, 2020). While these perspectives may warrant further debates, the empirical analysis presented in this paper is not affected by when the backsliding episode was terminated. The difference-in-difference design presented below concerns primarily the timing of the treatment—the beginning of the Blacklist operation.

In addition to the generally detrimental effect it has on tangible public goods such as development and health, the paper shows that backsliding damages an intangible one, namely, cultural dynamism. The damaging career effect of Blacklist is estimated to be surprisingly comprehensive and conspicuously cumulative, which suggests that the damage on the intangibles is no less significant than that on the tangibles. As such, the paper brings to the researchers' attention to a crucial, but overlooked, consequence of backsliding.

Second, by highlighting the hard-to-observe consequence, the paper joins the ongoing discussion on the ontology of democratic backsliding. While a large body of literature points to a global wave of democratic backsliding currently unfolding (e.g., Boese, Lindberg, and Lührmann, 2021; Hellmeier et al., 2021), others contend that the existence of such a wave is questionable particularly if the conceptual confines of backsliding are guided by 'objective' indicators (Little and Meng, 2023). The cultural consequences of backsliding highlighted in this paper imply that a narrow conceptualization of democratic backsliding focused solely on 'vertical' or 'horizontal' dimensions of democracy could overlook the full extent of democratic erosion, particularly those in the 'diagonal' dimension such as suppression of freedom of speech (Lührmann, Marquardt, and Mechkova, 2020). This possibility of under-diagnosis is more pronounced when considering that government attempts at suppressing freedom of speech can often be secretive and covert and yet have significant real-world impacts, as the Blacklist case suggests.

Third, the present study also identifies a unique way in which backsliding political leaders attempt to control public narratives. Extant literature has focused on censorship activities in the news media outlets (e.g., Hayes and Reineke, 2007) or those in online opinion forums (e.g., King, Pan, and Roberts, 2013). These studies highlight the suppression strategies of shutting down the 'product' or the medium of potential dissenting voices. The paper reports that an alternative government strategy would be to de-platform the 'producers' of these voices in popular culture. The paper also uncovers the heterogeneous nature of the negative cultural consequences of backsliding: less-visible, lower-profile victims are more adversely affected than the higher-profile, more popular ones. The effect of backsliding, it suggests, is not equal to everybody.

The paper consists of five sections. The section following this introduction discusses the possible linkage between democratic backsliding and cultural dynamism via suppressing freedom of expression and undermining democratic accountability. The nature and scope of Blacklist and related government operations are also discussed. The third and fourth sections detail the strategies for, and the results of, the empirical analysis, respectively. The concluding section summarizes the findings and discusses their implications.

2 Democratic Backsliding and Blacklist in Korea

2.1 Democratic Backsliding, Freedom of Expression, and Culture

Democratic backsliding occurs with changes in the "qualities associated with democratic governance" within a polity (Waldner and Lust, 2018, 95). While this 'quality' can be defined in various terms, scholars agree that a backsliding episode unfolds when the executive branch of a government cannot be held accountable for its actions (Bermeo, 2016; Kaufman and Haggard, 2019; Lueders and Lust, 2018).

Drawing on the Dahlian approach to democracy (Dahl, 1971), 'holding governments accountable' can be understood in terms of "to whom, for what, and how" government actions are constrained (Lührmann, Marquardt, and Mechkova, 2020, 2). How the actions of the executive body of a government are limited by other governmental actors such as the judiciary or legislature ('horizontal accountability') and how a government is responsive to, and sanctioned by, citizens through elections ('vertical accountability') emerge as empirical focal points in the studies of democratic backsliding. Scholars suggest that when inter-institutional checks and balances are rendered ineffective (e.g., Levitsky and Ziblatt, 2018) or electoral integrity is undermined (e.g., Dresden and Howard, 2016), backsliding unfolds.

On the contrary, in systematic studies of backsliding, 'diagonal accountability,' which is "the extent to which actors outside of formal political institutions [such as civil society and the media] ... hold a government accountable" (Lührmann, Marquardt, and

Mechkova, 2020, 2), seems to attract relatively scant attention. Freedom of expression serves as a critical instrument that enables this accountability mechanism by allowing the relevant actors to observe government performance. Limiting freedom of expression, however, is often understood as a mere unfortunate byproduct of declining horizontal or vertical accountability (e.g., Riaz, 2021), not as one of the core processes of democratic backsliding. This might be in part because unlike political institutions and citizens, the media and civil society are not 'principals' themselves and cannot directly sanction the government (Mechkova and Pernes, 2019). Lorch (2021), for instance, contends that in South and Southeast Asia, a vibrant civil society was simply insufficient to hold off backsliding when institutions were frail.[2]

Because media and civil society actors primarily operate in cultural realms, this paucity of attention to diagonal accountability leads to under-investigation of cultural effects of democratic backsliding. In the empirical literature, courts, legislatures and election offices are considered the domains where democratic backsliding is observed whereas newsrooms, theaters, and social forums are not. This oversight of culture is puzzling because numerous anecdotes hint at the systematic cultural effect of democratic backsliding across the board. Descriptive studies and news reports on India (Lulz and Riegner, 2021), Poland (Belavusau, 2018), the Philippines (Ragragio, 2021), and Hungary (Cendic and Gosztonyi, 2020) document explicit and implicit restrictions of freedom of expression, resulting in withering cultural activities. These ample anecdotes, however, have not yet led to much systematic empirical scrutiny. The scope and depth of the negative cultural effects of backsliding remain unexplored, consequently.

The problem is compounded by the covert nature of government operations. Backsliding governments' attempts to suppress potential dissident voices, often borrowed from traditional authoritarian tactics (Preda, 2017), tend to be secretive and intricate. In Hungary, for example, musical shows were canceled at theaters not by a direct government order, but due to the pro-Orbán media outlets' public campaigns (Tsioulcas, 2022).

[2] Laebens and Lührmann (2021) find strong civil society to be a deterrent against backsliding only under specific conditions.

Likewise, the ruling Law and Justice Party of Poland successfully "infiltrated" art institutions, thereby stacking up self-censoring allies among artists and practitioners (Sethi et al., 2022). In the end, this covertness of operations further hinders the identification of the cultural consequences of backsliding. Researchers do not have much more than loosely interrelated episodes implying the potential for compromised diagonal accountability resulting from backsliding. The literature, thus, can benefit from a systematic empirical approach that clearly delineates whether and how backsliding governments damage cultural industries beyond descriptive studies. The following section explains how the Blacklist case of South Korea offers a rare opportunity for such an investigation.

2.2 Democratic Backsliding in South Korea (2008-2017) and Blacklist

Democratic backsliding unfolded in South Korea between 2008 and 2017, a period that corresponds to the presidencies of Lee Myung-bak and Park Guen-hye. Substantial declines in its democratic accountability are observed in multiple dimensions: the use of the National Intelligence Service (NIS) for political surveillance (Sung, 2017), electoral interference through online manipulation (Doucette and Koo, 2016), the dissolution of the United Progressive Party in 2014 through a questionable process (Kim, 2017), and allegedly tampering with judicial independence (Kookmin Ilbo, 2018). The first two concern primarily the declines in vertical accountability and the other two are closely related to weakening horizontal accountability although elements of both dimensions exist in all cases.

Directly relevant to the present paper are the parallel declines in diagonal accountability in general and freedom of expression in particular as demonstrated in Figure 1. The early years of Lee Myung-bak presidency (2008, the first vertical dashed line in the figure) saw a significant fall in the level of freedom of expression (Pemstein et al., 2021), which dipped to a level that had not been seen since the early 1990s—the years when the democratic transition was still in the process. In Park Guen-hye's presidency beginning in 2013, freedom was further restricted and was not recovered to the pre-backsliding level until the presidential impeachment in 2017 (second vertical dashed line). The systematic

abuse of the criminal defamation law to protect government officials (Park, 2017), hunting down an artist depicting the president as a rat (Bae, 2010), and the violent suppression of the Candlelight protests (Lee and Anderson, 2013) are some of the well-known examples representing these declines in diagonal accountability. Up until the post-2017 recovery, the pattern runs almost in parallel with that of Hungary where President Victor Orbán "has strived to take over the public service media" (Lendvai, 2017), or Poland where the 'Memory Law' was suspected to limit freedom of speech (Belavusau, 2018).

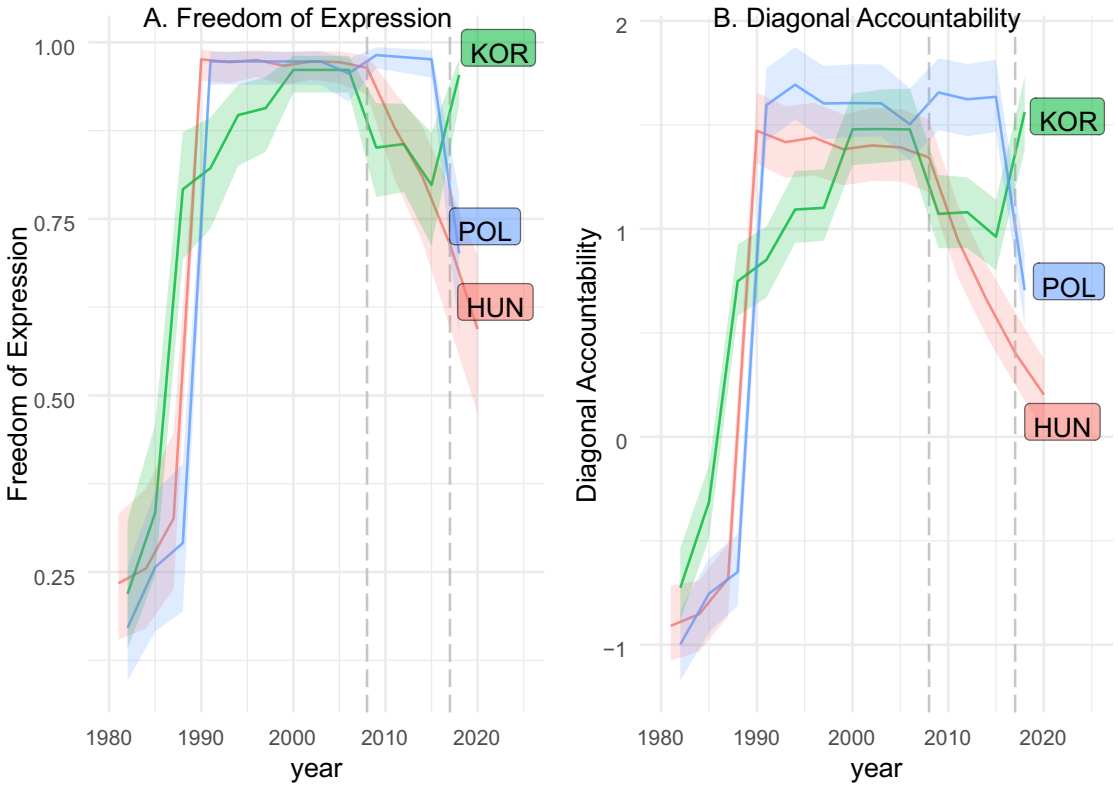

Figure 1: South Korea in a Comparative Perspective. KOR=South Korea; POL=Poland; HUN=Hungary

It is against this backdrop that Blacklist operations were carried out. Scholars suggest that the goal of the operation was to curtail the public presence of seemingly anti-government or allegedly left-leaning movie workers in the industry, essentially mimicking the old authoritarian tactics of using popular culture as means to silence dissents (Yuk 2019; Cho 2018; Kim 2018).

The post-impeachment Moon government established the 'Committee on Culture-Art Blacklist Investigation and Institutional Improvement' (or simply the 'Blacklist Committee') and opened an extensive investigation into the scope and depth of these lists, which mirrors the activities of post-democratization 'lustration' and 'truth commissions' (Nalepa 2022). The report published by the Ministry of Culture and Tourism offers the details of how the lists were compiled and used. There were essentially four lists, although they could be further disaggregated in different classifications (Blacklist Committee, 2019*a*, 30-31). The first one was created in 2008 as a response to the nationwide 'Candlelight Protest' against the Lee Myung-bak government's decision to loosen the import restriction on American beef. The protest escalated into a legitimacy crisis of the president and is generally considered to have led to an early lame duck. Led primarily by the NIS (Yeonhap News, 2017), the cornered government identified "problematic" public figures, most of whom openly criticized the government's violent handling of the crisis. This list was a relatively short one, containing eighty-two individuals, sixty of whom worked in the movie industry (Kyeonghyang Sinmun, 2017). There is some evidence indicating the existence of additional lists created by NIS during the Lee government in subsequent years, although they were focused almost exclusively on television shows (Blacklist Committee, 2019b, 346-347).

Table 1: Four Blacklists

List Number	Year Based	President when completed	Primary Reasons
1	2008	Lee	openly critical of the government, particularly during the Candlelight protest; 'left-leaning' (*only 60 movie workers*)
2	2012	Park	openly supporting the opposition presidential candidate, Moon Jae-in

3	2014	Park	participants of the open letter regarding the Sewol Ferry sinking; openly supporting the opposition candidate for Seoul Mayoral election, Park Won-sun
4	2015	Park	participants of the open letter criticizing government handling of the Sewol Ferry sinking

* 'Year Based' is the year in which the event that the list is based on occurred. 'President when completed' refers to the president in office when the list was reportedly completed. The data are based on Hankookilbo (2016) and Blacklist Committee (2019a).

The second, third, and fourth lists were completed during the Park Geun-hye presidency, although the first one was likely initiated and utilized before the inauguration.[3] These lists were much more comprehensive and disproportionately larger in size than the first one. The second list (#2 in Table 1), in particular, identifies the overwhelming majority (6,517) of those ever blacklisted (9,473). As summarized in Table 1, openly criticizing the government or supporting opposition candidates was the primary way in which an individual's name could appear on the list. But, other than the short, blanket reasons described in Table 1, the lists themselves do not contain detailed information about individual entries. The scope of these lists implies that it would be implausible to assume that each and every case was vetted through a separate scrutiny. Instead, the names appearing on allegedly anti-government open letters—at least twenty five of them confirmed to be tapped into by NIS—were simply wholesale copied onto the blacklists as the report by Ministry of Culture and Tourism confirms (Blacklist Committee, 2019*a*, 39-41).

[3] Ministry of Culture and Tourism (Blacklist Committee, 2019c, 28, Table 3.1) notes, for example, that NIS, "instantaneously with the inauguration of the Park government," submitted one of the core documents on which Blacklist later drew on to the presidential office. This timing implies that the beginning of creating the list was likely well before the early 2013. In fact, there exists some evidence that the second list was applied as early as late 2012 (Blacklist Committee, 2019d, 239-240).

The lists entailed substantive actions once completed at various levels of the government and other public advisory and/or promotion agencies for cultural industries. Blacklist Committee (2019*a*; 2019*d*) offers details of these actions. Some of the common patterns can be summarized as follows:

- interfering with potential employment of the blacklisted in art-related public agencies;
- pressuring art producers and supervisors to lay off, or stop renewing the contract with, the blacklisted;
- denial of funding, award, or financial support for the blacklisted or an entire art project that involved the blacklisted (e.g., career development grants);
- mobilizing pro-government, right-wing civic groups to sue the blacklisted artists on charges of defamation;
- direct interference with the theatrical release of a movie that involves the blacklisted;
- secret online disinformation campaign to tarnish the reputation of the blacklisted.

These actions would have adverse effects on the careers of the blacklisted through two mechanisms. First, the operations would simply generate direct career effects on the targets. Removing the blacklisted individuals from current and potential projects and employments would by definition curtail their careers. Given the long-entrenched dependence of cultural industries in Korea on tangible and intangible public support (Ryoo and Jin 2020), the tactics of denial of funding and award opportunities would also effectively suspend, if not cancel, numerous potential projects. The careers of those involved in these projects would suffer, consequently.

The first mechanism is intuitive and rather unsurprising. But it raises an important question regarding the scope of the effect of Blacklist. If these direct operations were the only ways in which Blacklist undermined the careers of the blacklisted, the scope of the effect must have been limited and the actual cases of career derailment, sparse. The primary agents of the operations, fractions of NIS, Ministry of Culture and Tourism, and the presidential office, were unlikely to be equipped with the vast amount of extra resources

to support over 9000 operations. The covert nature of the operations (Sung, 2017) would complicate this limitation even further. Indeed, the investigation by Blacklist Committee (2019a, 5) notes that some of the blacklisted were not the active targets of direct operations but instead kept in the lists for "surveillance purposes."

Another mechanism, by contrast, implies that the negative career effect of the lists would be much more comprehensive as it is predicated on Blacklist instilling self-censorship in the industries. Self-censorship in this context conceptually subsumes two different kinds of actions that industry participants might take without direct state coercion or order: 1) refraining from exercising their own freedom of expression and 2) suppressing others' freedom of expression. The latter, voluntarily censoring others,[4] is of particular relevance to the present paper as it explains the surprisingly wide-spread career effect of Blacklist. There are examples where rank-and-file government and public agents voluntarily censored movies and movie workers even without reporting to higher authorities (Blacklist Committee, 2019*c*, 40-42). Industry participants on the ground were also well aware of the rampant censorship as the rumor that the government was enforcing 'certain lists' spread through personal networks (Hankyoreh 2017). Many proactively excluded the 'problematic' individuals from projects (Shin, Jeong, and Park, 2022). Public agents and industry participants were sometimes given not much more than generic "guidelines" without specific references to the lists for evaluating cultural projects for government support or determining movies to screen. They nonetheless excluded scertain individuals and their projects from funding opportunities and screening based on Blacklist (Blacklist Committee, 2019*b*, 232-234).

This pattern of state repression on freedom of expression leading to self-censorship is in fact a familiar story in literature. Studies on Hollywood's Red Scare period, for instance, note that self-censorship was prevalent in times of state suppression of freedom of speech. Numerous studios, actors, and other movie workers not only 'repented' their liberal

[4] 'Self' in this context denotes 'voluntary' rather than 'their own,' therefore. This conceptualization of 'self' (censorship) follows the literature on the Hollywood Red Scare (e.g., Joo, 2010, 135; Lewis, 2000, 5).

views but also pre-emptively denied their personal connections with the accused (Joo 2010; Pontikes, Negro, and Rao 2010). As Ong (2021) discovers in the Southeast Asian context, individuals are likely to hedge against possible state repression by voluntarily undermining their and others' freedom of expression.

As such, the self-censorship induced by Blacklist suggests that its indirect consequences could be more far-reaching and profound than the direct operations carried out by government agents through orders. Risk-averse members of the industry would pre-emptively exclude the blacklisted from their business even in the absence of direct government threat aimed at themselves. This course of action seems reasonable given the enduring remnants of developmental state where cultural products served as vehicles for the state ideology in exchange for policy support. Therefore, proactive and prompt self-censorship would not be unfathomable in the Korean movie industry (Hong, 2019; Park, 2002). Sensing the overall 'tight' environment, for instance, a casting director well-versed in old policies would be inclined to exclude individuals known for 'lefty' ideas from the project. Such decisions were made even easier when and if these individuals committed themselves to public actions such as partaking in an open letter supporting the opposition candidate or condemning government policies. This is indeed how some individuals eventually blacklisted were affected by the lists even before their names actually appeared on the list.[5]

It is, thus, plausible to expect Blacklists to have comprehensive adverse effects on the victims' careers in the cultural industry. The blacklisted would be removed from the pool of potential employees. Often, an entire project was denied the opportunity for public recognition, which was crucial to the participants' career advancement. Lacking the protection of long-term employment arrangements, their careers were highly susceptible to changes in reputations and track records (Cho 2018). Where direct government actions

[5] For example, some movie workers were fired from the government-funded 'Indi-Plus' theater in 2013, well before their names appear on Blacklist, because they participated in a project, "the Jam Docu Kang-jeong" (2012), a documentary about the protest against a naval base project (Blacklist Committee, 2019*d*, 417–418).

were not carried out, self-censorship still operated. A hypothesis reflecting this empirical expectation is as follows:

- Hypothesis$_1$ (H_1): Being blacklisted decreases employment opportunities in the cultural industry.

While the scope of the effect of Blacklist is expected to be far-reaching, its intensity might not be uniform among all those affected. Two conditioning factors are at work. First, the authority had to consider the possible popular backlash that Blacklist coming to light could incite. Modern authoritarian leaders tend to avoid being portrayed as overly repressive and violent on culture. They find covert censorship regimes to be a cheaper and more effective option than direct repression on society to convince the public of their legitimacy (Guriev and Treisman, 2020). Because the entire operation hinges on the information manipulation, keeping the censorship covert is of great importance for these leaders. The Blacklist operations were deliberately kept secret, often involving complex chains of command system including the NIS units (Kim and Kim, 2017). This need for covert operations suggests that public recognition could negate the effect of Blacklist. From the authority's perspective, a large swath of famous individuals suddenly disappearing from public eyes would risk exposing the operation and, thus, the popular backlash.

Second, where self-censorship applies, the corporate risk of excluding 'stars' points to a similar conditioning effect of visibility. The commercial fate of large-budget projects in the Korean cultural industry has been determined significantly by a select group of stars from early on (Lee and Chang, 2009). The industry might have to strike a balance between excluding blacklisted individuals from projects and risking commercial success by doing so. Public recognition features prominently in such a decision. For example, a business-savvy director would be reluctant to forgo a superstar actor, albeit blacklisted. She would have a much easier time excluding a blacklisted assistant location manager from the project. In other words, the blacklisted would have to bear the full brunt of the operations when they are not popularly known. A hypothesis reflecting this conditioning effect of the public visibility is as follows:

- Hypothesis$_2$ (H_2): The negative career effect of being blacklisted is greater for those with less public recognition.

3 Research Design

3.1 Data

To test the two hypotheses proposed above, I use an original panel data dset containing information about the career trajectories of 'movie workers'—those who work in the movie industry, including actors, staff, and producers—in South Korea. The focus on movies as opposed to television shows or plays is driven primarily by the time series data availability. While not all the blacklisted are movie workers, obtaining comprehensive individual-level data from fields other than movies in the South Korean cultural industry is difficult, if ever possible. On the contrary, the Korea Box Office Information System (KOBIS) tracks over time all movie workers who have ever participated in a movie project officially released (or set to be released) in a Korean theater. The database, therefore, enables the construction of a panel data set where the unit of observation is the movie worker-year (KOFIC 2022).

Using KOBIS's application programming interface (API) through numerous iterations, I retrieved the data for all movie workers that the database recognizes. Although for some workers the data goes as far back as the 1920s, the quality of the data before 1990 is rather questionable. Even up until the late 1990s, the sheer volume of movie workers identified in the database is puny compared to that of the post-2000 period, suggesting the possibility that a small number of individuals are over-represented during this period. Therefore, the observations before 1990 are only used to measure individuals' careers and otherwise excluded from the sample. The last year available in the data is 2022 (March 12). Similarly, the cast of all 'foreign' movies, which KOBIS unambiguously identifies, are excluded from the dataset.

3.2 Outcome Variable: Movie Worker Career

The primary dependent variable is the logarithm of the number of movies where the movie worker is 'employed' in a given year. A numeric value of one is added (i.e., $ln(movies + 1)$) such that the variable is zero, not 'missing,' in a year she is completely unemployed. Because a substantial number of the observations are zero (i.e., not working for any movie in the year), a binary dependent variable that is coded as one when the movie worker has worked for any movie in the year and zero, otherwise, is also used as an alternative outcome variable.

It is worth noting here that the number of movies a worker worked for as count data may not be directly usable for the outcome variable for a couple of reasons. First, the presence of numerous zero observations and overdispersion require maximum likelihood estimators such as (zero-inflated) negative binomial regression. Using such estimators, however, presents well-known computational challenges to obtaining unbiased difference-in-different estimates (Guimaraes, 2008) with the highly unbalanced panel data structure stemming from different 'debut' years of movie workers. Second, when attempted nonetheless, the negative binomial fixed effect model simply did not converge.

3.3 Treatment and the Treated

A dummy variable, *Blacklisted*, capturing whether a movie worker was on Blacklist (i.e., 'treated') in a given year was created by matching the movie worker-year data set retrieved from KOBIS with the four Blacklists. The lists used are available from a Newstapa online article (Newstapa, 2017) which was originally reported earlier in 2016 (Hankookilbo, 2016).

In most years of the data, the treated account for no more than 20% of the observations. It is pertinent to emphasize that Blacklist contains only information about the fields (e.g., movie, art, or academy), and the source it tapped into (e.g., an open letter co-signers list). The exact identification of most of the blacklisted individuals would be impossible in many cases. This does not necessarily pose a challenge to the empirical analysis. Given

that most non-actor movie workers were not high-profile public figures, it would be reasonable to suspect that the career effect of blacklisting could potentially apply to anybody whose name matches those on the list. Only a handful of very well-known celebrities would be exceptions. As such, the coding of the treated variable does not distinguish between homonyms.

A dummy variable coded one for the year 2012 and later is the primary treatment variable (*PostTreatment*). 2012 marked the presidential election year. The disproportionate majority (about 85%) of movie workers that eventually ended up on Blacklist were identified by their open support for the opposition presidential candidate, Moon Jae-in. Most of the support came in the form of signing open letters published online and in newspapers (Blacklist Committee 2019d, 365). These letters served as a convenient tool for the authorities to identify 'anti-government' movie workers. The earliest time at which the far-reaching effect of Blacklist took place is likely the election year, 2012, although the full-completion of the list for this particular occasion was reportedly in 2014. In other words, by late 2012, the authorities already had in their hands multiple lists containing some of the names of these movie workers. These lists were only merged into one big list in 2014, which marks the 'completion.' There is also anecdotal evidence that the authorities started pressuring the industry to cut ties with these individuals as early as 2012 (Blacklist Committee, 2019*c*, 238–241).

In addition to the presidential election, numerous 'events' where the potential entries into Blacklist could be easily identified took place in 2012 (Son 2024). For example, high-profile movies and documentaries shedding critical light on the president's major policy failures such as the 'Yong-san Tragedy,' the 'US Beef Crisis,' and 'Kang-jung Naval Base' were produced in this year. This political landscape put the government on the defensive, increasing the need for aggressive public messaging operations (Kim, 2021). As such, the exclusion of the 'anti-government' movie workers from government funding programs, movie contests, or public sector jobs started this year even though most of them actually made the list later (Blacklist Committee, 2019*d*). All in all, year 2012 is determined to mark the beginning of the treatment period where a large number of movie workers started getting affected by Blacklist. Nonetheless, I also implement an alternative estimation

strategy (Sun and Abraham, 2021), considering the possibility that the treatment timings are meaningfully 'staggered' such that there exists a substantial variation in the timing of treatment for each individuals. Whether the primary estimates remain robust to an alternative model accounting for this staggered treatment timing is tested.

As covariates, the number of years that have elapsed since a movie worker's 'debut' (the year in which the movie worker first appeared in the data which dates as far back as 1920) as well as the number of movies she has so far worked for are included. As with the outcome variable, one is added for the logarithm (i.e., *ln(years_since_debut* + 1*)* and *ln(number_of_movies* + *1)*). The expectation can be either that a movie worker might be able to secure more jobs as her career progresses or that a movie worker (actors in particular) who has passed the most productive period after certain years into his career might face diminishing chances of employment over time.[6]

3.4 The Difference-in-Difference Model

Ideally, the causal effect of Blacklist could be obtained by comparing a) what would have happened to the careers of the blacklisted without Blacklist with b) what happened to them in reality. Since the former is never observable, a preferred alternative taking advantage of the panel data set is difference-in-difference (DiD), one of the most popular quasi-experimental approaches (Roth et al. 2023). The idea in the present context is to compare the career trajectories of the blacklisted with those not on the lists before and after the treatment. An ordinary least squared (OLS) model with layers of fixed effects is used to obtain DiD estimates reflecting the effect of being blacklisted on a movie worker's career. A simple functional form of the model can be written:

[6] In addition, 1) a dummy variable capturing whether an individual appears in multiple lists; 2) the number of lists the same individual appears; and 3) a dummy variable identifying the generation that went to college in the 1980s (so-called the '386 generation') were tried as control variables. As the coefficients and standard errors of the $Blacklisted_{it} \times PostTreatment_t$ appear identical to those of the benchmark (even at the fifth decimal point), those results are not reported here to spare space.

$$ln(movies_{it} + 1) = \beta_0 + \beta_1 Blacklisted_{it} \times PostTreatment_t + \beta_2 Blacklisted_{it} + \beta_3 PostTreatment_t + \mathbf{X} + FE(unit, year, role) + \epsilon_{it}, \quad (1)$$

where β_1 is the quantity of interest, representing the average treatment effect on the treated (ATT). I also refer to β_1 along with its standard error as the 'benchmark DiD estimate' to indicate the primary statistical result produced by this model, against which empirical alternative could be compared. **X** is a vector of covariates. For the hypothesis that Blacklist curtailed movie workers' careers (H_1) to be supported, β_1 should be significant and negative, which would indicate that the careers of the blacklisted, or the 'treated,' deteriorate significantly more than those of the unlisted after 2012.

In addition to the standard two-way fixed effects (i.e., movie-worker and year fixed effects) for the DiD model, 'role' fixed effects are also applied. The inclusion of the role fixed effect is crucial, in that the timing at which the treatment effect is actually observed can be endogenous to roles. Directors and actors might be chosen at the early stage of a movie production while many of the staff might not. In addition, there is an inherent difference in the number of projects that a movie worker can be employed for in a year. Certain staff (e.g., art directors) may work for multiple movie projects simultaneously while others (e.g., screenwriter) cannot, for example. The role fixed effect accounts for this heterogeneity. There are 127 roles, 32 years, 114,266 movie workers, and 1,216,866 worker-year observations identified in the data used for the benchmark model.

The same model can be used to test the second hypothesis when a more restricted sample is used. The strategy is to exploit the inherent difference between actors and non-actor movie workers. Actors are generally much more visible to the public and considered to be a more decisive and irreplaceable factor in determining the commercial success of a movie project by producers (Lee and Chang, 2009). Therefore, when the actors are excluded from the sample, the effect of Blacklist, β_1, should be larger and more significant as H_2 posits.

4 Results

4.1 Descriptive Examination

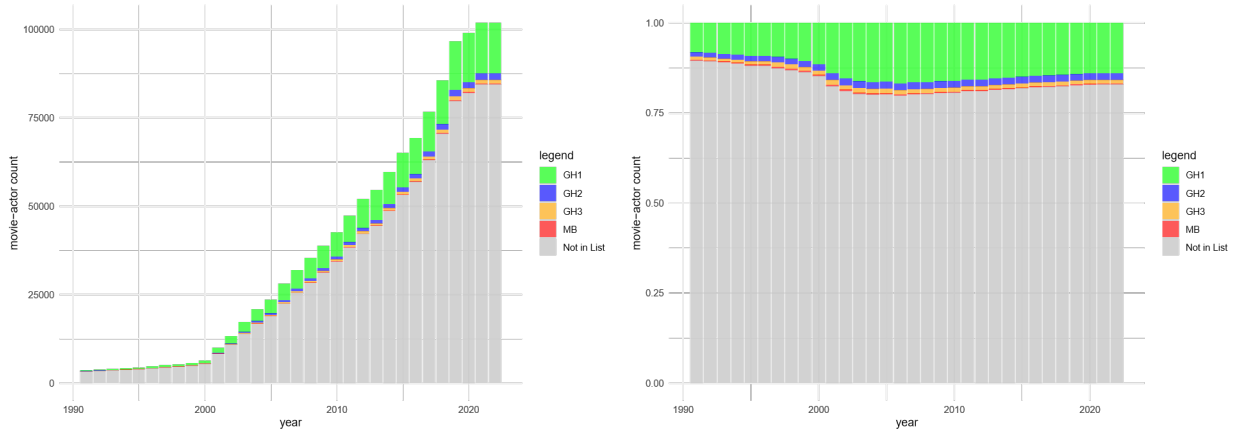

Figure 2: Movie Worker Trends, counts and ratios. GH1, GH2, GH3, and MB correspond to List number 2, 3, 4, and 1 in Table 1 respectively (i.e., first, second, and third Blacklist in Park Guen-hye government and the one in Lee Myungbak government). Data source: KOBIS.

Figure 2 presents the trend of the share of the blacklisted in comparison to those never blacklisted both in terms of absolute numbers (left panel) and ratios (right panel). The backlisted are disaggregated into each of the four lists identified in Table 1. Regardless of the exact year of blacklisting, those included in each list are identified as blacklisted to visualize the before- and after-the-treatment patterns for the treated as opposed to the untreated in this figure.

Some notable observations emerge from these descriptive figures. First, the South Korean film industry has grown dramatically over time. The early 2000s seems to be the time when a sizable number of people eventually included on the lists started making up a significant portion of the workforce in the industry. This growth in size can be attributed in large part to sociopolitical liberalization that allowed for the emergence of a new generation of movie workers and eventually 'New Koran Cinema' (Paquet, 2010). This justifies not extending the data series too far back in history. The industry was in a very different shape in the earlier periods than it is now.

Table 2: Benchmark Difference-in-Difference Estimates

	(1)	(2)	(3)
	Baseline	Benchmark	Non-Actor
Blacklisted × PostTreatment	−0.029***	−0.035***	−0.053***
	(0.004)	(0.004)	(0.005)
Number of Observations	1216866	1216866	934276
R^2	0.506	0.589	0.598
AIC	1539299.1	1316011.6	1096137.7
BIC	2913748.1	2690484.6	2126854.0
Log.Lik.	−655224.570	−543578.783	−460329.853
Unit Fixed	✓	✓	✓
Year Fixed	✓	✓	✓
Role Fixed	✓	✓	✓
Control		✓	✓

* p < 0.1, ** p < 0.05, *** p < 0.01 OLS estimates with standard errors clustered over unit. Only the key results are presented to save space. All models include unit, and year fixed effects. Models 2 and 3 also include role fixed effects. See Appendix Table B3 for full results.

From the early-2010s and on, however, the absolute size of the (eventually) blacklisted who were employed in movie projects stayed stagnant despite the continued rapid growth of the overall industry (left panel in 2). This means from the early 2010s the relative size of the blacklisted as a workforce in the industry shrank (right panel in 2), lending a cursory support to the first hypothesis.

4.2 Benchmark Difference-in-Difference Estimates

The second column in Table 2 presents the benchmark DiD estimates (Equation (1)). The negative and significant coefficient of *Blacklisted* × *PostTreatment* is consistent with the first hypothesis (H_1) that the blacklisted, on average, secured fewer jobs than those not blacklisted did after the treatment year, 2012. In simple terms, the result suggests that

getting blacklisted leads to on average about 3.5 % reduction in movie jobs.[7] The first column in Table 2 suggests that the simple 'baseline' estimates, where neither the control variable nor clustering standard errors is applied, are nearly identical to the benchmark. That is, the estimated effect of Blacklist does not seem to be driven simply by the benchmark model specification. The third column reports a similar result using the same model specification as the benchmark but a different sample that excludes actors. The size of the coefficient of *Blacklisted* × *PostTreatment* in Model 3 is much larger (-0.053) and more significant (t= -11.59) than that in Model 2 (β = -0.035, t= -9.01). More importantly, but not surprisingly, the contrast is even more pronounced when compared to the effect observed using the sample limited to the actors (β = -0.022, t= -4.38; See Appendix Table B3 for full results). The result is in line with the second hypothesis (H_2): Movie workers not publicly visible suffer more from Blacklist than the well-known. The covertness of the operations might be more amenable, and self-censorship might be more likely at work when the movie workers are behind the camera, not before it.

4.3 The Event Study Approach

$$ln(movies_t + 1) = \beta_0 + \lambda Blacklisted_{it} \times \Sigma yearDummy_t + \mathbf{X} + unit_{it} + role_{it} + \epsilon_{it}, \quad (2)$$

Following the standard practice in the literature (e.g., Roth et al., 2022), an event study analysis equivalent to the benchmark DiD model is conducted. An event study illuminates the dynamic nature of the treatment effect over time. Equation (2) specifies the model in a simple format. The coefficients for the interactions between the blacklisted and the year dummy variables (λ) are of particular interest here. For the result to lend further

[7] The relatively small size of the effect seems to be driven by the fact that the number of movie jobs is generally quite small in data. The median of the dependent variable ($ln(movies + 1)$) is simply zero and the mean is 0.24. The log re-scaling compounds this problem further, thereby systematically underreporting the size of the treatment effect (Chen and Roth, 2022, 4). The 'true' size of the effect, in other words, might be greater.

credence to the benchmark DiD estimates, these coefficients should be generally insignificant before the treatment timing (year 2012) and turns significant and negative afterwards.

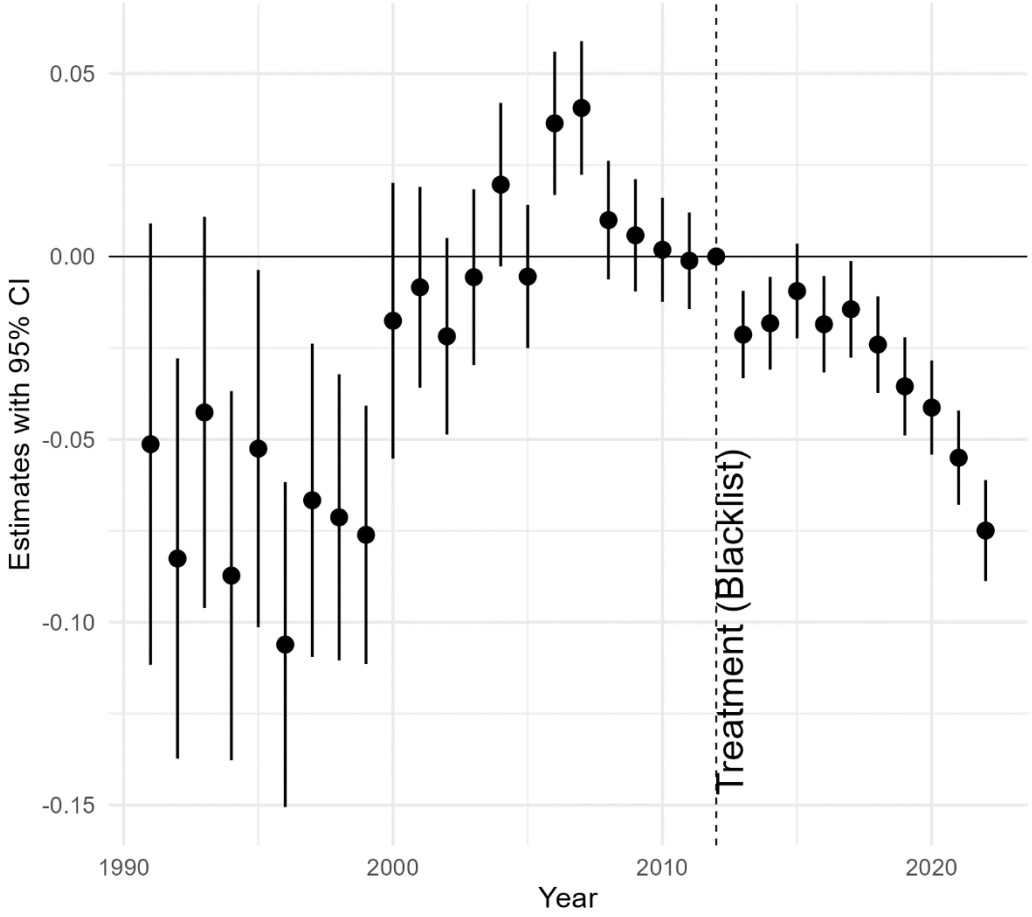

Figure 3: Event study (benchmark estimates). The vertical dashed line indicates the timing of the treatment, the right hand-side of which indicates the post-treatment period.

Figure 3 illustrates the event study estimates (Equation (2)), comparable to the benchmark (Model 2 of Table 2). In particular, the point estimates represent λ in Equation (2) and the band, the 95% confidence intervals. Any strong pre-treatment trend is not observed. The extremely wide confidence intervals before 2000 reflect incomparably small size of the industry (see Figure 2). The effect is statistically indistinguishable from zero for the decade leading to the treatment (i.e., from 2000 to 2011), only with the two 'odd'

exceptions in 2006 and 2007. In particular, during the four years immediately before the treatment, the period most important to ward off the concerns about pre-trend, the coefficients are reliably insignificant. This kind of visual examination, however, is hardly a sufficient test regarding the parallel treatment assumption (PTA), a critical condition to meet for inferences based on DiD to be valid. For this reason, I provide much more detailed and rigorous sensitivity tests in the Online Appendix A, where I address potential concerns about PTA in four different ways.

The figure indicates that from the treatment year, the effect remains consistently negative and significant, lending support to the H_1. Notable is the persistent downward trend of the treatment effect over time. In 2020, about seven years after the treatment year, the negative effect of Blacklist on movie workers' careers is about twice the size of the benchmark ATT. Two explanations, not mutually exclusive, are plausible. First, some of the treatment effects might take time to be observable. Since the observation of 'unemployment' of a movie worker is only visible once a movie is released while excluding the person from the project can happen much earlier, it is possible that a certain amount of time passes until the effect of blacklisting is observed, depending largely on the duration of the movie production. The second is cumulative effects. Since a career in the movie industry is heavily affected by the career trajectory (i.e., 'filmography'), expulsion from a movie project or failing to secure a professional development grant can have a cumulative, snowballing effect over time. This effect should be much stronger for early career workers than their senior counterparts. As seen in Appendix Figure B2, for instance, a disproportionate amount of the blacklisted early-career movie workers could secure little to know jobs (i.e., concentration on the left-hand side of Panel A) not only compared to their unlisted peers but also compared to their blacklisted senior counterparts (Panel B).

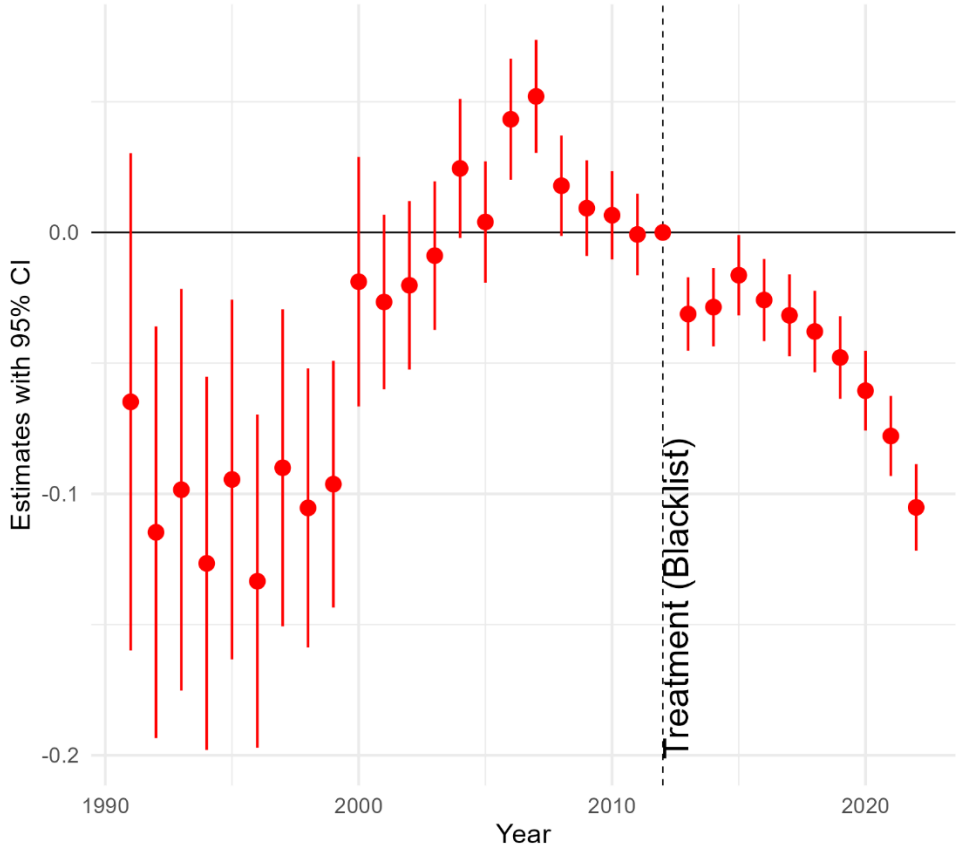

Figure 4: Event Study (Non-actor Sample). The vertical dashed line indicates the timing of the treatment, the right hand-side of which indicates the post-treatment period.

A similar story can be told about Figure 4 which presents the event study estimates equivalent to the benchmark non-actor estimates (Model 3 of Table 2). Here the model specification is exactly identical to Equation (2) but the sample is limited to the non-actor movie workers. Consistent with the visibility hypothesis (H_2), the treatment effect of Blacklist is clearer for non-actor movie workers than for actors. The effect is much more significant as the 95% confidence intervals stay much more consistently below the zero line. The size of the effect is also greater than that using the entire sample, as the negative career effect in 2020, for example, is estimated to be about 6%.

4.4 Robustness Checks

4.4.1 Sensitivity Analysis

Table 3: Sensitivity Analysis

	(1)	(2)	(3)	(4)
	Binary DV	Logit	Post-2000	double-SE
Blacklisted × PostTreatment	−0.016***		−0.038***	−0.035***
	(0.003)		(0.004)	(0.008)
Not Blacklisted, PostTreatment		-0.936***		
		(0.020)		
Blacklisted, Post Treatment		-1.911***		
		(0.029)		
Number of Observations	1216866	1138929	1176219	1216866
R^2	0.535		0.596	0.589
AIC	471344.5	216950.0	1273809.4	1316011.6
BIC	1845817.5	1512748.8	2652550.4	2690.484
Log.Lik.	−121245.268	0.000	−522631.721	−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. Only the key results are presented to save space. All models include unit, year, and role fixed effects. See Appendix Table B4 for full results.

Table 3 presents the results of four different robustness checks for the significant negative effect of Blacklists. The first column (Model (1)) suggests that using a binary outcome variable does not alter the benchmark estimates. Because many, though not all, movie workers simply cannot work on multiple movie projects simultaneously, it is possible that the outcome of interest here is often dichotomous—employed or not. And the dependent variable, the logarithm of the number of movies, might not capture this binary outcome effectively. The column thus reports OLS DiD estimates where the dependent

variable is coded as binary, as often used in the literature (Li et al., 2022): One if there was any movie job in a given year for the worker and zero otherwise. The result is not qualitatively different from the benchmark estimates. It also suggests that the benchmark effect is not driven by some of the extraordinary cases where certain movie workers have a very large number of jobs in a year.

Second, as a comparison, a two-way fixed effect logit model could be estimated for the binary outcome model. However, because the data contains disproportionately many units of 'perfect prediction'—either all ones or zeros after the treatment—the model does not converge and the standard errors are not obtained. As a limited alternative, a logit model containing only unit and role-fixed effects are estimated to gauge the before- and after-treatment marginal effect in terms of the possibility of a movie worker obtaining a movie job. This effect may not be interpreted as ATT, but a result in line with the benchmark would still increase our confidence in the benchmark estimates. The logit estimates are reported in Model 2 in Table 3 where cubic polynominal approximation is also controlled for (Carter and Signorino, 2010). Consistent with the benchmark, the size of negative career effect on the blacklisted was about twice that for those who were never blacklisted (see Appendix Figure A1).

Third, as shown in Figure 2, the pre-2000 period of the Korean movie industry were much smaller than what it has become, which raises the possibility that how movie workers secure their jobs could have evolved significantly over time. To ensure that the sample retains unit homogeneity, it is worth checking if the benchmark estimates remain intact when the observations of earlier periods are excluded from the sample. Model (3) of Table 3 confirms that using only the observations of the year 2000 and onward yields estimates nearly identical to the benchmark. Likewise, removing the years that might have been affected by the evolution of movie outlets (e.g., Netflix) does not produce results meaningfully different from the benchmark (see Appendix Table B1).

Finally, Model (4) reports the result when the standard errors are clustered over both units and years. Inferences about treatment effects can be sensitive to the structure of standard error clustering (Abadie et al., 2022). The result suggests that this concern is unwarranted as the estimates remain robust to double-clustering.

In the benchmark model, the timing of the treatment is universal, 2012. Some, though certainly not most, authorities and industry participants could not have learnt of who the 'anti-government' movie workers were until the lists were fully completed, however. It is worth checking if these potential 'late learners' pose a problem to the present analysis. Since the data does not have direct information on this possibility, one plausible way to navigate it is to make a rather extreme assumption that the treatment effect takes hold strictly after each list was entirely completed. A result nonetheless consistent with the benchmark DiD estimates should ward off the concerns about the different treatment timings. In this 'staggered' treatment setting, until blacklisted, those who will eventually be on the list (but not yet) are considered control observations, and the effect of being blacklisted is simply assumed to be similar across movie workers from 2012 over time.

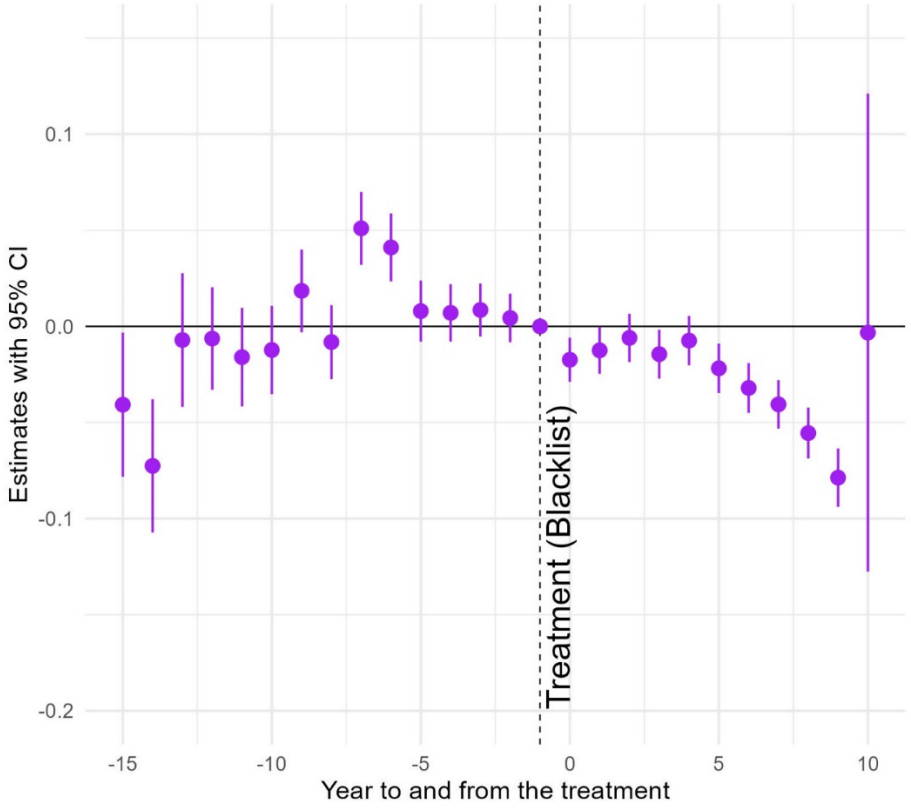

Figure 5: Event Study with staggered treatment assignment, using Sun and Abraham (2021). The vertical dashed line indicates the timing of the treatment, the right hand-side of which indicates

the post-treatment period. 'Zoomed in' for the period between t_{-15} and t_{+10} for a better visual identification of the treatment effect.

Figure 5 report the result of implementing the model that Sun and Abraham (2021) suggest as an optimal way to address the concerns about the staggered treatment design. It indicates that when the staggered treatment timing is introduced, the significance of the treatment effect becomes somewhat weaker, but remains largely consistent with what the benchmark estimate suggests. In fact, the 'delayed' effect— the effect of Blacklist after several years where differences in the treatment timings would be eventually inconsequential—remains almost identical. This might not be surprising, after all, as the vast majority of the blacklisted were 'treated' in the second list (see the portion for GH in Figure 2). All in all, the benchmark estimates remain robust to various alternative empirical scenarios.

5 Conclusion

Bearing on the nexus of democratic backsliding and popular culture, the present paper highlights a specific empirical domain, the deleterious effect of Blacklist on movie workers' careers in South Korea. Due to self-censorship in the movie industry as well as direct government operations, the blacklisted ended up landing significantly fewer movie jobs than those not blacklisted did. This difference was much more pronounced for those movie workers less visible in public eyes.

With the unprecedented success of Korean movies and TV series such as the Parasite (2019) and the Squid Game (2021), the fact that the industry suffered from a massive government censorship regime could be unfathomable. This counterintuitiveness motivates this paper, which suggests that the commercial success was achieved *despite* Blacklist, much like in Hollywood following the Red Scare period (Joo, 2010). In fact, the international prestige the Parasite earned would not have been possible, had Blacklist still been at work (Park 2020). Negative consequences of democratic backsliding, the paper implies,

might not always manifest themselves as tangible crises and could be easily concealed under the benign banality.

It is important to note that the results presented here reveal the scope of the effect of Blacklist that is potentially much wider than any collection of individual testimonies might indicate. As the investigators of the operation who collected evidence and interviewed victims suggest (Blacklist Committee, 2019*a*, 119–120), many victims, particularly early-career workers, did not realize that they were affected by the list given that the application of the lists was concealed and disguised as 'regular' human resource practices. These victims might have presumed that their careers simply did not take off. The empirical strategy adopted in this paper, in other words, sheds light on some of the effects of Blacklist that otherwise would not have been observable.

The paper contributes to our understanding of the negative, but often neglected, effect of democratic backsliding in the areas of diagonal accountability. In addition to their well-known proclivities such as failing to provide public goods for society and catering exclusively to their cronies and narrow support bases, backsliding leaders also seek to actively control public narratives (Tansel, 2018). Public domains such as popular culture are tempting grounds for these attempts (Gorsuch, 1992). Because of the often-secret nature of the operations, however, researchers rarely have an opportunity for a comprehensive look at this important aspect of backsliding unless manifested in observable ways such as journalists getting killed, prisoned, or explicitly threatened (e.g., Riaz, 2021). The paper demonstrates that backsliding through infringement upon freedom of expression may happen much more covertly while its effect would be surprisingly far-reaching via the revived remnants of the old authoritarian regime. The literature is perhaps underestimating the scope and magnitude of democratic backsliding as well as those of its consequences.

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Data Availability

The replication materials can be found: https://ben-son.netlify.app/publication/son2024a/

Online Appendix

The Online Appendix can be found: https://ben-son.netlify.app/publication/son2024a/

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