Conservative Media, Inventor Mobility, and Corporate Innovation*

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December 1, 2023

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Keywords: media influence, political ideology, innovation, human capital mobility

JEL: D72, J24, L82, J61, O31

*We thank Xi Chen, Stefano Manfredonia, Saiying Deng, Casey Dougal, Ruben Enikolopov, Irena Hutton, Kose John, April Knill, Sung Kwan Lee, Lubomir Litov, Ronald Masulis, Sergey Mityakov, Lalita Naveen, Greg Nini, Christoph Schiller, Margarita Tsoutsoura, and Tony Via for their helpful comments. We also thank seminar participants at the Southern Finance Association Conference, Australian Finance and Banking Conference, Financial Management Association Annual Meeting, Annual Philly Five Research Conference, and seminar participants at Temple University, Florida State University, and Kent State University for their helpful comments. All remaining errors are ours.

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Abstract

We examine the impact of Sinclair Broadcast Group, the largest conservative media network in the US local TV markets, on corporate innovation following its staggered expansion across the country. We find a significant reduction in innovation output two to three years after Sinclair entry. As a larger proportion of inventors self-identify as left-leaning, we find that the effect runs through inventor mobility. Inventors leaving Sinclair-exposed firms are more innovative compared to those who stay or are newly hired. The effect of Sinclair on innovation is larger in states with fewer restrictions on employee mobility and in red states.

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1 Introduction

> *[I]nnovation has nothing to do with how many R&D dollars you have. When Apple came up with the Mac, IBM was spending at least 100 times more on R&D. It's not about money. It's about the people you have.*
>
> – *Steve Jobs*

The modern economy has become increasingly reliant on innovation, making human capital a core driver of economic growth. As Kerr and Nanda (2015) note, firms that are able to attract and retain innovative human capital are better equipped to develop and implement new ideas, which is essential for their success. Notably, scientists in the US, the main driver of patent inventions, are more politically liberal than the general population (Kaurov, Cologna, Tyson, and Oreskes, 2022). A Pew Research Center survey conducted in 2009 reveals that among those scientists surveyed, 55% identified as liberals, 32% as independents, whereas only 6% said they are conservatives.[1] Related, the Creative Class theory pioneered by post-industrial urban economists posits that the new drivers of economic development are creative professionals.[2] Social attributes such as tolerance and openness can attract innovative talents, hence increase a region's innovation output.[3] In this paper, we analyze innovation and inventor data to study whether the expansion of the largest conservative media group, Sinclair Broadcasting Group (thereafter, Sinclair) disrupts innovation activities via the inventor mobility channel and incur a negative impact on corporate innovation.

The staggered expansion of Sinclair in the US local TV markets provides a quasi-natural experiment that led to a conservative shift in local ideology distribution. The existing litera-

[1]See the details at `https://www.pewresearch.org/politics/2009/07/09/section-4-scientists-politics-and-religion/`. Similarly, Verdent Labs publishes the data on political stance by profession and shows that scientists and engineers are predominantly left-leaning. See the details at `https://verdantlabs.com/politics_of_professions/index.html`.

[2]See Florida (2002), Florida, Mellander, and Stolarick (2008), and Wedemeier (2015).

[3]For example, Derrien, Kecskés, and Nguyen (2022) demonstrate that firms in younger labor markets produce more innovation; Vakili and Zhang (2018) document that social liberalization policies increase state-level patenting, while the anti-liberalization policies reduce patenting.

ture has shown ample evidence that media exerts considerable influence in shaping people's political identities and transforming the social and political behaviors of the local area.[4] A change in individuals' ideology can occur directly, or indirectly via interactions with friends, family, colleagues, or local communities. According to Abrams and Hogg (1988), people are shaped by local culture and beliefs through everyday interactions, which reinforce their preferences for shared social norms. In the context of our study, upon a conservative shift in local ideology induced by Sinclair's entry, innovative talent may find it more desirable to move away. This decision could be due to the individuals' direct rejection of the conservative media messaging, or a response to more conservative local communities and corporate cultures influenced by slanted media.[5] Such human capital turnover can disrupt ongoing R&D projects and existing patent applications, which implies a decrease in innovation output.

We study the impact of Sinclair because local news is the most trustworthy news source for the US public, compared to other sources such as cable news, newspaper, radio, social media, or friends and family, according to the Pew Research Center.[6] Sinclair was founded in 1986 and went public in 1995, and is presently the largest owner/operator of local TV stations in the US, covering 39% of the US population. It has been known as right-leaning since its founding. Martin and McCrain (2019) show that Sinclair stations tend to have a right-ward bias in the ideological slant of coverage, and estimate that the magnitude of the right-ward shift is equivalent to one standard deviation of the ideology distribution. There is also rich anecdotal evidence corroborating their finding, indicating that Sinclair tilts against reporting on climate change, gun safety, and labor unions.[7] Importantly, unlike Fox TV stations, Sinclair expands through acquiring local TV stations without re-branding the channels with the Sinclair name. Hence, many viewers are not aware of the acquisition

[4] See DellaVigna and Kaplan (2007); Gerber, Karlan, and Bergan (2009); Chiang and Knight (2011); Kaviani, Li, Maleki, and Savor (2023).

[5] Similarly, Hilary and Hui (2009) find that when they switch jobs, CEOs tend to join firms with a similar local religious belief to that of their previous firms.

[6] See "Why Sinclair matters: Local news is Americans' No. 1 news source" at https://www.cnn.com/2018/04/02/politics/sinclair-trust-in-local-news/index.html.

[7] For example, see https://www.mediamatters.org/donald-trump/former-sinclair-tv-reporter-anything-went-against-anything-corporate-wanted-was-just

of their local TV stations by Sinclair.[8]

We argue that the entry of Sinclair is mostly exogenous to the innovation variable. First, Kaviani et al. (2023) study Sinclair's effect on corporate social responsibility and show that Sinclair's entry into a new TV market is only marginally associated with the market's population size, but unrelated to economic or ideological factors. Their findings also suggest that Sinclair's expansion is primarily motivated by economic considerations rather than the urban-rural divide in American politics.[9] The finding is consistent with DellaVigna and Kaplan (2007), who show that the expansion of the cable TV channel Fox News is largely idiosyncratic conditional on regional characteristics. Second, we start the analysis with a temporal dynamic test, measuring innovation by patent counts (quantity) and the average life-long citation per patent (quality).[10] Event year dummy variables before and after the Sinclair entry year are created as independent variables and we regress innovation output on these variables and other firm characteristics, controlling for firm fixed effects and state by year fixed effects. All the variables indicating years before the Sinclair entry have insignificant loading, suggesting there is no concern for reverse causality. The dynamic test also reveals that innovation output starts to decline around two to three years following Sinclair's entry.

We then run a baseline regression in a difference-in-differences (DiD) setting focusing on total innovation in the three-year window following Sinclair entry. We find that the number of patents drops by 13.1% and the life-long number of citations per patent drops by 14.5% in this window. The magnitude is comparable to the effect of other shocks on innovation found

[8]See the Guardian's quote from John Oliver "Sinclair maybe the most influential media company you never heard of" at https://www.theguardian.com/media/2017/aug/17/sinclair-news-media-fox-trump-white-house-circa-breitbart-news.

[9]They consider the following socioeconomic factors for Sinclair's entry and find no significant effects: unemployment rate, the percentage of votes to the Republican party in the last presidential election, the percentage of the population with a college or higher degree, the percentage of female population, population above 65 years old, the Hispanic population, and African American population.

[10]The life-long number of citation counts for each patent is the total number of citations the patent accumulates from the year of filing to the year of 2018, which is the last year of the sample period.

in the literature.[11] Further analysis shows that the reduction in innovation is not restricted to one particular subset of patents. Sinclair exposure reduces the number of both related and unrelated patents to a firm's core business. There is a significant decline in patent originality and generality. We also find that Sinclair's entry is associated with a reduction in the economic value of patents. These results depict a coherent set of interrelated outcomes: there is a downward and multi-dimensional shift in corporate innovation in the treated firms compared to the controls. To address recent critiques to two-way fixed effect DiD models (Goodman-Bacon, 2021; Baker, Larcker, and Wang, 2022), we re-estimate the baseline using methods suggested by Callaway and Sant'Anna (2021) and Sun and Abraham (2021). The results remain with the new methodologies. Finally, the finding survives other robustness analyses with alternative model specifications and sub-samples. We also conduct multiple placebo tests and confirm that random chance embedded in the data does not drive the results.

We then study the underlying channel of inventor mobility and investigate whether treated firms can replace departed talent. We find that the inventors that stayed with the treated firm or were hired after Sinclair's entry tend to be less productive than those that left. To corroborate the human capital mobility channel, we assess whether state-level employee mobility constraints moderate the effect of Sinclair on corporate innovation. If Sinclair affects innovation via the human capital mobility channel, constraints on inventor mobility such as the non-competition agreements would dampen the effect. We find that the effect of Sinclair on innovation is indeed larger in states that enforce non-competition laws less stringently (Garmaise, 2011).

Next, we examine if there are any differences in mobility patterns of leavers in treated firms compared to control firms. We define three types of migration outcomes: (1) Red state to Blue state, (2) Blue state to Red state, (3) Non-switching (Blue to Blue, or Red

[11] See Cornaggia, Mao, Tian, and Wolfe (2015) and Chava, Oettl, Subramanian, and Subramanian (2013) among others.

to Red). Inventor mobility patterns in control firms are used as the baseline to account for non-Sinclair factors on mobility, e.g., family or health reasons. Hence, the difference between the treated and control groups reveals the additional effect of Sinclair. We find a significant difference in the two groups in the category of "Red to Blue", but not in "Blue to Red". This suggests that when Sinclair enters a red state, inventors are more likely to move to an ideologically-opposite state, while the opposite is not true. This is consistent with Murphy and Shleifer (2004), who suggest that media influence is stronger when existing ideology is more aligned with the media's value. We also find some marginal supporting evidence that Sinclair has a larger impact on innovation in red states than in blue states.

In the last section, we explore alternative plausible channels, which are not mutually exclusive with the inventor mobility mechanism. Kashmiri and Mahajan (2017) find that CEOs' degree of political liberalism positively affects the firms' innovation propensity. Sinclair could reduce innovation output via influencing CEOs' ideology. We address this alternative hypothesis by investigating Sinclair's effect on innovation inputs and whether the effect is different conditional on different CEO ideological leaning. We find no supporting evidence. The second alternative hypothesis posits that the entry of Sinclair could cause firms to switch to trade secrets rather than patenting them in order to protect their invention. This could result in the observed decrease in patenting. Since the expected legal costs of using trading secrets are smaller in areas with strong trade secrets protection laws, we should expect a larger decline of patent filing following Sinclair entry in states with stronger protection for trade secrets. We find no evidence consistent with this prediction. Finally, Sinclair's entry may potentially decrease innovation by reducing *green* innovation as the local community gravitates towards a more conservative ideology. However, our analysis finds no discernible difference in green patents before and after Sinclair's entry for treated firms compared to controls.

This paper contributes to the literature in several ways. First, it adds to the literature on the important role partisan media plays in determining economic outcomes via shaping individuals' ideology and behavior.[12] We focus on one key economic outcome, innovation, and find that a shock to local political ideology can significantly impact a region's corporate innovation. More importantly, we provide robust evidence that such impact is driven by inventor mobility. Second, our evidence suggests that more innovative individuals are more likely to leave compared to their less innovative peers after a shift towards more conservatism in local ideology. Third, and in a broader context, our findings expand the literature on regional and spatial innovation (e.g., Davis and Dingel, 2019), emphasizing the unique role of innovative talent in driving economic growth. Finally, we expand the body of research on the drivers of regional innovation that include the role of skilled labor, knowledge spillovers, infrastructure, and intellectual property rights (Galasso and Schankerman, 2010; Agrawal, Cockburn, Galasso, and Oettl, 2014; Alcácer and Chung, 2014; Moretti and Wilson, 2014).

The rest of the paper is organized as follows. We discuss the expansion of Sinclair in local TV markets in Section 2. Section 3 describes the sample, variable construction, and summary statistics. Section 4 presents the main empirical results and conducts various robustness tests. Section 5 provides evidence on the human capital mobility channel. Section 6 explores alternative channels and finds no supporting evidence for these channels. Section 7 concludes.

2 Sinclair and Local Ideology

2.1 *The Expansion of Sinclair in Local TV Markets*

A local TV market is officially called a "Designated Market Area" (DMA), also referred to as a media market. DMAs are defined by Nielsen Company and determine the cost of advertising in a specific area. There are 210 DMAs covering the whole country and they

[12]See Martin and Yurukoglu (2017), Durante, Pinotti, and Tesei (2019), Enikolopov, Petrova, and Zhuravskaya (2011), Yanagizawa-Drott (2014), and Oberholzer-Gee and Waldfogel (2009) among others.

are usually defined based on metropolitan areas, with suburbs often being combined within. Viewers in the same DMA receive the same or similar media coverage. Typically, there are multiple DMAs in one state. As there are around 3,000 counties in the US, one DMA always includes multiple counties, and in some cases, can also span multiple states. Figures 1 (A) and 1 (B) demonstrate two such examples. In the former, we highlight counties across the Pennsylvania, New Jersey, Delaware tri-state region that are included in the Philadelphia DMA. In the latter, we repeat the same practice for counties located in Arkansas, Tennessee, and Mississippi, forming the Memphis DMA. The local TV market is regulated by the Federal Communications Commission (FCC). For a media firm to operate in a DMA, it needs to obtain licenses issued by the FCC. The local TV market essentially provides media content as free public goods serving the interests of the local community. Hence, the local TV station must meet the needs and interests of the community it serves. Since local stations charge no fee from their viewers and are supposed to focus more on local issues than cable TV channels, it is harder for viewers to detect their political leaning. According to Pew Research, local TV news has scored higher than cable news or social media in viewer trust scores and viewership rates.

Sinclair was started in 1971 when Julian Sinclair Smith bought the local TV station WBFF-TV in Baltimore, Maryland. In 1986, his four sons established the Sinclair Broadcast Group, Inc. after acquiring several other local stations in Baltimore, Pittsburgh, and Columbus. Sinclair's station portfolio expanded to 59 stations in 1995 and it went public in the same year. The rapid expansion is fueled through the regular purchase of stations, and also via a creative usage of local marketing agreements (LMAs), which are a type of contract where one firm agrees to operate a radio or television station owned by another company. This type of lease or time-buy for the operating license allows Sinclair to circumvent many regulations once imposed by the FCC with respect to the ownership of operating licenses aiming to facilitate competition and foster diversity in media.

By the end of our sample period, Sinclair remains the largest operator/owner of local TV stations and the biggest producer of local news in the US. The company reportedly produces 2,400 hours of local news every week. It airs original programming from its 193 TV channels in more than 90 DMAs across the country. Sinclair now covers more than 39% of all American households. While it owns/operates the largest number of TV stations in the US, most of its target users are unaware of its existence. Sinclair has attained this anonymity due to its unique expansion approach over the last few decades: acquiring and operating local news stations without re-branding them as parts of the Sinclair network.[13] Figure 2 depicts the expansion of Sinclair TV across US counties over time. This graph combines multiple counties that are part of a DMA. Darker colors indicate a higher number of TV stations in a DMA. The graph visualizes the expansion over time and space of Sinclair across America. It also demonstrates that when Sinclair enters a region, it rarely exits from it. Moreover, the expansion is not concentrated in any particular geographic area, but rather widespread.

Sinclair's programs are widely viewed as leaning toward conservatism. Martin and McCrain (2019) study broadcast transcripts and find that Sinclair stations tend to have a right-leaning slant and emphasize more on national news, which tends to be more politically charged. Sinclair often produces a centralized news segment or commentary and distributes it to stations across the US for broadcast. For example, one of such news commentaries is the so-called "must-runs", where local TV hosts from different stations across the states are expected to read and broadcast from the same transcript. Sinclair's political inclination has attracted much attention from other media in recent years. The New York Times portrays Sinclair as a "conservative giant" and claims that Sinclair uses its TV stations "to advance a mostly right-leaning agenda".

[13] For example, Sinclair operates three stations: Fox-8 (Fox-affiliated), ABC-23 (ABC-affiliated), and NBC-6 (NBC-Affiliated) in Pennsylvania's Johnstown & Altoona TV market.

2.2 *What Determines Sinclair's Entry into a DMA?*

Kaviani et al. (2023) use the expansion of Sinclair in local TV markets as a quasi-natural experiment to proxy an exogenous shift in the media environment to a more conservative side, and examine its effect on corporate social responsibility. They find that the entry of Sinclair is not related to a geographic region's economic conditions or ideological characteristics. Instead, a DMA's population is the only factor that is correlated with Sinclair's entry. This finding is consistent with the language on Sinclair's 10-K filings, that the decision to expand is mostly driven by a cost and benefit analysis of the firm. We find the same results when we conduct these tests.[14] Sinclair's preference for more populous counties is also important because it alleviates the concern that Sinclair may disproportionately target rural areas. Moreover, this finding rejects the notion that conservative media expansion in the US is driven by the rural-urban divide in the American politics. Other plausible correlates of the rural-urban divide such as counties' level of education, percentage of senior citizens, female population, African American population, or Hispanic population have no loading on Sinclair's entry decision. These findings also align with the conclusion of DellaVigna and Kaplan (2007), who find that Fox News's decision to expand is largely idiosyncratic, after taking into account the observable characteristics of the regions.

2.3 *The Effect of Sinclair on Local Political Ideology*

The premise of our main hypotheses is that Sinclair TV reshapes local political ideology, which affects corporate innovation via the inventor mobility channel. There is extensive literature on how media influences a region's beliefs and preferences, changes voter behavior, and affects corporate policies. For example, Kaviani et al. (2023) provides several pieces of direct evidence on how Sinclair TV shifts local political ideology in a DiD setting, which manifests in public opinions of the local community and outcomes of local political events.

[14] The results are not reported here as it contains the same findings by Kaviani et al. (2023).

First, they study how public opinions shift on a spectrum from liberal to conservative, using data from the Cooperative Congressional Election Study (CCES).[15] They particularly focus on two categories: the county residents' opinion on climate change and support for affirmative action and find that opposition to these subjects grows significantly following Sinclair exposure. Second, they study the effect of Sinclair's entry on county-level legislative elections. County-level governments are an important part of American politics and their election results can reflect the ideology shift of a local community (De Benedictis-Kessner and Warshaw, 2020). They measure the local election result by the percentage of Democratic candidates elected to these local legislative bodies, and find that this percentage is reduced by 3.5% upon Sinclair's entry. Lastly, they use the campaign contribution data from the Federal Election Commission (FEC) database to study whether local firms' political contributions change with Sinclair exposure. The conclusion is that the relative contribution to the Republican party by the local firms increases in Sinclair-exposed areas.

These findings are also consistent with the ideological shift in electoral outcomes as the result of exposure to Sinclair programming documented by Miho (2018). The paper shows that an extra year of coverage by Sinclair programming increases a county's presidential Republican vote share. Studies on other conservative media outlets also find that exposure to slanted media affects audiences' behavior. For example, DellaVigna and Kaplan (2007) analyze the entry of Fox News in cable markets and its impact on presidential voting and find that Republicans gained 0.4 to 0.7 percentage points in towns with Fox News availability. Martin and Yurukoglu (2017) estimate a model of voters who select into watching slanted news, and depict how their ideologies evolve as a result. Knill, Liu, and McConnell (2022) show that partisanship in television news coverage influences fundamental corporate decisions such as investment expenditure and financial leverage. This is due to the fact that the

[15] See `https://today.yougov.com/`. CCES data is compiled by YouGov, which is an international research data and analytics group. The CCES is a 50,000+ person national stratified survey conducted around November of each year, and designed to be representative of all national adults in the US. It asks a wide range of questions on political and social issues, which broadly fall into eight categories: abortion, environment, guns, illegal immigrants, military, affirmative action, gay marriage, and government spending.

Republican slant in TV programming sways managers by depicting an optimistic outlook of the macroeconomic conditions, when the White House is controlled by a Republican president.

3 Sample, Variables, and Summary Statistics

We retrieve the patents and citations data from the US Patent and Trademark Office (USPTO) bulk data files for the period of 1996 to 2018.[16] We then link the Patent data and Compustat using a matching file compiled by Kogan, Papanikolaou, Seru, and Stoffman (2017).[17] Since we only observe patents that are granted eventually, patents applied in the last several years of our sample may not be granted. Similarly, we only observe citations to patents up to 2018, however, patents can be cited over a long period. As such, there are truncation problems for both patent counts and citation counts. To address these issues, we adjust the patent counts and citation counts based on the empirical distributions of application-grant lag and citation lag, respectively. For patent counts, we multiply the patent counts with the "weight factors" during the last six sample years following Fang, Tian, and Tice (2014). For citation counts, we move the adjustment factors created by the NBER patent data project forward by 12 years since our sample is extended by twelve years from 2006.[18] We drop the last two years of patent data (2017-2018) to further mitigate the truncation problem, since most patents are granted within two years (Hall, Jaffe, and Trajtenberg, 2001). Innovation output is measured by the number of patents filed, and the number of citations per patent each year.

Using a variety of resources, we compile data on the expansion of Sinclair into local TV

[16] Available at `https://www.patentsview.org/`.

[17] Available at `https://iu.app.box.com/v/patents`.

[18] For example, in the original NBER data, for a patent granted in the year 1998 and having a "chemical" classification, the adjustment factor for its citation counts is 1.9238. Hence, for a patent granted in the year 2010 (=1998+12) and having a "chemical" classification, the adjustment factor for its citation counts is also 1.9238.

markets. These resources include Sinclair corporation's 10-K filings, its websites, the FCC, and the Key Development database provided by the Capital IQ. Moreover, we manually complement this dataset by obtaining information from individual TV stations that are owned or operated by Sinclair. Our final sample consists of 68,176 firm-year observations of 7,338 unique firms in the period of 1996-2016. The summary statistics of the innovation variables and firm characteristics are presented in Table 1. The average number of patents is 3.76 with a median of zero. Around 24% of the sample firm-year observations have at least one patent. The average citation is 6.75 with a median of zero. About 17% of the observations are exposed to Sinclair TV.[19] The average firm in the sample has $220 million of assets and has 10 years of coverage by Compustat. Its profitability ROA is 2% with a leverage of 26% and a market-to-book ratio of 2.28. The value of R&D expenses on average is 7% of total assets, and 26% of total assets are fixed assets measured by plant, property, and equipment (PPE).

4 The Effect of Sinclair on Innovation

In this section, we test empirically the conjecture that Sinclair exposure reduces corporate innovation. First, we start from an agnostic approach and estimate a temporal dynamic model to gain insights on how many years it takes for Sinclair exposure to have an impact on local firms' innovation. Another purpose of the test is to detect if there is any difference in the pre-treatment trend between treated and control firms, which is an important validation test to rule out reverse causality for the main DiD analysis that follows. Second, we conduct a baseline test investigating the relationship between Sinclair exposure and innovation in the DiD setting, using both quantity and quality measures of innovation. Third, we study the impact of Sinclair on alternative measures of innovation, various scopes of patent portfolios,

[19] Because Sinclair rarely leaves a TV market once it enters, only around 0.92% of the firm-year observations correspond to the case of no Sinclair influence due to Sinclair exit. All results remain the same if we drop those observations.

and the economic value of patents. Lastly, we conduct an extensive set of robustness checks to validate the baseline results.

4.1 *The Temporal Dynamic Test*

Kaviani et al. (2023) find that corporate social responsibility ratings of locally-headquartered firms start declining in the third year following Sinclair's entry. For our study, there is no theoretical guidance on the time length it might take to observe an impact on corporate innovation from the exposure to Sinclair. Thus we start the empirical analysis by conducting a dynamic test following Cornaggia et al. (2015). The main assumption of a DiD design is that innovation growth should have followed a parallel trend between treated and control firms in the absence of Sinclair exposure. The dynamic test can also verify this assumption. We regress the innovation variables on a series of time-indicator variables representing the years before and after Sinclair's entry with the balanced sample of a seven-year window around Sinclair's entry, as shown below:

$$\begin{aligned} Innovation_{i,t} =& \alpha_i + \alpha_{s,t} + \beta_1 Before^3_{i,t} + \beta_2 Before^2_{i,t} + +\beta_3 Before^1_{i,t} + \\ & \beta_4 After^1_{i,t} + \beta_5 After^2_{i,t} + \beta_6 After^3_{i,t} + \gamma\prime X_{i,t} + \varepsilon_{i,t}, \end{aligned} \tag{1}$$

where i indexes firm and t indexes time. $Innovation_{i,t}$ is measured by the natural logarithm of one plus the total number of patents $Ln(1 + Pat)$ or by the natural logarithm of one plus the average life-long number of citations per patent for patents that firm i files in year t $Ln(1+Cite)$.[20] $Ln(1+Pat)$ captures the quantity of the innovation output and $Ln(1+Cite)$ measures the quality of those patents.

$Before^3_{i,t}$ is a dummy variable that equals one if a firm-year observation is three years

[20]The life-long number of citations for each patent is calculated as the total number of citations from the year the patent is filed to the year of 2016, which is the last year of the sample period. We use the life-long number of citations rather than the first year's citation for each patent as it is a more accurate measure for innovation quality.

before Sinclair's entry and zero otherwise, and similarly for $Before^2_{i,t}$ and $Before^1_{i,t}$. $After^1_{i,t}$, $After^2_{i,t}$, and $After^3_{i,t}$ are dummy variables that equal one if a firm-year observation is one, two, and three years, respectively, after Sinclair's entry and zero otherwise. The event year dummy is omitted from the regression as as it serves as the baseline innovation. Firms located in DMAs without any Sinclair's entry are also included as the baseline. α_i represents firm fixed effects, thus within-firm variations in patenting activities due to Sinclair exposure will drive the coefficient estimation. The state-by-year fixed effects, $\alpha_{s,t+k}$, control for any time-variant unobserved state effects. For example, state-level legislation can vary over time and affect local firms' patenting activities. Possible effects from variations in state-level electoral outcomes is also effectively captured by $\alpha_{s,t+k}$. Hence, unless stated otherwise, we include firm and state-by-year fixed effects in all regression models throughout the paper. We cluster the standard errors at the firm level, as the innovation variables are likely to be correlated within a firm over time. $X_{i,t}$ is a vector of control variables, including size, ROA, R&D expenses, capital expenditure, leverage, the market-to-book ratio, institutional ownership, firm age, asset tangibility, Herfindahl Hirschman Index of annual sales in the firms' affiliated industry, using 4-digit SIC codes (*Industry HHI*), *Industry HHI*^{2}, and the severity of financial constraints measured by the KZ index, which is constructed following Kaplan and Zingales (1997).

Table 2 present the results of this temporal dynamic test. The estimation results on control variables are largely consistent with the previous literature. Patent counts are positively related to firm size, firm age, profitability, R&D expenses, the market to book ratio, and negatively related to leverage, institutional ownership, industry competitiveness, and the severity of financial constraints measured by the KZ index. If innovation growth has similar pre-trends between treated and control groups, then the coefficients on $Before^3_{i,t}$, $Before^2_{i,t}$ and $Before^1_{i,t}$ would be small in magnitude and statistically insignificant. We find that the coefficient estimates on $Before^3_{i,t}$, $Before^2_{i,t}$, and $Before^1_{i,t}$ are all statistically insignificant for patent counts in Column (1) and citation counts in Column (2), validating the parallel trend condition of the DiD design. This indicates that our findings are unlikely to be driven by reverse causality.

In contrast, the coefficient estimates on $After^2_{i,t}$ and $After^3_{i,t}$ are negative and statistically significant in both columns (p-value $< 5\%$). Column (1) suggests that patent counts drop by 18.1% two years after Sinclair entry and 26.6% three years after. The magnitude of effect grows larger from $After^2_{i,t}$ to $After^3_{i,t}$, consistent with our prior. The magnitude of effect on innovation is also comparable to other shocks documented in the innovation literature. For example, Chava et al. (2013) show that intrastate bank deregulation has a negative effect while interstate bank deregulation has a positive impact on innovation by young and private firms. The order of magnitude by intrastate bank deregulation on the number of patents (citations) is -26.6% (-38.1%), and by interstate bank deregulation on the number of patents (citations) is 16% (15%) in a year. The quality of these patents has also declined as shown by the coefficient of $After^2_{i,t}$ to $After^3_{i,t}$ in Column (2). Our takeaway from this finding is that it takes Sinclair two to three years to have a real and large impact on local firms' innovation output.

4.2 *The Baseline*

We run a baseline analysis to test the effect of Sinclair exposure on innovation controlling for various drivers of innovation in a difference-in-differences (DiD) setting. The regression equation is specified as follows,

$$Innovation_{i,t+1\rightarrow t+3} = \alpha_i + \alpha_{s,t+k} + \beta\ Sinclair\ TV_{i,t} + \gamma\prime X_{i,t} + \varepsilon_{i,t+k} \tag{2}$$

where i, s, t index firms, states, and years, respectively. From the finding of the dynamic test, we are interested in innovation output starting from two years following Sinclair entry. Since annual innovation measures could be noisy as innovation activities do not occur across time evenly, we compute a three-year moving sum of $Innovation_{i,t+1\rightarrow t+3}$, which we aggregate patent counts or citations counts per patent over a rolling three-year window. Separately, we also use $Innovation_{i,t+2}$ and $Innovation_{i,t+3}$ as dependent variables. $Sinclair\ TV_{i,t}$ is a dummy variable indicating whether the firm i's headquarter is located in a DMA with at least one Sinclair TV station in year t. The rest of the model specification including the fixed effects, control variables, and standard error clustering is the same as the dynamic test specified in Equation 1. α_i and $\alpha_{s,t}$ represent firm fixed effects and the state-by-year fixed effects. Standard errors are clustered at the firm level.

The results of the regression are presented in Table 3. Panel A and Panel B show the results when innovation is measured by the number of patents and the average number of citations per patent, respectively. Columns (1), (3), and (5) show univariate results, while Columns (2), (4), and (6) control for firm characteristics. When the dependent variable is the three-year moving-sum patent or citation counts, we exclude firm-year observations near Sinclair's entry year if the moving sum period encompasses the event year, so that $Ln(1 + Pat)_{t+1\rightarrow t+3}$ or $Ln(1 + Cite)_{t+1\rightarrow t+3}$ observations can be clearly defined as before or after Sinclair exposure. That explains why in both panels, we see fewer observations in the regression in Columns (1) and (2), compared to Columns (3) to (6).

In Panel A, the first two columns show the aggregate effect of Sinclair on innovation from year $t+1$ to $t+3$ and the coefficients on $Sinclair_t$ are negative and highly significant (p-value less than 1%). Taking the coefficient with the smaller magnitude from Column (2), we find a 13.1% decline in patent counts. The magnitude is similar to what we find in the dynamic test. In Columns (3) to (6), we continue to find a statistically significant yearly decline in patent counts two and three years after Sinclair exposure, although the magnitude of the coefficients of $Sinclair_t$ is slightly reduced. This is consistent with the fact that Column (2) measures the aggregate reduction in patent counts in the three-year window, while Columns (3) to (6) measure the yearly reduction.

In Panel B, when innovation quality is measured by the average life-long number of citations per patent, we observe a similar pattern. The first two columns show the negative effect of Sinclair on average life-long citations of each patent filed in a three-year window with Sinclair exposure. The last four columns demonstrate that the effect also exists when we study the citations for patents filed in year $t + 2$ and $t + 3$. Combining both panels in Table 3, we conclude that Sinclair has a negative impact on corporate innovation, in terms of both quantity and quality.

4.3 *Robustness Tests*

4.3.1 *Effects on Scopes of Patents*

The baseline results in Table 3 establish an economically large and statistically significant negative effect on patent counts and life-long citation counts per patent. The advantage of using patents as a measure of innovation output is the granularity of the data that allows us to assess the scope of patent portfolios beyond the quantity and the scientific quality (the number of citations received by a patent). In this section, we investigate how Sinclair exposure affects various scopes of the patent portfolios, including relatedness to the core business, originality, generality, and their economic value.

The USPTO adopts a patent classification system that assigns patents to a three-digit technology class based on technology categorization. We use a concordance table developed by Hsu, Tian, and Xu (2014) that connects the USPTO technology classes to two-digit SIC codes, mapping patents in each technology class to one or multiple two-digit SIC codes. The number of related patents in a firm is calculated by multiplying patent counts with the corresponding mapping weights of its main two-digit SIC industry. The number of unrelated patents is the total number of patents minus the number of related patents. Patents that cite a wider array of technology classes of patents have greater originality according to the innovation literature. Similarly, patents that are cited by a wider array of technology classes of patents are viewed as having greater generality. Hence, we define a patent's originality score as one minus the Herfindahl index of the three-digit technology class distribution of all the patents it cites, and define a patent's generality score as one minus the Herfindahl index of the three-digit technology class distribution of all the patents that cite it (Hall et al., 2001). We repeat the baseline regression in Equation 2, replacing the dependent variable with the natural logarithm of one plus the number of related patents, the natural logarithm of one plus the number of unrelated patents, the average originality score of all patents within a firm, and the average generality score of all patents by a firm in a given year. The results are shown in Table 4.

We investigate the effect of Sinclair exposure on each dependent variable in the three-year window $(t+1, t+3)$ and in year $t+2$ and $t+3$ separately. The results suggest a consistently negative impact of Sinclair on all four innovation scope variables. Out of the 12 specifications, 9 coefficient estimates are statistically significant and negative. The remaining three produce negative coefficients, albeit insignificant. Results from Columns (1) to (6) suggest that Sinclair exposure reduces patents both related and unrelated to firms' core business. Columns (7) to (12) indicate that both originality and generality of the patent portfolios decline after Sinclair exposure.

While patent citations reflect the scientific value of innovation, they do not provide much information on the economic value a patent may generate for a firm. We follow Kogan et al. (2017) and compute the economic value of each patent based on stock market reactions to announcements of patent grants. Specifically, we multiply the firm's abnormal stock return when patent grants are announced by its market capitalization one day before the announcement. Using stock returns to capture patent value is advantageous as asset prices are forward-looking and provide an estimate for the dollar value of the patent based on ex-ante information.

To shed light on how the exposure to Sinclair programming affects the economic value of patents, we obtain patent economic value data from Noah Stoffman's website.[21] Since many firms hold more than one patent, we compute the average value of all patents applied in the moving three-year window *Average Patent Value*$_{i,t+1\to t+3}$.[22] The regression results are reported in Column (13) of Table 4. We find that the average economic value of patents declines significantly after Sinclair's entry, suggesting that Sinclair TV exposure leads to a reduced economic value of patents, in addition to declines in the quantity and scientific impact of patents.

4.3.2 *Timing and Heterogeneity in Treatments*

There has been growing concern over the use of two-way fixed effects (TWFE) in DiD models with unit and time fixed effects in the recent literature. The problem with the TWFE approach lies in its comparison of treated units to those who were treated previously and continue to experience the treatment's effects. This renders the previously treated units invalid as controls since their observations contain a part of the treatment effect itself. Depending on the heterogeneity of the treatment effect and its post-treatment dynamics, TWFE may result in either a positive or negative bias in the estimated coefficient (see Goodman-Bacon, 2021). To circumvent this issue, we follow Baker et al. (2022) and report estimates using methods suggested by Callaway and Sant'Anna (2021) (the CS method) and Sun and Abraham (2021) (the SA method). Each of these methods modifies the effective comparison units to avoid comparing treatment units to inappropriate controls.

Specifically, Callaway and Sant'Anna (2021) developed an estimator that constructs group-time treatment effects based on 2×2 comparisons of before versus after treatment

[21]We thank Noah Stoffman for making the patent data publicly available at https://iu.app.box.com/v/patents.

[22]We exclude the firm-year observations near Sinclair's entry year when the moving average is computed over a period that encompasses the event year, so that all *Average Patent Value*$_{i,t+1\to t+3}$ observations can be clearly defined as before or after Sinclair exposure.

and control versus treated. These group-time fixed effects serve as the building blocks for estimating the overall average treatment effect and how it emerges over the event window. The aggregates are weighted averages of the group-time fixed effects, with appropriate weights described by the CS method. Given that our main variable of interests is $Ln(1+Pat)_{t+1\rightarrow t+3}$, the CS method conveniently provides the codes to calculate the aggregate treatment effect of any time window. We apply their method with $Ln(1 + Pat)_{t+1\rightarrow t+3}$ and Column (1) of Table 5 reports the results with this estimator. The estimated treatment effects are negative and highly statistically significant. Its magnitude is higher than the TWFE estimator in the baseline result, likely due to a cleaner control set. Given the consistency of the findings, we infer that the estimated coefficients are not affected by concerns around treatment timing.

To ensure the robustness of our findings, we also implement the estimators proposed by Sun and Abraham (2021). This method is designed to estimate dynamic treatment effects with heterogeneous treatment effects and treatment timing. Sun and Abraham's approach involves constructing a weighted average of treatment effects, where the weights are dependent on the estimated heterogeneity and timing of the treatment effects. This method produces consistent estimates of the average treatment effect, even in the presence of dynamic treatment effects. Additionally, this method can be used to estimate the distribution of treatment effects and identify subgroups with larger or smaller treatment effects. In comparison to CS method, the SA method does not require the specification of group-time fixed effects, making it easier to implement. The SA method however does not generate the *aggregate* treatment effect in a time window but the *average* per-period effect. We follow their method and calculate the average annual effect in the window of $(t+1, t+3)$. The results are reported in Column (2) of Table 5. We continue to find a negative and significant treatment effect. Note that the coefficient magnitude with the SA method in Column (2) is much smaller than that with the CS method in Column (1), because the CS method estimates the aggregate treatment effect while the SA method produces the average treatment effect, both in the window of years $(t + 1, t + 3)$. Without any randomness in the data, we should expect the magnitude of the coefficient in Column (1) to be three times of that in Column (2). The number of observations in Column (2) is larger than that in Column (1), as Callaway and Sant'Anna (2021) require at least one pre-treatment observation, while Sun and Abraham (2021) does not. Both exercises confirm that our result is robust to different assumptions on treatment timing and heterogeneous effects of treatment.

4.3.3 *Alternative Specifications and Sub-Samples*

We conduct several other robustness tests to the baseline specification as follows and present the results in Table 6. The dependent variable is $Ln(1 + Pat)_{t+1\rightarrow t+3}$. Panel A shows the results with alternative specifications and Panel B shows the results with alternative sub-samples. All original control variables of the baseline are included, and their regression estimates are not shown for the sake of brevity.

(i) Controlling for Local Demographics: We add the county-level demographic controls from the US Census Bureau to the baseline, which includes the logarithm of total population, the male-to-female ratio, and the percentage of the population with college or higher degrees. Column (1) of Panel A shows that the negative impact of Sinclair on innovation remains.

(ii) Industry-Level Dynamics and DMA-Level Characteristics: We add the industry-by-year fixed effects or the DMA fixed effects to the baseline. Adding industry-by-year fixed effects controls for time-varying industry effects and adding DMA fixed effects further controls for TV market characteristics. Columns (2) and (3) of Panel A show the baseline result remains with these two specifications, respectively.

(iii) Alternative Ways of Clustering Standard Errors: Instead of clustering standard errors at the firm level, we use alternative ways to cluster the standard errors. Columns (4), (5), and (6) show the results when we cluster by DMA, industry, and county, respectively. The baseline result remains in all three specifications.

(iv) Nonlinear Model: We re-estimate the baseline model using the Poisson model instead of the linear regression model using the number of patents as the dependent variable in Column (7) of Panel A.[23] We continue to find a negative effect of Sinclair on innovation.

(v) Firms with Mostly Local vs. Non-local Activities: In the baseline analysis, we study the effect of Sinclair presence on corporate innovation using firm headquarters to match innovation activities to Sinclair influence. We rely on past literature showing that firm headquarters play a vital role in both stock performances and corporate policies (Pirinsky and Wang, 2006 and John, Knyazeva, and Knyazeva, 2011 among others), and assume that a majority of innovation activities take place near corporate headquarters. On the other hand, due to the lack of data on the location of inventors and corporate research labs, we cannot directly verify that for each firm, human capital involved in corporate innovations are influenced by Sinclair messaging. To address this question, we study whether Sinclair has different effects on firms with activities centered around the headquarter and firms involved in broader geographic locations. Garcia and Norli (2012) provide the share of activities of each Compustat firm in various states of the US based on textual analysis of 10-K filings. We define a firm as having mostly local activities if more than 50% of its activities are concentrated in its headquartered state. Firms with more than 50% of activities in non-headquartered states are categorized as the ones with mostly non-local options. We run the baseline specification separately for the two sub-samples. The results are shown in Column

[23]The number of observations in the regression is much smaller compared to the baseline regression (24,178 obs vs. 50,466 obs), because the Poisson regression drops all observations when the dependent variable is zero, that is, firms with zero patents.

(1) and (2) of Panel B in Table 6. The results indicate a larger effect of Sinclair on innovation of firms with mostly local activities compared to those with mostly non-local activities (0.132 vs. 0.091). More importantly, both effects are statistically significant, suggesting that the results are not driven by only a subset of the firms.

(vi) Are the Results driven by the Most Innovative Firms? We drop the most innovative firm-year observations measured by the number of patents, to rule out the possibility that the results are driven by extreme values in the sample. We exclude either the top 5% or the top 10% of the observations with the highest number of patents. Columns (3) and (4) of Panel B show that the effect of Sinclair on the number of patents remains.

(vii) Alternative Choice of Sample Firms: We drop firms with zero patents and restrict the sample to only firms with at least one patent during our sample period, to address the concern that non-innovative firms may drive the results. Column (5) of Panel B shows a significant and negative effect of Sinclair on innovation. Furthermore, when we aggregate the total number of patents by state, California is the most innovative state. We drop all firms headquartered in California to rule out the possibility that the result is only driven by one state. Column (6) of Panel B shows that the baseline results remain.

(viii) Regions with Different Existing Political Ideologies: Since Sinclair affects innovation via influencing the local political ideology, the effect could vary in regions with different existing ideologies. To address the concern that the sub-sample of highly liberal or conservative regions drives the results, we drop the top 5% of the most liberal and/or conservative states measured by the latest presidential votes. In Columns (7) to (9) of Panel B, we continue to find a negative effect of Sinclair on innovation.

(ix) The Expansion of Fox News: In the early part of our sample period, the media environment is also affected by the expansion of Fox News in the US. Schroeder and Stone (2015) document that most of Fox News expansion occurred during the period of 1996-2008. To address the concern that the decline in corporate innovation documented in this paper is driven by the expansion of Fox News, rather than Sinclair, we re-run the baseline excluding the observations from 1996-2008. Column (10) of Panel B shows that the result on the negative impact of Sinclair still holds.

4.3.4 *Placebo Test*

We address the concern that our results may be influenced by unobserved shocks or variables that are omitted from the baseline model but also correlated with the timing of Sinclair's entry. The staggered timing of Sinclair's entry into various DMAs can mitigate this concern as there is a minute chance that other shocks with similar effects take place in a parallel geographic and temporal fashion. Nevertheless, to rule out this possibility, we conduct a formal placebo test.

First, we obtain the empirical distribution of Sinclair's entry years to different DMAs. Next, we randomly re-assign entry years across sample DMAs in 1,000 rounds and re-estimate our baseline specification in each round. As a result, we obtain 1,000 samples with pseudo Sinclair's entry years. If our results are driven by Sinclair's entry and not random chance, then the random re-assignment of the entry dates should largely weaken the results. However, if shocks or variables other than Sinclair's entry are the true drivers of the observed outcome, then our results should remain in the placebo samples because the true drivers of corporate innovation should still reside in the testing framework. We plot the distribution histogram for the t-statistics of $Sinclair_t$ when innovation is measured by $Ln(1 + Pat)_{t+1 \to t+3}$ and $Ln(1 + Cite)_{t+1 \to t+3}$, in Figure 3(A) and (B) respectively. The X-axis shows the bins of

t-values, and the Y-axis represents the frequency corresponding to each bin. The red dashed line indicates the t-value of our baseline regression models (t-value is -2.68 in 3(A) and -2.27 in 3(B)), which clearly lies in the lower 1% or 2.5% of the placebo distribution. This exercise hence rules out the possibility that the baseline finding is driven by comfounding unobserved shocks.

5 Inventor Mobility

Motivated by the essential role human capital plays in producing innovations, and by the above finding that Sinclair causes a large drop in innovation output, we conjecture that the conservative media shock to the local community creates disruptions in innovation activities via the inventor mobility channel. More specifically, the creative class theory suggests that social attributes such as tolerance and openness can attract innovative talents and hence increase a region's innovation output. It deems the geographical distribution of the creative class as one of the major drivers of the US cities' development in the post-industrial time. US cities that succeed in attracting and maintaining the creative class thrive while those that fail to do so stagnate (Florida, 2002; Florida et al., 2008; Vakili and Zhang, 2018; Derrien et al., 2022). For example, in 2004 the governor of Michigan initiated a state-wide program, referred to as the "Cool Cities" to attract "urban pioneers and young knowledge workers who are a driving force for economic development and growth". Similar policies were implemented in Denver to make the city a "creative center".[24] Furthermore, a majority of scientists and engineers in the US are left-leaning evidenced by Pew research surveys and academic studies. We hence conjecture that innovative individuals could choose to move away once Sinclair enters a region as the local ideology shifts to more conservatism. This decision is likely the response to a more right-leaning local community or corporate culture. Such human capital turnover can be detrimental to ongoing research projects and existing patent applications,

[24] See https://www.americansforthearts.org/sites/default/files/MCC_initial2_88765_7.pdf. For more information, see http://www.creativeclass.com/.

especially when the more innovative ones dislocate, causing declines in innovation output.

5.1 *Sinclair Exposure and Inventor Turnover*

The PatentsView data from USPTO provides information on inventors and firms (assignees) of each patent.[25] It allows us to explore the inventor turnover patterns. To test the hypothesis, we restrict observations to a seven-year window including three years before and three years after Sinclair's entry for all treated firms. For each treated firm, we then carefully select a control firm, which is from a neighboring county so that they share similar economic and demographic conditions, is in the same two-digit SIC coded industry, and has no exposure to Sinclair throughout the seven-year period. In the case of multiple matches, we select the one with the closest asset value. The Sinclair's entry year for the treated firm serves as the pseudo-event year for the control firm. Following prior literature (Gu, Mao, and Tian, 2017; Brav, Jiang, Ma, and Tian, 2018), we define an inventor as a "leaver" if he/she filed at least one patent before Sinclair's entry but no patents after Sinclair, and filed at least one patent in a different firm after Sinclair. An inventor is defined as a "stayer" if he/she filed at least one patent in the firm both before and after the event year, but filed no patents for other firms within the event window. An inventor is defined as a "new hire" if he/she filed no patent in the firm but filed at least one patent in a different firm before the event year, and filed at least one patent in the firm after Sinclair. We require both the treated firm and the control firm to have at least one leaver, one stayer, or one new hire. We end up with 221 pairs of treated and control firms.

We next investigate whether more innovative inventors tend to leave treated firms. While any inventor turnover can be disruptive to a firm's innovation activities, losing the more innovative talent exacerbates the negative consequences. If firms can easily replace lost talent with new ones, the effect of inventor mobility on innovation can be limited. In this

[25] See https://www.patentsview.org/.

section, we compare innovativeness between leavers and stayers, and between leavers and new hires for both treated and control firms. It is important, however, to compare their innovativeness in the pre-Sinclair period. This is because Sinclair's entry itself could change an inventor's innovativeness. For example, if we compare patent counts between leavers and stayers in the post-Sinclair period and find that leavers produce more patents, it does not necessarily indicate that leavers are more innovative than stayers. Instead, stayers and leavers can be equally innovative to begin with, however, stayers may have become less innovative under the influence of Sinclair. Empirically we measure the innovativeness of inventors by their total patent counts during the three-year window before the event year. We average innovativeness across inventors within the same category (leavers, stayers, or new hires) at the same firm, then we average across firms in the treated or the control group before the event year. We report the results testing these conjectures in Table 7.

In Panel A, we present the difference in the pre-Sinclair innovativeness between leavers and stayers for both treated and control firms. In Column (1), we report the average difference in innovativeness measured by the number of patents between leavers and stayers in the three-year period before the event year for treated firms. In Column (2), we repeat the analysis for control firms. Lastly, we show the DiD results between treated firms and control firms in Column (3). The p-values of the two-tailed t-statistics testing the null hypothesis of zero mean difference are shown in parentheses. We find that the number of patents produced by leavers is significantly greater than that produced by stayers in treated firms (p-value $< 1\%$), whereas no significant difference is observed in control firms (p-value $= 0.58$). The DiD estimator is positive and significant at the 10% level (p-value $= 0.069$). This finding indicates that treated firms have trouble retaining talents post-Sinclair compared to control firms: they lost more innovative talent, and retained those who were less innovative.

In Panel B, we report the result of the DiD test comparing the difference in the pre-Sinclair innovativeness between leavers and new hires for both treated and control firms. We find that the number of patents by leavers is significantly higher than that produced by new hires in treated firms (p-value < 0.0001). However, the difference in innovativeness between leavers and new hires is statistically insignificant in control firms (p-value $= 0.393$). The DiD estimator is positive and significant at the 1% level (p-value $= 0.0003$). The results suggest that treated firms have trouble recruiting new talents: they lost highly innovative inventors and hired those who are less innovative following Sinclair entry, leading to reduced innovation output. The results from these two panels provide direct evidence for the mobility channel, and are consistent with the creative class theory.

To address the possibility that our results could just be a fluke of data due to the relatively smaller sample size of 221 pairs of firms, we also conduct placebo tests by randomizing the treated status between each pair. We repeat the analysis 1,000 times and calculate the DiD t-statistics for the innovativeness difference between leavers and stayers, leavers and new hires, and then compare the differences between the treated and the control. We plot the histogram of the pseudo t-statistics and use the red dashed line to represent the true DiD t-statistics. The results are presented in Figure 4. In the two sub-figures, the true t-statistics is significant at either the 2.5% level or the 0.1% level. In summary, upon Sinclair's entry, highly innovative inventors tend to leave the firms, and new hires and those who remained in the firms on average are less innovative than the leavers.

5.2 *Inventor Mobility: Evidence from Employee Mobility Constraints*

We have shown so far that Sinclair negatively impacts a region's innovation output via the human capital mobility channel: innovative human capital in treated firms are more likely to leave upon Sinclair's entry compared to the control firms. Here, we provide additional evidence on this channel by studying mobility constraints. Specifically, if inventor mobility is at play, then regional employee mobility constraints should moderate the Sinclair effect. In particular, we expect Sinclair to have a greater impact on innovation in states with fewer constraints on human capital mobility. To examine this conjecture, we focus on the enforceability of non-competition agreements between employers and employees. Non-competition agreements are contracts that restrict workers from joining or forming a rival company. They represent one of the most important mechanisms binding employees to a firm. Meanwhile, there is substantial variation across states in the US on how binding these laws are. Malsberger (2004) provides a detailed survey describing non-competition law in the 50 US states and the District of Columbia. Subsequently, Garmaise (2011) considers 12 questions analyzed by Malsberger (2004) and assigns one point to each jurisdiction for each question if the jurisdiction's enforcement of that dimension of the law exceeds a given threshold.[26] We use this index for each state where our sample firms are headquartered in. A dummy variable *Low Enforceability* is created, which is equal to one if the index is below the sample median, and zero otherwise.

We then interact the dummy variable *Low Enforceability* with the Sinclair TV dummy variable and add that to the baseline regression. If human capital mobility indeed is the driving force through which local ideology affects innovation, then we should expect a negative coefficient on the interaction term, i.e., the larger impact of Sinclair in states with less binding non-competition agreements, as inventors are less restricted from moving. The regression results are presented in Table 8. The coefficient estimates of *Low Enforceability*×*Sinclair TV* are negative across all six columns, and statistically significant in four out of six columns, with the remaining two columns showing also negative coefficients. This finding provides key direct evidence for the human capital mobility channel and helps rule out alternative hypothesis not built on human capital mobility. For example, one might argue that conservative media per se might have a direct impact on the overall productivity of human capital. But if this is the case, the impact of Sinclair should not be related to the enforceability of non-competition laws. We explore other alternative hypothesis in detail in Section 6.

[26]Total scores on the enforcement degree, therefore, should range from 0 to 12, and Garmaise (2011) scales it by 12 to generate a score from 0 to 1.

5.3 *Inventor Migration Pattern in Red and Blue States*

In this section, we examine inventor migration patterns along the line of existing ideology by studying whether such effects could be different in conservative vs. liberal (red vs. blue) states. We define red and blue states as those where the Republican or Democratic Party has won the most recent presidential election, respectively. If an inventor's relocation is indeed motivated by the local ideology shift, we should observe different migration patterns for red vs. blue states. Murphy and Shleifer (2004) show that media influence is stronger when the media and the audience share similar values. Populations in conservative areas might be more amenable to receiving and internalizing Sinclair's messaging, while those in liberal areas may simply choose to switch off their TVs. Kaviani et al. (2023) also find that the effect of Sinclair on corporate social responsibility is larger in red states than blue states. If innovators relocate in response to Sinclair-induced ideological shifts, then the migration of innovative talent should be more visible in regions that are affected more.

We study such patterns in treated firms and control firms. Inventor relocation in control firms captures the normal human capital mobility due to reasons orthogonal to ideology shift, e.g., family consideration, weather preference, job opportunities, etc. The difference in migration patterns between treated and control firms reflects the net impact of Sinclair in inventor mobility. We identify three categories of migration: (1) Red to Blue: an inventor from a red state before Sinclair exposure moved to a blue state afterwards; (2) Blue to Red: an inventor from a blue state before Sinclair exposure moved to a red state afterwards; (3) Non-switching: an inventor moved from one state to another state with the same ideology leaning (Blue to Blue or Red to Red).

We report the percentage of inventor dislocations of each type in Panel A of Table 9 for both treated and control firms. There are several interesting findings. First, investors move to an ideologically similar region most of the time. Non-switching relocations make up 86.22% of the migration in treated firms and 94.9% in control firms. This is consistent with the prior literature (e.g., Hilary and Hui, 2009), people tend to adhere to their previous social and cultural environment as they switch jobs. Second, the difference in non-switching frequency between the treated and the control firms is -8.68%, suggesting inventors working in firms with Sinclair exposure are more likely to go to ideologically opposite states. Further investigation on the frequency of "Red to Blue" and "Blue to Red" suggests that this is due to the fact that inventors living in red states with Sinclair exposure are much more likely to pack up and move to a blue state. This is evidenced by the frequency of 8.68% in treated firms vs. 0.89% in control firms for the "Red to Blue" type. In contrast, there is not a significant difference in the frequency of the "Blue to Red" type between the treated and control (5.11% vs. 4.21 %). Lastly, the Chi-Square test rejects the null hypothesis that the proportions of migration pattern are the same in treated firms and control firms.

In summary, controlling for the non-Sinclair related factors of mobility, the marginal effect of Sinclair exposure on inventor mobility is larger in red states than in blue states. Inventors in red states are more likely to move to blue states upon Sinclair entry compared to inventors in the control firms, but there is no difference for inventors from blues states between the treated and the control.

5.4 *Effects on Innovation and Inventor Turnover in Red vs. Blue States*

Next, we investigate directly whether the effect of Sinclair on innovation output is larger in red states. We add an interaction term of a *Red State* dummy variable and the *Sinclair TV* dummy variable in the baseline regression Equation 2. The *Red State* dummy takes the value of one if a firm's headquarter is in a red state, and zero otherwise. The regression results are in Panel B of Table 9. Columns (1) and (2) lag the Sinclair TV dummy variable by two and three years respectively. In Column (1), the coefficient on the interaction term is negative and significant, indicating a larger effect of Sinclair exposure in red states. In Column (2), it is negative albeit insignificant. We interpret this as some weak evidence that the effect of Sinclair on innovation is larger in red states, consistent with the inventor mobility pattern. This finding also echoes the finding in Kaviani et al. (2023) that Sinclair's effect on corporate social responsibility is stronger in red states.

We then revisit inventor mobility data in red and blue states separately to illustrate the channel at work driving the larger effects of Sinclair in red states, and find consistent evidence. We re-run the DiD tests in Table 7 for red and blue states, and the results are reported in Table 10. In Panel A, we compare innovativeness between leavers and stayers in treated firms relative to control firms in the two sub-samples of inventors in red and blue states. It shows a significant and positive DiD estimate in red states, but an insignificant one in blue states. This suggests that leavers are more productive than stayers in red states but not in blue states. In Panel B, we conduct the same analysis as in Panel A but compare the innovativeness between leavers and new hires. The DiD estimates are positive and significant in both sub-samples, although the magnitude is larger in red states. This table provides evidence that the larger effect of Sinclair on innovation in red states is driven by higher rates of talent departure in these states.

6 Alternative Channels of Effect

We explore alternative mechanisms that may explain the drop in innovation quantity and quality following Sinclair's entry found in the baseline results. In total, we investigate three alternative hypotheses based on: the channel of executive decisions on innovation, the effect of trade secrets on patent filing, and the possible impact of Sinclair on green patents. In the remaining part of the section, we conduct various tests to see if there is any supporting evidence in the data for these alternative channels.

Kashmiri and Mahajan (2017) show that firms have higher innovation propensity when the CEOs exhibit more political liberalism. Hence an alternative hypothesis is that Sinclair's presence reduces corporate innovation via exerting a conservative influence on management. In other words, the change in innovation occurs in a top-down direction rather than via the inventors in a bottom-up fashion. We formally test this hypothesis in two ways. First, if corporate innovation of local firms is affected by Sinclair via the management, it can manifest as reductions in innovation *inputs*. This is because one of the most important factors of innovation that executives can decide on is the allocation of resources to innovation activities. We repeat the baseline regression by changing the dependent variable to innovation inputs: R&D expenses scaled by total assets, and the annual asset growth rate. We include asset growth as an alternative measure of innovation inputs because it is common for firms to obtain innovation via acquisitions (see Cassiman and Veugelers, 2006; Bena and Li, 2014). Hence, the growth of total assets can reflect acquired innovation. The results are reported in Table 11. Columns (1) to (3) show the results with R&D expense, and Columns (4) to (6) present the results with asset growth. In all six regressions, none of the coefficients on the Sinclair TV dummy are statistically different from zero. This means that the negative effect of Sinclair appears to only exist on innovation *outputs*, but not innovation *inputs*, implying that the negative effect of Sinclair does not take place via executive decisions on cutting back innovation inputs, but is likely due to individual inventors' behavior.

Second, Murphy and Shleifer (2004) show that when values of the media and the receiving audience are more aligned, the effect of media influence is larger. This has been shown to be true in this context with the earlier finding that Sinclair indeed has a larger effect in red states. If Sinclair affects innovation via influencing CEOs, we should observe larger effects for firms with CEOs whose ideological leaning is similar to that of Sinclair's messaging. Babenko, Fedaseyeu, and Zhang (2020) show that CEOs' political leaning strongly drives firms' political leaning, hence we use firms' political contribution to proxy for the CEO's political ideology. Campaign contribution data is collected from the Federal Election Commission compiled by the Center for Responsive Politics.[27] The dummy variable *Republican CEO* takes the value of one if the firm contributes more dollar amount to the Republican party candidate than the Democratic party in the current election cycle, and zero otherwise. We repeat the baseline regression adding the *Republican CEO* dummy variable and the interaction term of *Republican CEO*×*Sinclair TV*. The results are presented in Table 12. The interaction term is insignificant across all eight columns when both innovation quantity and quality are studied. This suggests no difference in the Sinclair effect for firms with CEOs of different political stances.

The second alternative channel is based on the possibility that the decline in patenting upon Sinclair's entry might be attributed to firms' decision to shift from patenting to using trade secrets as an alternative way to protect proprietary technology knowledge. Consequently, the decline in patents following Sinclair's entry might be attributed to a change in the particular way to protect innovation rather than reduced innovation per se. Sinclair could potentially incentivize this shift by intensifying media coverage of local firm's operations, prompting firms to adopt a more secretive approach to safeguard their innovations. Given that Sinclair stations broadcast more national news at the price of a cutback in local news (Martin and McCrain, 2019), this is unlikely to be the case. Nevertheless, we test this alternative hypothesis by examining whether there is a cross-sectional difference in innovation response to Sinclair based on how well trade secrets are protected in the local economy.

Compared to filing patents, relying on trade secrets come with benefits and costs. The main benefit is that trade secrets does not require any formal filing with the patent office and no proprietary innovative information is disclosed. The downside however, is that such information is mainly protected via filing lawsuits when violations and offenses perpetrated by competitors are perceived by the firm. Whether the plaintiff would win the case crucially depends on the local legal system. Hence, a firm's choice to file patents or to keep trade

[27]The data is available at http://www.opensecrets.org. Contributions are measured as the total sum of contributions in a two-year period for each firm per election cycle, and we fill in the second year using the previous year's data. Missing values of the contribution data are replaced by zero.

secrets relies on the perceived strength of trade secrets protection in the local economy. Firms located in areas with stronger legal protection of trade secrets enjoy the same benefits of information secrecy but are burdened with much smaller expected legal costs. If firms find it necessary to adopt a more secretive approach to protect their inventions following Sinclair's entry, we should expect to observe a much larger reduction in patent counts in treated firms located in areas that offer stronger trade secret protection. In the extreme scenario where firms have zero protection for trade secrets, firms would still choose to file patents to protect their inventions even if Sinclair excessively covers them.

We use the states' adoption of the Uniform Trade Secrets Act (UTSA) to proxy for the level of protection for trade secrets. UTSA strengthens trade secrets in various ways, by codifying the states' existing common laws and standardizing how a trade secret is legally defined in the laws. Also, UTSA helps enumerate what activities can constitute illegal misappropriation and clarifies the rights and protections that are attributed to the victimized firms.[28] We add to the baseline model an interaction term between the UTSA adoption dummy and the Sinclair TV dummy. Based on the alternative hypothesis, a larger effect of Sinclair on treated firms located in states with USTA suggests that the coefficient on the interaction term should be negative. Note that since state-year interaction fixed effects are included in the specification, the stand-alone dummy variable of UTSA adoption is absorbed and not included in the model. The results are shown in the first three columns of Table 13. The first two columns show statistically insignificant coefficients for the interaction term, while in Column (3) the coefficient is significant. All three columns present coefficients with positive signs, which are the opposite to what the alternative hypothesis has predicted. This implies that the effect of Sinclair on treated firms' patent filing is weaker, not larger, in states with stronger trade secret protection. We conclude that it is unlikely that Sinclair's negative impact on patenting is due to firms switching to use trade secrets as the alternative way to protect their inventions.

[28] Guernsey, John, and Litov (2022) provide a detailed description of these laws and stress their importance for trade secrets protection.

Lastly, we evaluate the alternative hypothesis that Sinclair reduces total patent counts by negatively impacting the filing of green innovation. The urgency to resolve environmental and climate change issues has always been a controversial topic between the liberal and the conservative camps. If Sinclair exerts a conservative influence on the local ideology, it is possible that the observed reduction in total innovation output is driven primarily by the reduction in green innovation.[29] As with Cohen, Gurun, and Nguyen (2021), we classify green patents following the guidelines of the Organization for Economic Co-operation and Development. Green patents belong to various broad environmental technology categories including environmental management, water-related adaptation technologies, biodiversity protection and ecosystem health, energy generation, and waste-water treatment or waste management. We gather data on green patents and run the baseline regression with the number of green patents by local firms as dependent variables. The results are shown in Column (4) to (6) of Table 13. We find no reduction in green patents in the three years following Sinclair's entry. This contrasts with the observed reduction in total patents following Sinclair. This exercise rules out the alternative channel that the Sinclair effect on innovation is via the reduction of green patents.

7 Conclusion

In this paper, we use the expansion of the conservative media conglomerate Sinclair Broadcasting Group in local TV markets as an exogenous shock to the local media environment and study its impact on corporate innovation. We hypothesize that a shift to more conservatism on the ideology spectrum could disrupt innovation activities in treated firms via

[29]See "Sinclair trashed renewable energy and pushed fossil fuel propaganda" at https://www.mediamatters.org/sinclair-broadcast-group/february-sinclair-trashed-renewable-energy-and-pushed-fossil-fuel?utm_source=flipboard&utm_content=other. Also see: "She tried to report on climate change. Sinclair told Suri Crowe to be more 'Balanced'." at https://www.buzzfeednews.com/article/stevenperlberg/sinclair-climate-change, "At least one Sinclair station has been trying to cast doubt on climate science" at https://grist.org/briefly/at-least-one-sinclair-station-has-been-trying-to-cast-doubt-on-climate-science/.

the channel of human capital mobility and exert a negative effect on corporate innovation. This hypothesis builds on the survey evidence that a larger proportion of US scientists and engineers self-identify as left-leaning rather than right-leaning. Related, the creative class theory stresses the importance of the social context for innovation as a more socially liberal environment attracts young and innovative talents. Firms in Sinclair-exposed areas hence could lose more inventors compared to the control firms, and as a result their innovation output could suffer.

A temporal dynamic test first reveals that both patent counts and their life-long citations drop after two to three years following Sinclair's entry. The baseline results based on that time frame further confirm the findings. A further investigation shows that other aspects of innovation including counts of related patents, counts of unrelated patents, patent originality, generality, and their economic value are all negatively impacted in treated firms. A closer examination of inventor mobility patterns provides direct evidence supporting this hypothesis. Specifically, we observe that individuals who leave these firms tend to be more innovative than those who stay or are newly hired, a distinction not found in the control group. Additionally, in line with the inventor mobility channel, we find a larger impact of Sinclair in states with fewer constraints on employee mobility.

Along the lines of ideology differences, we find that the negative impact on innovation as well as inventor turnover is larger in red states than in blue states. This is consistent with Murphy and Shleifer (2004) and Alesina, Miano, and Stantcheva (2020), who argue that media influence is stronger among the parties who share similar value systems. We corroborate these finding by ruling out three alternative hypotheses about the channels of the Sinclair effect on innovation: via management, via a switch to protecting intellectual property with trade secrets, via a reduction in green patents. Technological innovation is vital for a country's economic growth (Solow, 1957 and Romer, 1986) and human capital is the key for innovation. Our paper contributes to the innovation literature by showing a crucial social factor critical for innovation output through its effect on human capital: political ideology. In this regard, our paper also sheds light on the real economic effect of conservative media in the corporate setting.

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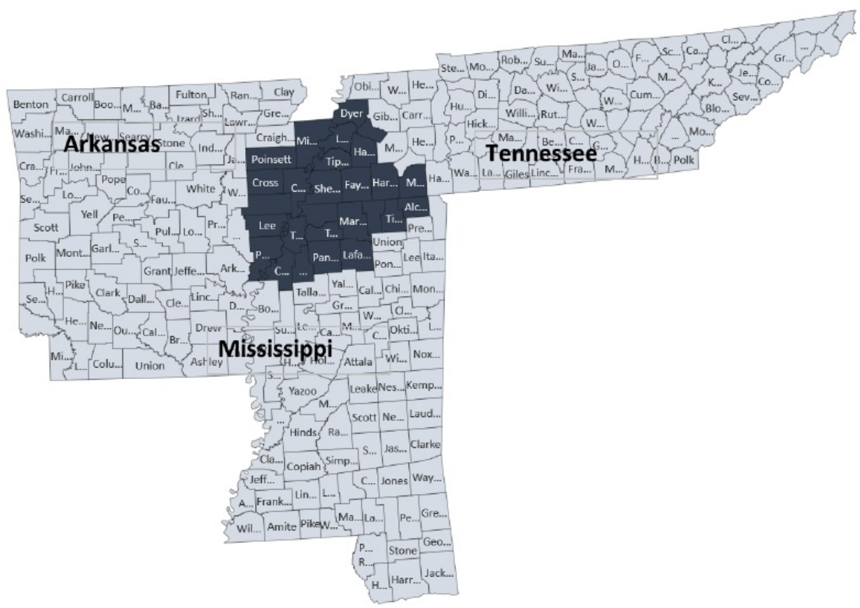
Figure 1: Examples of a DMA Spanning Multiple Counties

This figure demonstrates two examples of a single DMA spanning multiple counties. Figure 1(A) shows that counties from PA, NJ, and DE are within the Philadelphia DMA, and Figure 1(B) presents the Memphis DMA with counties from AR, TN, and MS.

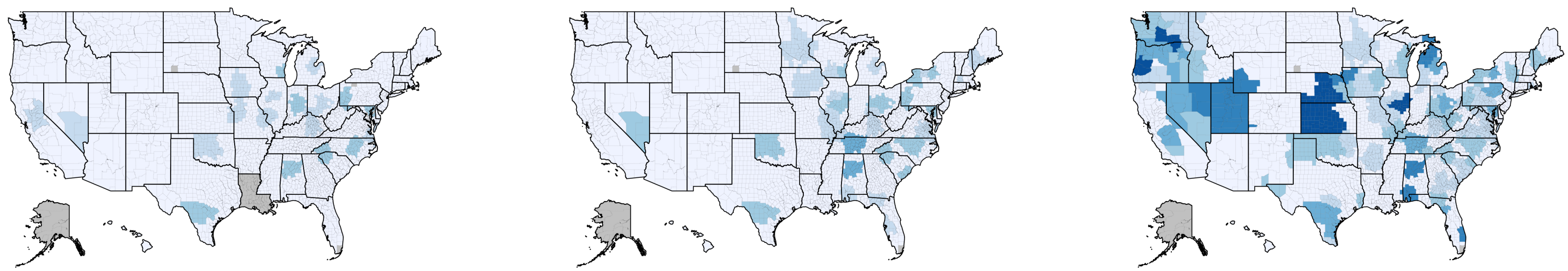
(A) The Philadelphia DMA



(B) The Memphis DMA

Figure 2: Expansion of Sinclair TV Stations

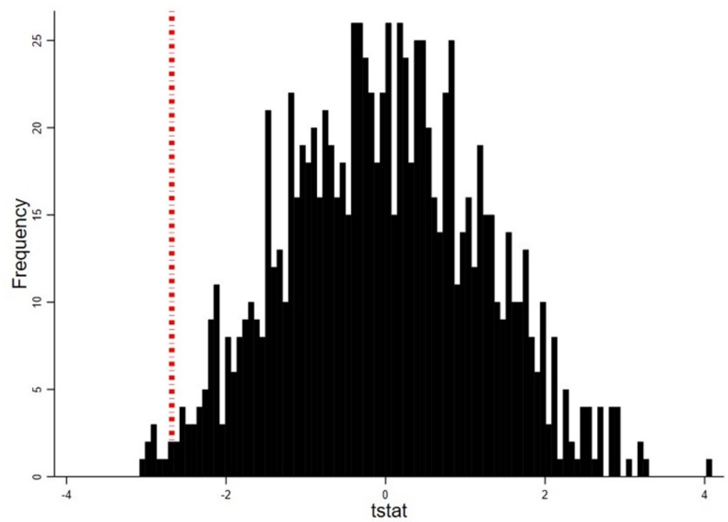
This figure depicts the expansion of Sinclair TV stations in the US over time. From 1996 to 2016, this figure provides a snapshot every ten years of the number of TV stations owned or operated by Sinclair Broadcasting Group. The figure is color-coded and darker colors indicate more stations.



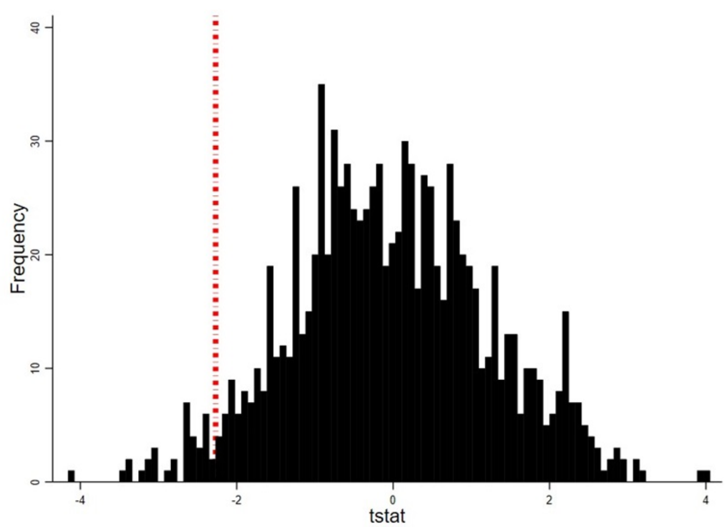
(a) 1996 (b) 2006 (c) 2016

Figure 3: Placebo Tests of the Baseline

This figure plots the distribution histogram for the t-statistics of $Sinclair_t$ in the regression Equation 2 when innovation is measured by $Ln(1+Pat)_{t+1\rightarrow t+3}$ and $Ln(1+Cite)_{t+1\rightarrow t+3}$, in Figure 3(A) and (B) respectively. $Ln(1+Pat)_{t+1\rightarrow t+3}$ is the natural logarithm of one plus the total number of patents filed during the three years of $t+1$, $t+2$, and $t+3$. $Ln(1+Cite)_{t+1\rightarrow t+3}$ is the natural logarithm of one plus the average number of citations per patent for patents filed during the years of $t+1$, $t+2$, and $t+3$. We use the original distribution of the Sinclair's entry years to randomly reassign the placebo entry years to different DMAs and re-estimate its effect on innovation 1,000 times. The vertical axis shows the frequency of the t-statistics. The red dashed line shows the t-statistics of $Sinclair_t$ in the true baseline regressions, which is -2.68 in 3(A) and -2.27 in 3(B). All specifications include firm fixed effects and the state-year interaction fixed effects. Standard errors are clustered at the firm level.



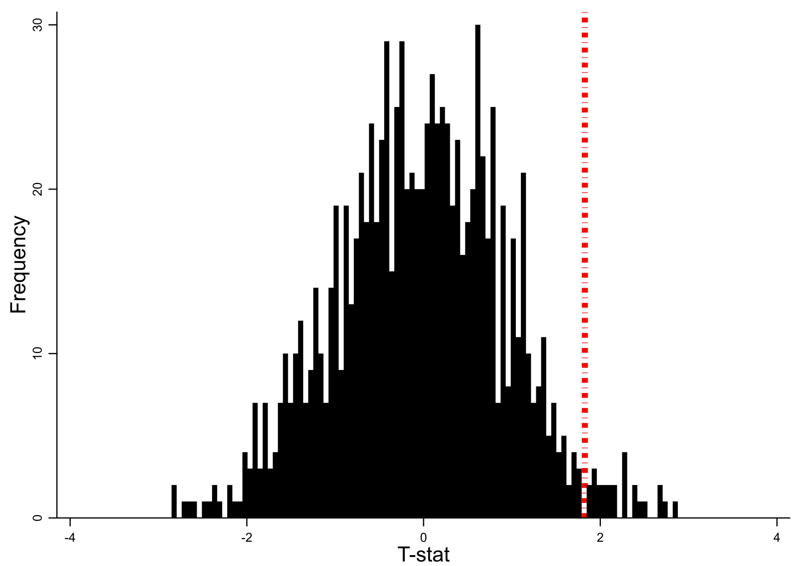
(A) Distribution of t-statistics of Sinclair TV on patent counts with randomization



(B) Distribution of t-statistics of Sinclair TV on citation counts per patent with randomization

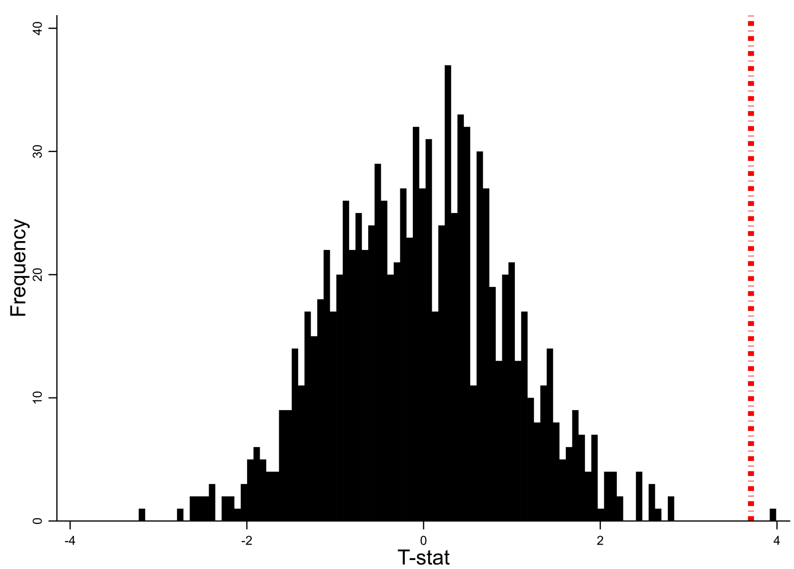
Figure 4: Placebo Tests for Human Capital Mobility

This figure plots the distribution histogram for the t-statistics of the difference in innovativeness before the Sinclair exposure between leavers and stayers for both treated and control firms in (A), and the difference in innovativeness between leavers and new hires in (B). We use the original treatment and control firms in Table 7, and randomly reassign the placebo treatment firms (and their matched firms become control firms) and re-estimate its effect on innovation 1,000 times. The red dashed line shows the t-statistics of the differences of the number of patents in the true sample, which is 1.83 in 4(A), and 3.71 in 4(B). In 4(A), 2.5% of the placebo t-statistics are greater than the true t-statistics; in 4(B), 0.1% of the placebo t-statistics are greater than the true t-statistics.



(A)

Distribution of t-Statistics of Sinclair TV on patent counts of leavers vs. stayers with randomization



(B)

Distribution of t-Statistics of Sinclair TV on patent counts of leavers vs new hires with randomization

Table 1: Summary Statistics

This table presents summary statistics for variables constructed based on the sample of Compustat US firms from 1996 to 2016 excluding utility and finance industries. The Compustat data is merged first with innovation data from the US Patent and Trademark Office (USPTO), then with the data on Sinclair TV stations' geographic distribution in local TV markets of the US by the headquarters of firms. Variables are defined in the Appendix.

Variable	N	Mean	S.D.	P25	Median	P75
Pat	68,176	3.76	12.65	0	0	1
Cite	68,176	6.75	23.43	0	0	0
Sinclair TV	68,176	0.17	0.37	0	0	0
Ln(Assets)	68,176	5.39	2.18	3.78	5.35	6.88
ROA	67,839	0.02	0.36	0.01	0.1	0.16
R&D	68,176	0.07	0.14	0	0	0.07
CAPEX	67,530	0.06	0.07	0.02	0.04	0.07
Leverage	67,894	0.26	0.32	0.02	0.18	0.37
Market-to-Book	60,727	2.28	2.99	1.1	1.52	2.4
Institutional holding	68,176	0.28	0.34	0	0.09	0.57
Ln(Firm Age)	68,176	2.33	0.83	1.79	2.48	3
PPE	68,050	0.26	0.23	0.08	0.18	0.37
Industry HHI	68,176	0.83	0.25	0.6	1	1
Industry HHI^2	68,176	0.75	0.35	0.36	1	1
KZ Index	59,226	–11.27	47.2	–6.36	–1.07	0.98

Table 2: Innovation Dynamics around Sinclair's Entry

This table reports the regression results that estimate the innovation dynamics surrounding the year Sinclair enters the county where the firm's headquarter is located. Innovation is measured by the number of patents in Column (1) and by the number of patent citations in Column (2). The dependent variables are measured in year t. Before^3 is a dummy that equals one if year t is three years before Sinclair's entry and zero otherwise. Before^2 is a dummy that equals one if year t is two years before Sinclair's entry and zero otherwise. Before^1 is a dummy that equals one if year t is one year before Sinclair's entry and zero otherwise. After^1, After^2, and After^3 are dummy variables that equal one if year t is one, two, and three years, respectively, after Sinclair's entry and zero otherwise. Variable definitions are in the Appendix. To study the trend of innovations, we focus on the sample of a seven-year window around Sinclair's entry. The current event year dummy ($t = 0$) is omitted as baseline. Firms located in DMAs that are never exposed to Sinclair's entry are also included as baseline. Both regressions include firm fixed effects and state×year fixed effects. Standard errors are clustered at the firm level. P-values are reported in parentheses. *, **, and *** indicate significance at the 0.1, 0.05, and 0.01 levels, respectively.

	$Ln(1+Pat)_t$ (1)	$Ln(1+Cite)_t$ (2)
Before^3	0.019	0.019
	(0.757)	(0.881)
Before^2	0.015	0.043
	(0.766)	(0.702)
Before^1	−0.010	−0.097
	(0.825)	(0.308)
After^1	−0.083	−0.146
	(0.123)	(0.213)
After^2	−0.181**	−0.368***
	(0.013)	(0.001)
After^3	−0.266**	−0.318**
	(0.027)	(0.018)
Ln(Assets)	0.078***	0.051***
	(0.000)	(0.000)
ROA	0.011	0.049**
	(0.429)	(0.049)
R&D	0.215***	0.305***
	(0.001)	(0.007)
CAPEX	0.038	−0.036
	(0.489)	(0.709)
Leverage	−0.068***	−0.131***
	(0.000)	(0.000)
Market-to-Book	0.008***	0.019***
	(0.000)	(0.000)
Institutional Holding	−0.089***	−0.216***
	(0.002)	(0.000)
Ln(Firm Age)	0.115***	0.053
	(0.000)	(0.166)
PPE	0.072	0.136*
	(0.101)	(0.057)
Industry HHI	−0.355*	0.116
	(0.051)	(0.620)
Industry HHI^2	0.251**	−0.076
	(0.047)	(0.653)
KZ Index	0.000	0.000*
	(0.141)	(0.064)
Firm FE	Yes	Yes
State×Year FE	Yes	Yes
Observations	50,427	50,427
Adjusted R^2	0.806	0.619

Table 3: The Baseline: Sinclair Exposure and Innovation Outputs

This table shows the effects of Sinclair exposure on firm patent counts in Panel A and citation counts per patent in Panel B using all Compustat US firms between 1996 and 2016 excluding utility and finance industries, merged with the Sinclair's entry data using firms' headquarter locations. In Panel A, the dependent variables are patent counts. $Ln(1+Pat)_{t+1\rightarrow t+3}$ is the natural logarithm of one plus the total number of patents filed during the three years of $t+1$, $t+2$, and $t+3$. $Ln(1+Pat)_{t+2}$ and $Ln(1+Pat)_{t+3}$ are the natural logarithm of one plus the total number of patents filed in year $t+2$ and $t+3$. In Panel B, the dependent variables are the average number of life-long citations per patent for a firm. $Ln(1+Cite)_{t+1\rightarrow t+3}$ is the natural logarithm of one plus the average number of citations per patent for patents filed during the years of $t+1$, $t+2$, and $t+3$. $Ln(1+Cite)_{t+2}$ and $Ln(1+Cite)_{t+3}$ are the natural logarithm of one plus the average number of citations per patent for patents filed in year $t+2$ and $t+3$. We exclude the firm-year observations near Sinclair's entry year when the three-year moving sum is computed over a period that encompasses the event year, so that all $Ln(1+Pat)_{t+1\rightarrow t+3}$ and $Ln(1+Cite)_{t+1\rightarrow t+3}$ observations can be clearly defined as before or after Sinclair exposure. Sinclair TV_t is a dummy variable that equals one if the headquarter of a firm is in a county with at least one Sinclair station in year t, and zero otherwise. Variable definitions are in the Appendix. All regressions include firm fixed effects and state×year fixed effects. Standard errors are clustered at the firm level. P-values are reported in parentheses. *, **, and *** indicate significance at the 0.1, 0.05, and 0.01 levels, respectively.

Panel A: Sinclair Exposure and the Number of Patents

	$\text{Ln(1+Pat)}_{t+1\rightarrow t+3}$		Ln(1+Pat)_{t+2}		Ln(1+Pat)_{t+3}	
	(1)	(2)	(3)	(4)	(5)	(6)
Sinclair TV_t	-0.135^{***}	-0.131^{***}	-0.114^{***}	-0.113^{**}	-0.098^{**}	-0.091^{*}
	(0.003)	(0.007)	(0.004)	(0.012)	(0.029)	(0.072)
Ln(Assets)		0.102^{***}		0.058^{***}		0.037^{***}
		(0.000)		(0.000)		(0.000)
ROA		0.046^{**}		0.013		0.031^{**}
		(0.015)		(0.345)		(0.031)
R&D		0.214^{***}		0.075		0.027
		(0.006)		(0.189)		(0.674)
CAPEX		−0.080		−0.003		0.008
		(0.199)		(0.955)		(0.883)
Leverage		-0.097^{***}		-0.077^{***}		-0.078^{***}
		(0.000)		(0.000)		(0.000)
Market-to-Book		0.011^{***}		0.010^{***}		0.011^{***}
		(0.000)		(0.000)		(0.000)
Institutional Holding		−0.034		-0.069^{**}		-0.057^{*}
		(0.317)		(0.014)		(0.068)
Ln(Firm Age)		−0.011		0.091^{***}		0.100^{***}
		(0.712)		(0.000)		(0.000)
PPE		0.200^{***}		0.153^{***}		0.197^{***}
		(0.000)		(0.000)		(0.000)
Industry HHI		−0.128		−0.148		0.042
		(0.495)		(0.371)		(0.812)
Industry HHI^2		0.104		0.130		0.003
		(0.432)		(0.261)		(0.982)
KZ Index		0.000		0.000^{**}		0.000^{**}
		(0.226)		(0.023)		(0.014)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
State×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	58,555	50,466	68,176	58,323	59,551	51,353
Adjusted R^2	0.906	0.911	0.801	0.808	0.805	0.810

Panel B: Sinclair Exposure and the Number of Patent Citations

	Ln(1+Cite)$_{t+1\rightarrow t+3}$		Ln(1+Cite)$_{t+2}$		Ln(1+Cite)$_{t+3}$	
	(1)	(2)	(3)	(4)	(5)	(6)
Sinclair TV$_t$	−0.158*	−0.145**	−0.199***	−0.174***	−0.125**	−0.112*
	(0.010)	(0.023)	(0.000)	(0.002)	(0.023)	(0.056)
Ln(Assets)		0.060***		0.030***		0.012
		(0.000)		(0.009)		(0.316)
ROA		0.045		0.030		0.052*
		(0.115)		(0.249)		(0.055)
R&D		0.264**		0.128		0.073
		(0.022)		(0.191)		(0.527)
CAPEX		−0.144		−0.037		−0.087
		(0.145)		(0.680)		(0.353)
Leverage		−0.129***		−0.112***		−0.101***
		(0.001)		(0.000)		(0.001)
Market-to-Book		0.014***		0.017***		0.014***
		(0.000)		(0.000)		(0.000)
Institutional Holding		−0.149***		−0.203***		−0.157***
		(0.001)		(0.000)		(0.000)
Ln(Firm Age)		−0.037		0.029		0.032
		(0.357)		(0.389)		(0.367)
PPE		0.205***		0.170***		0.251***
		(0.008)		(0.007)		(0.000)
Industry HHI		0.428*		0.336		0.470**
		(0.064)		(0.106)		(0.034)
Industry HHI^2		−0.279*		−0.204		−0.294*
		(0.096)		(0.170)		(0.066)
KZ Index		0.000		0.000**		0.000***
		(0.126)		(0.013)		(0.005)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
State×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	58,555	50,466	68,176	58,323	59,551	51,353
Adjusted R^2	0.761	0.759	0.618	0.623	0.625	0.629

TABLE 4: SINCLAIR EXPOSURE AND PATENT RELATEDNESS, ORIGINALITY, GENERALITY, AND ECONOMIC VALUE

This table reports the regression results of Sinclair exposure on patent relatedness, originality, generality and economic value. The USPTO adopts a patent classification system that assigns patents to a three-digit technology class based on technology categorization. We use a concordance table developed by Hsu et al. (2014) that connects the USPTO technology classes to two-digit SIC codes mapping patents in each technology class to one or multiple two-digit SIC codes. The number of related patents in a firm is calculated by multiplying patent counts with the corresponding mapping weights of its main two-digit SIC industry. The number of unrelated patents is the total number of patents minus the number of related patents. A patent's originality score is one minus the Herfindahl index of the three-digit technology class distribution of all the patents it cites. A patent's generality score is one minus the Herfindahl index of the three-digit technology class distribution of all the patents that cite it. We obtain data on the economic value of patents from Kogan et al. (2017). The economic value is calculated using the firm's abnormal stock returns around the announcement of patent grants multiplied by its market capitalization one day before the announcement. The dependent variable is the average value of all patents applied in a three-year window, denoted by $Average\ Patent\ Value_{t+1 \to t+3}$. The control variables are the same as in the baseline regression in Equation 2 and their coefficients are not reported for the sake of brevity. Variable definitions are in the Appendix. All regressions include firm fixed effects and state×year fixed effects. Standard errors are clustered at the firm level. P-values are reported in parentheses. *, **, and *** indicate significance at the 0.1, 0.05, and 0.01 levels, respectively.

	Ln(1+ Related patents)			Ln(1+ Unrelated patents)			Originality			Generality			Value
	$t+1 \to t+3$	$t+2$	$t+3$	$t+1 \to t+3$	$t+2$	$t+3$	$t+1 \to t+3$	$t+2$	$t+3$	$t+1 \to t+3$	$t+2$	$t+3$	$t+1 \to t+3$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Sinclair TV_t	-0.097***	-0.066*	-0.050	-0.108***	-0.090**	-0.074*	-0.019	-0.031***	-0.022*	-0.023**	-0.025**	-0.012	-5.383*
	(0.019)	(0.076)	(0.206)	(0.010)	(0.019)	(0.092)	(0.117)	(0.005)	(0.068)	(0.070)	(0.010)	(0.256)	(0.057)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	50,466	58,323	51,353	50,466	58,323	51,353	50,466	58,323	51,353	50,466	58,323	51,353	18,086
Adjusted R^2	0.904	0.794	0.794	0.908	0.806	0.808	0.727	0.594	0.601	0.640	0.500	0.495	0.872

Table 5:
Sinclair Exposure and Innovation Outputs: Alternative Estimates of DiD

This table reports the impact of Sinclair exposure on corporate innovation using alternative estimators of DiD. Column (1) reports the *aggregate* treatment effect in the window of $(t+1, t+3)$ post Sinclair using Callaway and Sant'Anna (2021). Column (2) reports the *average* treatment effect in the window of $(t+1, t+3)$ post Sinclair using Sun and Abraham (2021). In both models, we include the same set of covariates as in Column (2) of Table 3 and state fixed effects. Control variables' coefficients are not reported for brevity. The control group include the firms that never receive treatment, i.e., Sinclair never enters the local TV market during the sample period. Innovation is measured by $\text{Ln(1+Pat)}_{t+1\rightarrow t+3}$. Sinclair Entry_t is a dummy variable that equals one after Sinclair enters the county where a firm's headquarter is located, and zero otherwise. Variable definitions are provided in the Appendix. Standard errors are clustered at the firm level. P-values are reported in parentheses. *P*-values are reported in parentheses. *, **, and *** indicate significance at the 0.1, 0.05, and 0.01 levels, respectively.

	Callaway & Sant'Anna: Aggregate effect	Sun & Abraham: Average effect
	$\text{Ln(1+Pat)}_{t+1\rightarrow t+3}$	$\text{Ln(1+Pat)}_{t+1\rightarrow t+3}$
	(1)	(2)
Sinclair TV	-0.390^{***}	-0.077^{*}
	(0.000)	(0.075)
Controls	Yes	Yes
State FE	Yes	Yes
Observations	48,555	58,106

Table 6: Robustness of the Baseline

This table shows the coefficient estimates of *Sinclair TV*$_t$ in the baseline regression of Equation 2 when innovation is measured by Ln(1+Pat)$_{t+1\to t+3}$ in various robustness analyses. Panel A uses alternative specifications, while Panel B uses alternative sub-samples. In Panel A, Column (1) includes county-level demographic controls (log of total population, male-to-female ratio, and the percentage of the population with college or higher degrees); Column (2) controls for Industry×Year fixed effects; Column (3) controls for DMA fixed effects; Columns (4), (5), and (6) report the results when standard errors are clustered at the DMA, industry, and county-level, respectively; Column (7) uses the Poisson model instead of the linear regression model. In Panel B, Column (1) includes only firms with mostly local activities; Column (2) includes only firms with mostly non-local activities; Column (3) drops the top 5% of firm-year observations ranked by the number of patents; Column (4) drops the top 10% of firm-year observations ranked by the number of patents; Column (5) drops firms with no patents during our sample period; Column (6) drops firms headquartered in California; Column (7) drops the most liberal states which are the top 5% measured by the latest presidential votes on the Democratic candidate; Column (8) drops the most conservative states which are the top 5% measured by the latest presidential votes on the Republican candidate; Column (9) drops both the top 5% liberal states and top 5% conservative states. Column (10) drops observations during the period of 1996-2008 when Fox News expands across the US. The control variables are the same as in the baseline regression in Equation 2 and their coefficients are not reported for the sake of brevity. Variable definitions are in the Appendix. All regressions include firm fixed effects and state×year fixed effects. Standard errors are clustered at the firm level. P-values are reported in parentheses. *, **, and *** indicate significance at the 0.1, 0.05, and 0.01 levels, respectively.

Panel A: Alternative Specifications

	(1) County-level demographic variables	(2) Industry×Year FE	(3) DMA FE	(4) Cluster at the DMA level	(5) Cluster at the industry level	(6) Cluster at the county level	(7) Poisson model
Sinclair TV$_t$	-0.129^{***}	-0.104^{**}	-0.131^{***}	-0.131^{**}	-0.131^{***}	-0.131^{***}	-0.238^{*}
	(0.008)	(0.024)	(0.007)	(0.018)	(0.004)	(0.006)	(0.094)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry×Year FE	No	Yes	No	No	No	No	No
DMA FE	No	No	Yes	No	No	No	No
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	50,191	50,410	50,466	50,466	50,466	50,374	24,178
Adjusted R^2	0.911	0.914	0.911	0.911	0.911	0.911	0.905

Panel B: Alternative Sub-Samples

	(1) Firms with mostly local activities	(2) Firms with mostly non-local activities	(3) Drop top 5% innovative obs.	(4) Drop top 10% innovative obs.	(5) Drop firms with zero patents	(6) Drop firms in California	(7) Drop top 5% liberal states	(8) Drop top 5% conservative states	(9) Drop top 5% liberal and top 5% conservative states	(10) Drop obs. in 1996-2008
Sinclair TV_t	−0.132*	−0.119*	−0.127***	−0.094**	−0.173*	−0.134***	−0.133***	−0.121**	−0.123**	−0.183**
	(0.082)	(0.070)	(0.003)	(0.013)	(0.056)	(0.010)	(0.007)	(0.012)	(0.011)	(0.011)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	23,565	24,071	47,912	45,339	24,193	41,937	48,227	47,981	45,742	8,822
Adjusted R^2	0.909	0.913	0.863	0.805	0.871	0.904	0.912	0.911	0.912	0.939

Table 7: Sinclair Exposure and Inventor Mobility

This table shows the innovativeness of inventors, categorized as leavers, stayers, or new hires, in treated and control firms before Sinclair's entry. We restrict observations to be within a seven-year window including three years before and three years after Sinclair's entry for all treated firms. For each treated firm, we then select a control firm, which is from a neighboring county, in the same two-digit SIC coded industry, and has no exposure to Sinclair throughout the seven-year period. In the case of multiple matches, we select the one with the closest asset value. The Sinclair's entry year (the event year) for the treated firm serves as the pseudo event year for the control firm. Following prior literature (Gu et al., 2017; Brav et al., 2018), we define an inventor as a "leaver" if he/she filed at least one patent before Sinclair's entry but no patents after Sinclair, and filed at least one patent in a different firm after Sinclair. An inventor is defined as a "stayer" if he/she filed at least one patent in the firm both before and after the event year, but filed no patents for other firms within the event window. An inventor is defined as a "new hire" if he/she filed no patent in the firm but filed at least one patent in a different firm before the event year, and filed at least one patent in the firm after Sinclair. We require both the treated firm and the control firm to have at least one leaver, one stayer, and one new hire. The sample has 221 pairs of treated and control firms. We measure the innovativeness of inventors by their total patent counts during the three-year window before the event year. We first average innovativeness across inventors with the same categorization (leavers, stayers, or new hires) at the same firm, then we average across firms in the treated or the control group before the event year. We examine the difference in innovativeness before the Sinclair exposure between leavers and stayers for both treated and control firms in Panel A, and the difference in innovativeness between leavers and new hires in Panel B. *, **, and *** indicate significance at the 0.1, 0.05, and 0.01 levels, respectively.

Panel A: Difference in Innovativeness between Leavers and Stayers

	Treated Mean Difference (1)	Control Mean Difference (2)	DiD estimator (Treated - Control) (3)
Leavers − Stayers			
Patent counts	0.161***	0.025	0.137*
P-value	(0.0077)	(0.5809)	(0.0693)

Panel B: Difference in Innovativeness between Leavers and New Hires

	Treated Mean Difference (1)	Control Mean Difference (2)	DiD estimator (Treated - Control) (3)
Leavers − New hires			
Patent counts	0.299***	−0.066	0.365***
P-value	(<0.0001)	(0.3931)	(0.0003)

Table 8: The Effect of Sinclair Exposure on Innovation Conditional on Employee Mobility Constraints

This table shows the differential effects of Sinclair exposure on firm innovation conditional on state-level employees' mobility. We repeat the baseline analysis in Table 3 while adding an interaction term of the *Low Enforceability* dummy variable with the Sinclair exposure dummy variable. The *Low Enforceability* dummy variable is equal to one if a firm's headquarter is located in a state where the non-competition enforceability index described in Garmaise (2011) is below or equal to sample median, and zero otherwise. The regressions include firm fixed effects and state×year fixed effects. Standard errors are clustered at the firm level. Variable definitions are in the Appendix. *P*-values are reported in parentheses. *, **, and *** indicate significance at the 0.1, 0.05, and 0.01 levels, respectively.

	Ln(1+Pat)$_{t+1\to t+3}$ (1)	Ln(1+Pat)$_{t+2}$ (2)	Ln(1+Pat)$_{t+3}$ (3)	Ln(1+Cite)$_{t+1\to t+3}$ (4)	Ln(1+Cite)$_{t+2}$ (5)	Ln(1+Cite)$_{t+3}$ (6)
Sinclair TV$_t$	−0.067	−0.020	−0.013	−0.033	−0.049	−0.047
	(0.278)	(0.714)	(0.827)	(0.672)	(0.459)	(0.542)
Low Enforceability$_t$	−0.156	−0.242***	−0.205**	−0.275**	−0.325***	−0.173
×Sinclair TV$_t$	(0.116)	(0.009)	(0.049)	(0.038)	(0.005)	(0.137)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
State×Year	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	50,466	58,323	51,353	50,466	58,323	51,353
Adjusted R^2	0.911	0.808	0.810	0.759	0.623	0.629

Table 9:
The Migration Pattern and the Effects of Sinclair in Red vs. Blue States

This table shows inventors' migration patterns post-Sinclair and the differential effects of Sinclair on innovation in red vs. blue states. Panel A presents the frequency of leavers' migration pattern in red and blue states for treated and control firms. We identify three types of migration patterns: (1) Red to Blue: an inventor from a red state before Sinclair exposure moved to a blue state afterwards; (2) Blue to Red: an inventor from a blue state before Sinclair exposure moved to a red state afterwards; (3) Non-switching: an inventor moved from one state to another state with the same ideology leaning (Blue to Blue or Red to Red). We report the percentage of inventors who relocate within each type of migration patterns for both treated and control firms. Panel B repeats the baseline regression of Equation 2 while adding an interaction term of the *Red State* dummy variable with the *Sinclair TV* dummy variable. The *Red State* dummy variable is equal to one if a firm's headquarter is located in a state where the Republican Party wins the most recent presidential election, and zero otherwise. The control variables are the same as in the baseline regression and their coefficients are not reported for the sake of brevity. Variable definitions are in the Appendix. All regressions include firm fixed effects and state×year fixed effects. Standard errors are clustered at the firm level. *P*-values are reported in parentheses. *, **, and *** indicate significance at the 0.1, 0.05, and 0.01 levels, respectively.

Panel A: Leavers' migration patterns post-Sinclair in red vs. blue states

	Percentage of Dislocations		
Category	Treated	Control	Treated - Control
Red to Blue	8.68	0.89	7.79
Blue to Red	5.11	4.21	0.9
Non-switching	86.22	94.9	–8.68
Total	100	100	
Pearson Chi-Square		176.91***	
P-value		(<0.001)	

Panel B: Effects of Sinclair in red vs. blue states

	Ln(1+Pat)$_{t+2}$ (1)	Ln(1+Pat)$_{t+3}$ (2)
Sinclair TV$_t$	−0.066	−0.057
	(0.193)	(0.298)
Red State$_t$ × Sinclair TV$_t$	−0.072*	−0.052
	(0.069)	(0.195)
Firm FE	Yes	Yes
State×Year FE	Yes	Yes
Controls	Yes	Yes
Observations	58,323	51,353
Adjusted R^2	0.821	0.824

Table 10: DiD Analysis of Inventor Mobility around Sinclair's entry in Red vs. Blue States

In this table we repeat the analysis in Table 7 in red and blue states respectively. Columns (1), (2), and (3) show results in red states, while columns (4), (5), and (6) show results in blue states. We define red (blue) states as the ones where the Republican (Democratic) Party wins the most recent presidential election. In Panel A, we examine the difference in innovativeness between leavers and stayers before Sinclair's entry for both treated and control firms. In Panel B, we examine the difference in innovativeness between leavers and new hires before Sinclair's entry for both treated and control firms. *, **, and *** indicate significance at the 0.1, 0.05, and 0.01 levels, respectively.

Panel A: Difference in Innovativeness between Leavers and Stayers

Leavers − Stayers	Red States			Blue States		
	Treated Mean Difference	Control Mean Difference	DiD estimator (Treated - Control)	Treated Mean Difference	Control Mean Difference	DiD estimator (Treated - Control)
	(1)	(2)	(3)	(4)	(5)	(6)
Patent counts	0.227**	−0.015	0.242**	0.145**	0.035	0.110
P-value	(0.0409)	(0.747)	(0.0141)	(0.0408)	(0.5258)	(0.2291)

Panel B: Difference in Innovativeness between Leavers and New Hires

Leavers − New hires	Red States			Blue States		
	Treated Mean Difference	Control Mean Difference	DiD estimator (Treated - Control)	Treated Mean Difference	Control Mean Difference	DiD estimator (Treated - Control)
	(1)	(2)	(3)	(4)	(5)	(6)
Patent counts	0.414***	−0.108	0.522***	0.269**	−0.055	0.324***
P-value	(0.0032)	(0.2228)	(0.0003)	(0.0010)	(0.5598)	(0.0069)

Table 11: Sinclair exposure and Innovation Inputs

This table shows the effects of Sinclair exposure on innovation inputs: R&D expenses scaled by total assets, and the annual asset growth rate, after one, two, and three years of Sinclair exposure. Columns (1), (2), and (3) report the results on R&D expenses; Columns (4), (5), and (6) present the results on asset growth. Variable definitions are in the Appendix. All regressions include firm fixed effects and state×year fixed effects. Standard errors are clustered at the firm level. P-values are reported in parentheses. *, **, and *** indicate significance at the 0.1, 0.05, and 0.01 levels, respectively.

	R&D			Asset Growth		
	$t+1 \rightarrow t+3$	$t+2$	$t+3$	$t+1 \rightarrow t+3$	$t+2$	$t+3$
	(1)	(2)	(3)	(4)	(5)	(6)
Sinclair TV_t	0.003	0.004	0.004	−0.167	−0.040	−0.019
	(0.420)	(0.285)	(0.155)	(0.323)	(0.219)	(0.605)
Ln(Assets)	−0.005***	−0.005***	−0.003**	−1.473***	−0.283***	−0.239***
	(0.003)	(0.000)	(0.019)	(0.000)	(0.000)	(0.000)
ROA	−0.019***	−0.015***	−0.006	−0.067	−0.024	0.013
	(0.004)	(0.000)	(0.157)	(0.184)	(0.547)	(0.690)
R&D				1.270*	0.269***	0.178*
				(0.058)	(0.005)	(0.097)
CAPEX	0.001	−0.005	−0.025***	0.724	0.064	−0.015
	(0.916)	(0.578)	(0.003)	(0.132)	(0.493)	(0.873)
Leverage	−0.005	−0.005	−0.002	−0.541***	−0.079***	−0.073***
	(0.409)	−0.294	−0.635	(0.000)	(0.003)	(0.005)
Market-to-Book	−0.001***	0.000	−0.001	0.109***	0.004	−0.010***
	(0.049)	(0.321)	(0.196)	(0.000)	(0.000)	(0.000)
Institutional Holding	−0.003	−0.004	−0.002	0.448***	0.054***	0.058***
	(0.427)	(0.220)	(0.476)	(0.000)	(0.009)	(0.001)
Ln(Firm Age)	−0.004	−0.006**	−0.006*	−0.287*	−0.039**	−0.076***
	(0.228)	(0.047)	(0.095)	(0.004)	(0.033)	(0.000)
PPE	0.003	−0.001	0.001	−0.357	−0.020	0.054
	(0.749)	(0.951)	(0.875)	(0.434)	(0.732)	(0.301)
Industry HHI	−0.008	0.000	−0.007	0.019	−0.176	0.015
	(0.452)	(0.962)	(0.548)	(0.971)	(0.109)	(0.878)
Industry HHI^2	0.006	0.000	0.005	−0.100	0.121	−0.027
	(0.503)	(0.978)	(0.574)	(0.782)	(0.126)	(0.706)
KZ Index	0.000	0.000	0.000	0.000	0.000	0.000
	(0.876)	(0.377)	(0.287)	(0.471)	(0.312)	(0.815)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
State×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	50.446	58.323	51.353	50,466	58,122	51,096
Adjusted R^2	0.769	0.759	0.762	0.337	0.125	0.107

Table 12: The Effect of Sinclair Exposure on Innovation Conditional on CEO Ideology

This table shows the effects of Sinclair exposure on innovation conditional on CEO ideology. The variable *Republican CEO* is a binary indicator that equals one if the firm donates a larger dollar amount to the Republican party candidate than to the Democratic party candidate in the ongoing election cycle, and equals zero otherwise. Sinclair TV_t is a dummy variable that equals one if the headquarter of a firm is in a county with at least one Sinclair station in year t, and zero otherwise. $Ln(1+Pat)_{t+1\to t+3}$ is the natural logarithm of one plus the total number of patents filed during the three years of $t+1$, $t+2$, and $t+3$. $Ln(1+Pat)_{t+2}$ and $Ln(1+Pat)_{t+3}$ are the natural logarithm of one plus the total number of patents filed in year $t+2$, and $t+3$. $Ln(1+Cite)_{t+1\to t+3}$ is the natural logarithm of one plus the average number of citations per patent for patents filed during the years of $t+1$, $t+2$, and $t+3$. $Ln(1+Cite)_{t+2}$ and $Ln(1+Cite)_{t+3}$ are the natural logarithm of one plus the average number of citations per patent for patents filed in year $t+2$, and $t+3$. We exclude the firm-year observations near Sinclair's entry year when the three-year moving sum is computed over a period that encompasses the event year, so that all $Ln(1+Pat)_{t+1\to t+3}$ and $Ln(1+Cite)_{t+1\to t+3}$ observations can be clearly defined as before or after Sinclair exposure. Variable definitions are in the Appendix. All regressions include firm fixed effects and state×year fixed effects. Standard errors are clustered at the firm level. P-values are reported in parentheses. *, **, and *** indicate significance at the 0.1, 0.05, and 0.01 levels, respectively.

	$\text{Ln(1+Pat)}_{t+1\to t+3}$ (1)	Ln(1+Pat)_{t+2} (2)	Ln(1+Pat)_{t+3} (3)	$\text{Ln(1+Cite)}_{t+1\to t+3}$ (4)	Ln(1+Cite)_{t+2} (5)	Ln(1+Cite)_{t+3} (6)
Sinclair TV_t	−0.133***	−0.116***	−0.091*	−0.148**	−0.177***	−0.116**
	(0.006)	(0.009)	(0.069)	(0.021)	(0.001)	(0.048)
Republican CEO_t	0.019	−0.025	0.057	−0.011	−0.039	−0.012
	(0.574)	(0.511)	(0.127)	(0.720)	(0.263)	(0.746)
Republican CEO_t	0.070	0.075	0.027	0.070	0.085	0.104
×Sinclair TV_t	(0.254)	(0.330)	(0.735)	(0.237)	(0.227)	(0.112)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
State×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	58,323	66,416	68,176	50,466	51,353	58,555
Adjusted R^2	0.808	0.803	0.801	0.911	0.810	0.906

Table 13: Sinclair Effect, Trade Secrets, and Green Innovation

This table presents the results of two alternative hypotheses. Column (1) to (3) investigate whether the Sinclair effect is different for firms located in states with different trade secrets protection laws by adding the interaction term $USTA \times Sinclair\ TV$, where $USTA$ is a dummy variable indicating whether the state has adopted the Uniform Trade Secrets Act (UTSA) laws. $Sinclair\ TV_t$ is a dummy variable that equals one if the headquarter of a firm is in a county with at least one Sinclair station in year t, and zero otherwise. In these columns we do not include stand-alone UTSA adoption indicator because it is absorbed by the state×year fixed effects. Columns (2) to (5) report on the results on whether Sinclair entry has an impact on green innovations. Following Cohen et al. (2021), green patents are classified according to the Organization for Economic Co-operation and Development. All regressions include firm fixed effects and state×year fixed effects. Standard errors are clustered at the firm level. P-values are reported in parentheses. *, **, and *** indicate significance at the 0.1, 0.05, and 0.01 levels, respectively.

	$Ln(1 + Pat)$			$Ln(1 + Green\ Pat)$		
	$t+1 \rightarrow t+3$	$t+2$	$t+3$	$t+1 \rightarrow t+3$	$t+2$	$t+3$
	(1)	(2)	(3)	(4)	(5)	(6)
Sinclair TV_t	−0.237***	−0.197***	−0.201**	0.010	0.010	0.019
	(0.008)	(0.006)	(0.015)	(0.695)	(0.417)	(0.183)
$USTA_t \times$ Sinclair TV_t	0.156	0.115	0.156*			
	(0.137)	(0.115)	(0.058)			
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
State×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	50.466	58.323	51.353	50.453	58.323	51.353
Adjusted R^2	0.911	0.808	0.810	0.827	0.641	0.644

Appendix

Variable Description

- *Pat*: Total number of patents filed (and eventually granted) in a given year after adjustment for truncation.
- *Cite*: Number of citations received per patent in a given year after adjustment for truncation.
- *Average Patent Value*: Average value of patents as defined in Kogan et al. (2017).
- *Related Patent*: Number of patents that are related to a firm's core business, i.e., the number of patents that are mapped to a firm's main two-digit SIC industry (or industries).
- *Unrelated Patent*: Total number of patents minus related patents.
- *Originality*: One minus the Herfindahl index of the three-digit technology class distribution of all the patents it cites.
- *Generality*: One minus the Herfindahl index of the three-digit technology class distribution of all the patents that cite it.
- *Total Patent Value*: Total value of patents as defined in Kogan et al. (2017).
- *Sinclair TV*: A dummy variable that equals one for treated DMAs, i.e. those that experience Sinclair Media entry.
- *Ln(Assets)*: Natural logarithm of total assets.
- *ROA*: The ratio of operating cash flow to total assets.
- *R&D*: R&D expenditures divided by total assets.
- *CAPEX*: Capital expenditures scaled by book value of total assets.
- *Leverage*: The ratio of book value of total debt to total assets.
- *Market-to-Book*: Ratio of market value of assets (book value of assets minus book value of equity plus market value of equity) to book value of total assets.
- *Institutional Holding*: Institutional holdings (%), calculated as the arithmetic mean of the four quarterly institutional holdings reported through form 13F.
- *Ln(Firm Age)*: Natural logarithm of the number of years since the firm appeared in Compustat.
- *PPE*: Net property, plant and equipment divided by total assets.
- *Industry HHI*: Herfindahl Hirschman Index based on annual sales in each 4-digit SIC industry .
- *KZ Index*: The KZ index is a measure of the severity of financial constraints faced by a firm based on Kaplan and Zingales (1997), calculated as $-1.002 \times \text{Cash flow} + 0.28 \times \text{Tobin's Q} + 3.18 \times \text{Leverage} - 39.368 \times \text{Dividends} - 1.315 \times \text{Cash holdings}$.
- *Asset Growth*: The percentage growth rate of assets from the previous year.