Technical Change and Superstar Effects: Evidence from the Rollout of Television*

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Abstract

Technical change that extends economies of scale can generate winner-take-all dynamics with large income growth among top earners and adverse effects for other workers. I test this classic "superstar model" in the labor market for entertainers, where the historic roll-out of television creates a natural experiment in scale-related technological change. The launch of a local TV station multiplied audiences of top entertainers nearly threefold, and skewed the entertainer wage distribution to the right, with the biggest impact on the very top tail of the distribution. Below the star level the effects diminished rapidly and other workers were negatively impacted. The results confirm the predictions of the superstar model and are distinct from canonical models of technical change.

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1 Introduction

In a celebrated 1981 article Sherwin Rosen argues that technical change can amplify inequality at the top of the wage distribution and generate extremely high paid "superstar" earners. The driving force of such superstar effects are technologies that facilitate an increase in market scale. Rosen concludes that such technologies enable "many of the top practitioners to operate at a national or even international scale ... [and lead to] increasing concentration of income at the top" (Rosen 1981).[1] Superstar theory has become widely popular since Rosen's original article, and has been used to model income inequality in various settings.[2] However, credible causal evidence for superstar effects is lacking and recent advances in empirical methodology have placed a renewed focus on testing such economic theories with clean identification strategies.[3]

This paper uses a natural experiment to test the superstar model and studies arguably the most prominent case, the entertainment sector, during the historic rollout of television.[4] Before the launch of television in the middle of the 20th century, successful entertainers typically had a live audience of a few hundred individuals, while their audience was an order of magnitude bigger after the launch of television. In line with superstar effects, I find that this shift resulted in disproportionate income gains for top entertainers. The launch of a TV station increased the income share of the top percentile of entertainers by 50%, with much smaller gains for back-up stars and serious adverse effects for average talents. Employment and incomes declined sharply below the star level.

The first part of the paper derives testable predictions from the canonical superstar model. The theoretical literature has focused on cross-sectional predictions of superstar-effects, comparing the dispersion of talent to the dispersion of incomes.

[1] Early version of the superstar theory appear in Tinbergen (1956); Sattinger (1975); Sattinger (1979).

[2] See, for example, Kaplan and Rauh (2013); Cook and Frank (1995) for a review of multiple sectors, Terviö (2008); Gabaix and Landier (2008); Gabaix, Landier, and Sauvagnat (2014) for CEOs, Garicano and Hubbard (2009) for lawyers, Kaplan and Rauh (2009); Célérier and Vallée (2019) for finance professionals, and Krueger (2005); Krueger (2019) for entertainers.

[3] List and Rasul (2011) review the use of field experiments to test labor market theories. In the context of technical change, Card and DiNardo (2002); Lemieux (2006) famously stressed the need for clean identification to test skill biased technical change theories, and several subsequent studies indeed leverage exogenous variation to implement such a test (Bartel, Ichniowski, and Shaw 2007; Akerman, Gaarder, and Mogstad 2015; Michaels and Graetz 2018).

[4] Classic studies of superstar effects motivate their analysis with examples from entertainment (see, e.g., Krueger 2019; Cook and Frank 1995; Rosen 1981).

While such cross-sectional predictions are distinctive, they are difficult to test without a credible cardinal measure of talent. I therefore present alternative predictions that focus on *changes* to inequality and are testable in the absence of data on the talent distribution. The relevant technologies for superstar effects are "scale related technical changes" (SRTC) which make large scale production easier.[5] Such technical changes magnify superstar effects and produce characteristic changes in inequality that are different from other classic models of technical change.[6] During SRTC the most talented workers in the profession (the "superstars") attract an increased share of customers at the expense of lower-ranked talents. As a result, the right tail of the income distribution grows, and incomes become concentrated at the top, while employment and returns of lower-level talents decline.

The second part of the paper tests these predictions of the superstar theory. The government deployment plan of early television stations provides clean variation in a large SRTC. Entertainer audiences multiplied through television and shows eventually reached a national audience; however, this transformation took place in stages.[7] Shows on early TV stations were broadcast via airwaves to the local population and in this pioneering period, technological constraints required TV shows to film near the broadcast antennas. As a result, filming occurred simultaneously in multiple local labor markets and provided entertainers a bigger platform in locations where stations were launched. Pioneering work on the US television rollout in Gentzkow (2006); Gentzkow and Shapiro (2008) used the staggered rollout process and regulatory interruptions as a natural-experiment to study the impact on television viewers. I build on this work and study the effect of television on workers in the entertainment sector.

The study uses a difference-in-differences (DiD) analysis across local labor markets during the staggered rollout. The results show evidence of sharply rising superstar

[5] Superstar effects also require imperfect substitutability of talent. Entertainment is often used as a representative case, additionally multiple studies suggest that top talent is also difficult to replace in several other settings (Huber, Lindenthal, and Waldinger 2020; Sauvagnat and Schivardi 2020; Azoulay, Zivin, and Wang 2010).

[6] Influential theories of technical change include skill biased technical change (Autor, Goldin, and Katz 2020; Autor 2014; Acemoglu and Autor 2011; Autor, Levy, and Murnane 2003; Katz and Murphy 1992) and routine biased technical change (Autor and Dorn 2013; Acemoglu and Autor 2011; Autor, Katz, and Kearney 2008).

[7] Mass media (e.g., radio, newspapers and cinema) predates television and could reach a national audience. The variation in television made additional formats of entertainment scalable, and the local variation largely unfolds orthogonal to established media formats. In the analysis, I treat the location of production hubs of the established media as a local fixed effect.

effects. With the launch of a TV station the entertainer income distribution became more right skewed, with most of the skew happening in the very top tail. The fraction of entertainers with incomes that reach the top 1% of the US wage distribution doubled, with smaller increases at slightly lower income levels. Further down in the distribution such gains disappear quickly and lower-ranked talents lost out. The share of entertainers with mid-paid jobs declined, and total employment of entertainers contracted by approximately 13%. Data on consumer expenditure shows that this is driven by a shift away from traditional live entertainment (e.g., grandstand shows at county fairs), as returns and audiences for the most successful TV shows increased strongly. As a result, the income share of the top 1% entertainers increased approximately 50%. In short, SRTC moved the entertainment industry toward a winner-take-all extreme, as predicted by superstar theory.

Two additional sources of variation help to strengthen the identification strategy. First, I use an unplanned interruption of the television deployment process. During the interruption, a group of places that were next in line for television had their permits blocked. Newly collected data on pending regulatory decisions allows me to track affected places.[8] Such places show no evidence of spurious shocks, and results from placebo tests support the assumption that the television deployment was exogenous to local demand conditions. Second, I exploit the staggered launch of TV stations together with the later decline of local filming. The decline provides an additional difference in the DiD setting and allows me to check if treatment effects disappear. While the analysis focuses on the rise and fall of local television filming, nationalized TV stars of course continued to thrive in the subsequent years. Strikingly, by 1993 entertainers had become one of the 5 most important occupations for the US top 0.1% income share (Bakija, Cole, and Heim 2012). Today entertainers reach global audiences and continue to be among the highest-paid individuals in the US economy.[9]

Two "endogeneity challenges" have made it difficult to obtain causal estimates of the effect of technological change on inequality. A first challenge is that periods of technical change often coincide with trends in deregulation and shifts in pay-setting

[8]Previous studies indirectly use this interruption period but lack the data to identify specific locations held up by the interruption.

[9]Among the top 0.1% highest-paid Americans, only finance professionals and entrepreneurs receive higher incomes than entertainers. Entertainers also contribute more to top income shares than medical professionals or CEOs of publicly traded companies and about the same amount as engineers, despite the small size of the sector (Based Tables 3a and 7a of Bakija, Cole, and Heim (2012) and ExecuComp records on the compensation of CEOs of publicly traded companies).

norms, thereby making it difficult to identify the impact of the technical change itself. In this study, I use industry-specific year effects to absorb the impact of aggregate and industry level trends and exploit that the television deployment plan affected different parts of the US at different times. A second challenge concerns the endogenous adaption of technology. Endogenous adaption can lead to a simultaneity problem between variation in technology and local labor market shocks.[10] In the context of television, the rollout deployment rules alleviate this problem. The government used predetermined local characteristics to rank locations in order of priority, thereby making launch dates unresponsive to local shocks and generating variation that is exogenous to local economic conditions. The rollout interruption provides an additional check on this process and we can verify that places where stations failed to launch do not experience spurious effects around planned launch dates.

To examine the robustness of results, I probe whether the change in inequality is driven by television or by other contemporaneous local changes. I find that the effect of television remains broadly similar when I control for changes in the local demographic makeup and different trends between urban and rural areas, and even when controlling for completely flexible differences in linear trends across local labor markets. Furthermore, I confirm that stations only affect local inequality when filming is local. The estimates show that local television stations have an impact in the 1940s and stations seize to have an effect after videotaping eliminates local filming in the late 1950s. This rise and fall pattern suggests that the effects are indeed driven by television and rules out that differential linear trends could be driving the results. Taken together, all these tests confirm that other correlated shocks cannot explain the rise of entertainer inequality after television launches.

One potential driver of the change in the wage distribution is a change in the pool of people who become TV stars. I use panel data and historical records on the recruitment of TV stars to quantify the importance of this channel and find that it played only a small roll. TV stations mainly hired the same actors as pre-television outlets. I also investigate the magnitude of migration responses across local labor markets and again find very minor effects. These results suggest that changes in the return to different ranks of talent are the main driver of the observed increase in inequality.

This study relates most closely to previous work on superstar effects which found

[10] For a discussion of endogenous technical change see Acemoglu (1998), and for historical evidence see Beaudry, Doms, and Lewis (2010).

that this theory could help understand inequality in multiple settings (Cook and Frank 1995; Krueger 2005; Gabaix and Landier 2008; Terviö 2008; Garicano and Hubbard 2009; Kaplan and Rauh 2009; Kaplan and Rauh 2013; Gabaix, Landier, and Sauvagnat 2014; Krueger 2019).[11] Such studies find a strong correlation between top income growth and market reach and argue that superstar effects could explain this correlation. In this study, I exploit a natural experiment to test the superstar mechanism and provides evidence for the empirical relevance of such effects.

More broadly, this study relates to the work on the labor market effects of technological change. Influential work has analyzed effects on the skill premium (Autor, Goldin, and Katz 2020; Autor 2014; Acemoglu and Autor 2011; Autor, Levy, and Murnane 2003; Katz and Murphy 1992) and on routine occupations (Autor and Dorn 2013; Acemoglu and Autor 2011; Autor, Katz, and Kearney 2008) and used natural experiments to test these theories (see, e.g., Bartel, Ichniowski, and Shaw 2007; Akerman, Gaarder, and Mogstad 2015; Michaels and Graetz 2018). Evidence for superstar effects, by contrast, is still scarce.

Another related literature discusses the link between the market dominance of superstar firms and market power.[12] Both these factors relate to market shares, which makes it difficult to distinguish their impacts in practice. I use additional variation to separate the effect of the two and analyze how competition affects the magnitude of superstar effects. Specifically, I differentiate cases where the television deployment process leads to a single television station, allowing for scale but no competing employers, and cases with multiple stations, featuring scale and competition. The results show marked differences across the two settings. Top incomes only increase if employers have to compete for talent, while top incomes are nearly unchanged in locations where a single station operates. As a next step, I estimate rent-sharing equations that quantify the pass-through of show revenues to entertainer superstars.[13] To perform this analysis, I collect additional information on productivity (output prices, audience sizes and revenues). I use this data in an instrumental variables

[11] Other work points out that the rise of information and communications technologies may have created a burst of superstar effects that could help explain why inequality has increased so rapidly in the wider economy (see, e.g., Cook and Frank 1995; Brynjolfsson and McAfee 2011; Guellec and Paunov 2017; Kaplan and Rauh 2013).

[12] Autor et al. (2020) study "superstar firms" and find that the rise in their scale can explain a decline in the labor share. Studies of monopsony power include, among many others, Van Reenen (1996); Harmon and Caldwell (2019); Kroft et al. (2020).

[13] Similar rent-sharing equations have been estimated for construction workers (Kroft et al. 2020), for patenting firms (Kline et al. 2019) and for CEOs (Gabaix and Landier 2008).

strategy, which instruments show revenues with the launch of television stations and find that star entertainers receive roughly 1/5 of the additional revenue associated with the increase in production scale.

In this paper I focus on the impact of SRTC on the labor market, which of course is only one aspect of the social consequences of SRTC. For example, SRTC may play an equalizing force in the marketplace for goods and services. Acemoglu, Laibson, and List (2014), for example, discuss superstar effects in teaching and highlight that SRTC could bring higher quality teaching to a greater number of students.

The rest of the paper is organized as follows: section 2 derives testable predictions from a canonical superstar model, section 3 discusses the television rollout and data collection, section 4 presents the empirical results, section 5 analyses the role of competition, and section 6 concludes.

2 The Superstar Model

This section presents a standard model of the superstar economy and shows how technical change generates inequality. A superstar economy features heterogeneous workers (actors) and employers (theaters) of varying sizes. A theater of size s hires an entertainer of talent t and generates revenue $Y(s,t)$. For simplicity, I assume that each theater hires only one entertainer and produces revenue:[14]

$$Y(s,t) = \pi\phi(st)^{1/\phi}, \tag{1}$$

where π is the output price and ϕ is the scalability parameter. A reduction in ϕ decreases the curvature of the production function and makes large-scale production cheaper. Also note that the Cobb-Douglas exponents on s and t are the same, which may seem like a restrictive assumption; however, when talent t cannot be measured in a cardinal way, any Cobb-Douglas function can be transformed into this type of function by changing the units of t. The assumption is thus without loss of generality and saves on notation. A second important feature of Y is that talented actors have a comparative advantage in larger shows; in other words Y is supermodular in talent ($Y_{ts} > 0$).

The equilibrium is competitive and will meet the social planner optimum, and we can therefore focus on the optimal allocation. The first equilibrium result follows

[14]For more general production functions, see Rosen (1981); Tinbergen (1956); Sattinger (1975).

from comparative advantage. Better actors are more valuable in bigger theaters, and the optimal matching, therefore, requires positive assortative matching (PAM). Formally PAM implies that a matched actor–theater pair are at the same percentile of their respective distributions $p^t = p^s$. The second equilibrium condition follows from incentive compatibility. For continuous distributions of talent and theater size, this requires that wages grow in line with productivity, $w'(t) = \frac{\partial Y}{\partial t}$. Actors and theaters have outside options that are only infinitesimally worse, and as a result, neither party earns rents over their outside option (see e.g., Terviö 2008).[15] The third equilibrium condition is market clearing ($\int_0^1 -Y(s(t), t)dp^t = D(\pi^*)$), with demand $D(\pi^*)$ equal to supply at equilibrium prices π^* (for the formal derivation of the equilibrium, see Online Appendix A.1).

With these three equilibrium conditions, we can derive the wage distribution. To obtain a closed-form solution, we additionally assume that talent t and theater size s follow Pareto distributions, with shape parameters α and β for talent and theater size, respectively (with inverse CDF $p^t = t(p)^{-\frac{1}{\beta}}$ and $p^s = s(p)^{-\frac{1}{\alpha}}$). Similar results hold approximately for broader distributional assumptions (see Terviö 2008; Gabaix and Landier 2008). Combining the incentive compatibility condition with the production function and integrating gives the wage in the superstar economy:

$$ln(w_i) = ln(\lambda) + \frac{\alpha + \beta}{\alpha} ln(s_i^{1/\phi}) = ln(\lambda) + \xi \cdot ln(s_i). \tag{2}$$

An individual i receives wage w_i, which depends on the individual's audience reach s_i and the intercept $ln(\lambda) \equiv ln(\pi \frac{\beta}{\alpha+\beta})$. The effect of audience reach on wages is $\xi = \frac{\alpha+\beta}{\alpha\phi}$. Empirical studies have used equation 2 to estimate model parameters and found that the superstar model closely fits the data in several contexts—for instance, for CEO compensation (Terviö 2008; Gabaix and Landier 2008).[16]

There are at least three challenges with estimating equation 2. A first empirical challenge is that s_i is endogenous, and the correlation with wages is unlikely to produce unbiased estimates of the model parameters. A second challenge is to measure the relevant variation in $s_i^{1/\phi}$. In the model this parameter represents a market reach primitive—equivalent to the audience in the entertainment setting—but

[15] Terviö 2008 concludes: "Due to the continuity assumption, the factor owners do not earn rents over their next best opportunity within the industry." This holds even though each actor and show is a monopolist of its type because of continuity. If the distribution of theater size has jumps, the theater owner at the jump has market power and could extract all the surplus at that jump.

[16] These studies implicitly use the first equality of 2, which relates "effective units" of audience reach ($s_i^{1/\phi}$) to wages and model primitives.

since this input to the production function is rarely observed directly, studies instead use proxies such as profits. A concern with this approach is that profits also capture the endogenous price response and thus lead to biased estimates. A final challenge is that alternative models could yield a similar correlation of wages and market size. The correlation thus provides only weak evidence for superstar effects. The following section provides a framework to tackle these challenges.

2.1 The Effect of Technological Change

To build a more robust test for superstar effects I present additional predictions of the superstar theory. These predictions leverage the effect of SRTC. The magnitude of superstar effects is closely linked to the scale of economic production: when scale economies improve ($\phi \downarrow$), superstar effects get magnified. The following proposition summarizes the impact of SRTC (for derivations, see Online Appendix A.2):[17]

Proposition. *In the superstar economy, SRTC leads to*

a) Top wage growth: Denote the share of workers with incomes above a top income threshold ω *by* $p^\omega \equiv Pr(w > \omega)$. *SRTC increases* p^ω *and more so at higher levels of income:* $\triangle ln(p^\omega) > \triangle ln(p^{\omega'})$ *if* $\omega > \omega'$*;*

b) Fractal inequality: For top income shares (s_x) *at two percentiles* x *and* x', *pay differences increase:* $\Delta s_{x'}/\Delta s_x > 1$ *if* $x' > x$*;*

c) Adverse effects for lesser talents: Employment at mid-pay levels declines; and

d) Employment loss: For a given outside option w^{res} *and corresponding participation threshold* $\bar{p}$, *SRTC leads to* $\frac{\partial \bar{p}}{\partial \phi} < 0$.

The first two results, (a) and (b), state that top earners experience the largest income growth and income *growth rates* escalate towards the top of the distribution.[18] To derive the share of top earners (p^ω), combine the size distribution $p^s = s_p^{-\frac{1}{\alpha}}$ with equation 2.

$$log(p^\omega) = \gamma_0 - \gamma_1^\omega \phi, \tag{3}$$

[17] An equivalent SRTC shock could be modeled as an increase in the dispersion in the size distribution.

[18] Note that the effects are expressed in terms of the share of entertainers above wage thresholds. We could alternatively measure wage growth at different percentiles of the distribution. The two approaches are perfectly interchangeable and the wage distribution provides a direct mapping between the two. The first approach has empirical advantages and I therefore focus on these results.

where $\gamma_0 = ln(\frac{\beta\pi}{\alpha+\beta})\frac{1}{\alpha\xi}$ and $\gamma_1^{\omega} \equiv \frac{ln(\omega)}{(\alpha+\beta)}$ captures the heterogenous impact of ϕ at different wage levels ω. Notice that the coefficient γ_1^{ω} is bigger for larger ω, implying that SRTC ($\phi \downarrow$) has the biggest impact on the superstars in the economy, while the impact decreases as we move further down in the distribution (a). A further implication of this result is that the difference between top income shares increases (b). The top income share of the top 1% of earners, for instance, rises more than the the share of the top 10%, a pattern known as "fractal inequality." A key feature of these results is that they hold independently of the distributional parameters and we can thus test for superstar effects without assumptions on the talent distribution.

The final two results, (c) and (d), capture the winner-take-all nature of superstar effects and state that mid-income jobs are destroyed and overall employment drops as markets move towards a winner-take-all setting. This effect operates through declines in entertainment prices π. A simple case of falling π arises with completely inelastic demand—in this case, the rising scale of stars directly reduces demand for other workers, but more generally the winner-takes-all phenomenon arises when the demand is sufficiently inelastic.[19] The declining price affects the value of the intercept (γ_0) in equation 3. This intercept affects wages at all percentiles equally, but it carries a bigger weight at lower wage levels, where the benefit from scale (γ_1^{ω}) is small. As a result, SRTC benefits stars, while lower-ranked workers suffer from falling demand for their services. Workers whose wage drops below the reservation wage exit entertainment and employment therefore falls.

The effects of SRTC are summarized in Figure 1. The figure shows how wages in entertainment are predicted to spread out relative to the rest of the economy: Extreme pay becomes more common, while the share of mid-income jobs declines. Notice that Figure 1 shows bins that get narrower at the top of the distribution in order to zoom in on the part of the distribution that is most affected. The impact at the top resembles an upward pointing hockey stick, with the sharpest effects at the very top and declining impacts at lower income levels. At the bottom of the distribution, the figure shows a case where SRTC decreases the lower bound of the wage distribution, which in turn results in a growing low paid sector and an increased share of workers at the lowest income levels.[20]

[19] See Appendix A.2 for further details on this demand condition.

[20] Effects at the bottom of the distribution are more sensitive to assumptions. A case with a uniform reservation wage and no adjustment costs, for instance, implies that the lower bound of the wage distribution is fixed at the reservation wage. Demand shifts then only affect employment and do not change wages of low paid workers.

It is useful to compare these effects to alternative models of technical change. Superstar effects differ from a large class of alternative models. The canonical model of skill biased technical change, for example, features only two skill groups and thus produces little top income dispersion. Even extensions to SBTC models will struggle to generate top income inequality, particularly of the fractal nature described above. To replicate fractal inequality with SBTC, we would need to get rid of the groups of perfectly substitutable workers and introduce imperfect substitution between workers. This is in principle feasible by taking the number of skill groups to infinity. Such an approach, however, is unattractive, as it introduces infinitely many parameters and makes the model impossible to falsify. The superstar economy instead provides a parsimonious and thus falsifiable model of income inequality. A second challenge for models with labor augmenting technical change is to generate real wage and employment losses (Caselli and Manning (2019)). The superstar framework produces such losses naturally, as shown by (c) and (d).

An alternative class of technical change models has introduced task-specific technical change (for a summary of task based models, see Acemoglu and Autor (2011)). Such models have similarities to the superstar framework in that the latter also uses an assignment process to assign workers to tasks (or stages in our case). The task framework can produce real wage declines in response to labor augmenting technical progress by shifting workers into other tasks and increasing the supply of workers to such tasks. When it comes to top-income dispersion, task models face similar limitations to SBTC: Workers of equal skills are perfect substitutes and wage dispersion thus arises across skill groups only. We can generate a task model that is near isomorphic to a superstar model by letting the number of tasks and skill groups approach infinity. This generates one-to-one matching between tasks and worker types, just like the superstar model. A remaining difference is how the two models conceptualize technical change. The task model has been used to study the impact of factor augmenting shocks, or changes in the tasks performed by workers. Factor augmenting shocks can produce fractal inequality if we assume that the technological shock is fractal itself, in the sense that technology boosts productivity most for the highest productivity workers. And while it is thus possible to generate fractal inequality, we would practically assume the conclusion that we generate. In the superstar framework technical change (SRTC) affects a different parameter (the scale parameter) and the effect on labor demand at different skill levels arises endogenously, producing fractal inequality.

3 Data and Setting

To take these predictions to the data, I build a novel data set that covers the entertainment sector during the middle of the 20th century. I combine historical records from multiple archival sources to track the locations of technological change, the resulting shift in market reach, and labor market outcomes. In addition, I collect information on administrative rules to isolate plausibly exogenous variation in the television rollout process.

3.1 Television Rollout

At the start of the 20th century, local live shows—particularly vaudeville shows, the legitimate stage, and county fairs—were among the most popular forms of entertainment. Vaudeville shows typically featured a variety of acts, including comedy, stunts, acrobatics, ballet, burlesque and dance, while the legitimate stage presented drama and theatrical plays. The local entertainment sector changed quickly with the launch of television in the 1940s and 1950s. Through television traditional stage entertainment could reach an audience multiple times the size of a live show and began to reach mass audiences.

Early TV stations predominantly filmed their own content and broadcast local shows via airwaves to the local area. This fragmentation of filming was the result of technological and regulatory constraints of the early period of television. The most important reason was the lack of infrastructure to transmit shows from station to station (see Sterne (1999) for a detailed account). A second constraint was that recording technologies were in their infancy and resulted in poor image quality. As a consequence, recorded shows were a poor substitute for local live television shows.[21] Finally, regulation also imposed restrictions on studio locations and required that "the main studio be located in the principal community served."[22] As a result, TV studios were scattered across the country and the launch of a local TV station implied the launch of local filming.

In order to track the rollout, I digitize archival records of TV stations published in "Television Digest" reports and match station addresses to local labor markets. This broadly follows the strategy of Gentzkow (2006); Gentzkow and Shapiro (2008)

[21] Non-local content had to be put on film and shipped to other stations, where a mini film screening was broadcast live. This was known as "kinescope."

[22] See FCC Rules & Regulations, Section 3.613 (version May 1952)

who study variation in TV signal during the roll-out process. Television filming started in the early 1940s, and Figure 2 shows television filming by the time of the US population Census in 1949. At this point 62 stations were active, and the Figure shows where they were located.[23] In the following decades the number of stations grew substantially, but by the mid-1950s local stations started to lose their relevance for filming. This rise and fall of local television filming will provide the basis for a DiD analysis that compares local labor markets during the launch and subsequent decline of local filming.

An unplanned interruption of licensing in 1948 creates a natural experiment that strengthens the identification strategy. During the interruption a group of locations that would have received television narrowly missed out on television launches. The data on the affected places comes again from "Television Digest" which includes a supplement that reports on ongoing permit decisions of the FCC.[24] To exploit the variation from this interruption, I define a "rollout interruption sample" which narrows in on 113 CZs which either had television at the time of the interruption or missed out on television launches because of the interruption.

The principal reason for the rollout interruption was an error in the FCC's airwave propagation model. This model was used to delineate interference-free signal catchment areas, but the error implied that signal interference occurred between neighboring stations. To avoid a worsening of the situation, the FCC put all licensing on hold and ordered a review of the model. Previous studies noted this interruption but lacked the records to identify locations that where held up by the FCC. I collected new data to distinguish such locations from late adopters, and show were blocked stations are located in Figure 2. Licensing only resumed in 1952, delaying the onset of television by at least four years in the affected locations.[25]

After the initial rollout, local television filming eventually declines. The main driver of this decline was the invention of the Ampex videotape recorder which made

[23] I assume that all stations were filming locally at that time. A handful of stations are an exception and operated a local network. This was rarely feasible because the technical infrastructure was still in its infancy. In my main specifications, I code all members of such networks as treated to avoid potential endogenous selection of filming locations within the network.

[24] I use the "TV Directory" No. 6 of January 1949 to identify places affected by the interruption. All places where the FCC had started vetting potential licensees ("applicants") at this time are coded as affected by the interruption.

[25] The timing of the interruption (1948–1954) coincides with the 1950 US decennial Census which makes it possible to investigate the labor market consequences in detail. Initially, the interruption was expected to last a year. However, the review was delayed to ensure compatibility with rising new transmission technologies (UHF and color transmission).

recorded shows a close substitute for local live shows. Once videotaping was possible, stations increasingly substitute away from local filming. This decline of local filming provides an additional check for the analysis, as the impact of local station should fade as local filming declines. The videotape recorder was first presented at a trade fair in 1956, and immediately more than 70 videotape recorders were ordered by TV stations across the country. That same year, CBS started to use the technology, and the other networks followed suit the next year, resulting in the rapid decline of local filming. In subsequent years television filming started to concentrate in two hubs, Los Angeles and New York, and declined in other locations.[26] During this videotape era, we have to account for the emergence of national filming hubs and regressions will include fixed effects for hubs in the post-videotape period. To avoid a potential endogenous control issue, I do not control for filming hubs directly but use a proxy for comparative advantages of a location as a filming hub. These proxies are based on a location's fixed characteristics, such as sunshine hours and landscapes, that largely drove location decisions. I quantify such predetermined factors using the share of movies filmed in the local labor market in 1920.[27]

3.2 Labor Market Data

Data on labor market outcomes are based on multiple historical sources. The first set of outcomes comes from the microdata samples of the decennial US Census (1940–1970). I focus on five entertainment occupations that benefited from the introduction of TV: actors, athletes, dancers, musicians, and entertainers not elsewhere classified and track their labor market outcomes across the 722 local labor markets that span the mainland US.[28] The Census first collected wage data in 1940, and in all years asked about annual wage income in the previous year; wages reported in 1940 thus refer to 1939.[29] The wage data is top-coded, but fortunately, the top code bites above the 99th percentile of the wage distribution, and up to that threshold, detailed analysis of top incomes is possible.

To evaluate the predictions of the superstar model, I compute several inequality

[26]This trend was also helped by the contemporaneous rollout of coaxial cables that allowed producers to relay live shows from station to station.

[27]The historic location data of movie filming comes from the online Internet and Movie Database (IMDb).

[28]I follow Autor and Dorn (2013) and define local labor markets based on commuting zones (CZ).

[29]The Census reports wage income in all sample years, while business income is not consistently available. I therefore focus on wage incomes.

metrics at the local entertainer labor market level. A first set of outcomes focuses on the rank of local entertainers in the US income distribution. I first compute the share of local entertainers that reaches the top 1% of the US income distribution.[30] The value goes from 0 when no entertainer earns such extreme to 100 in a winner-takes-all market with a single superstar entertainer.[31] The share in market m at time t is given by

$$p_{m,t}^{\omega^{99}} = \frac{\sum_{i\epsilon I} E_{i,m,t}}{\bar{E_t}}, \tag{4}$$

where E is a dummy that takes the value 1 for entertainer occupations and I is the set of workers in the top 1% of the US wage distribution. The wage top code bites above the 99th percentile of the US distribution and we can thus identify all workers in the top 1%.[32] A potential issue with these shares is that fluctuations in the denominator can generate spurious effects. To prevent this, I use the number of entertainers in the average labor market ($\bar{E_t}$) as denominator instead of local labor market counts.[33] As an alternative approach, I compute per capita counts which use the local population as the denominator. These measure map directly into the predictions derived in section 2 and measures how the top tail of the entertainer distribution stretches out relative to the US distribution. We can naturally extend the analysis to other percentiles and study where entertainers rank in the US distribution across all income ranges. Finally, I also compute the wage at the top percentile of the local entertainer distribution and top income shares of local entertainers.

I complement this data on local inequality with a small panel on the work history of TV superstars. The large amount of fan interest generates unusually detailed records on the background of this group and makes it easier to identify and track the history of entertainer stars. The data on TV stars comes from the 1949 "Radio and Television Yearbook" which publishes an annual "Who is Who" in television—a list of stars similar to modern Forbes lists. The data covers the top 100 or so most successful

[30] This metric is similar in spirit to Chetty et al (2017) who also study ranks of local workers in the national distribution. The authors highlight that such ranks have advantages over income levels for comparisons over longer time periods.

[31] In the baseline estimates, I code areas without local entertainers – for instance areas where television displaced all local entertainers – as 0.

[32] The relevant top 1% thresholds are: 7,555 8,050 11,859 16,247 in 1950 USD for 1940, 1950, 1960 and 1970 respectively.

[33] To interpret the estimates as percentage point changes, I normalize by the average number of entertainers in *treated* labor markets. Note that this normalization also implies that $p_{m,t}^{\omega^{99}}$ can in principle be bigger than 100. For robustness, I also run the regressions without the normalization and find similar results (for more details, see Appendix B5).

TV entertainers and their demographic information (e.g., names, TV station employer, birthdays and place of birth) but not income. To obtain information on their pre-TV careers, I link this data to de-anonymized records of the 1940 Census. This link is based on names and additional demographic information (e.g., place of birth, birth year, parental information) and I can uniquely identify 59 of these TV superstars in the Census.[34] While the data is inevitably imperfect, it offers a rare window into the background of the stars of a profession and allows me to study the background of the group that benefitted most from the SRTC of television.

To measure the effect of TV on traditional live entertainment, I collect additional data on attendance and spending at county fairs. The data cover annual records of revenues and ticket sales for more than 4,000 county fairs over 11 years (1946–1957) and spans most US local labor markets. I collect these records from copies of the "Cavalcade of Fairs," an annual supplement to *Billboard* magazine and compute spending at local fairs for three spending categories that are differentially close substitutes for television: spending on live shows (e.g., grandstand shows), fair entrance tickets, and carnival items (e.g., candy sales and fair rides). Live shows most closely resembled TV shows at the time, while candy sales and fair rides are by nature less substitutable with TV.

Finally, I trace where and when county fairs faced competition from TV shows. Figure 3 shows where TV was available in 1950, based on TV signal data from Fenton and Koenig (2020). The year 1950 falls in the period of the rollout interruption, and hence a number of places that were meant to have TV did not yet. Records of technical features of such stations allow me to reconstruct where such stations would have broadcast, and these locations are also illustrated in Figure 3.

3.3 Audience and Revenue Data

The entertainment setting offers a unique opportunity to quantify the market reach of workers by measuring show audience sizes. I collect data on audiences and revenues of live and TV shows from archival records. For live shows I use the venue capacity reported in the 1921 *Julius Cahn-Gus Hill Theatrical Guide*. This guide claims to provide "complete coverage of performance venues in US cities, towns and villages"

[34] To maximize the match rates of the "Who Is Who" and Census data, I supplement the available demographic information with hand-collected biographic information from internet searches. As a result, I achieve a 70% unique match rate among the 68 records with birth-year information, while a few cases are matched without birth-year information.

and covers over 3,000 venues across roughly 80% of local labor markets.[35] For TV shows I compute the number of TV households in a station's signal catchment area. This uses TV ownership data from the Census and signal data from Fenton and Koenig (2020). I also collect price information from TV stations' pricing menus, the so called "rate cards," and compute the revenue of local shows. TV shows provided an enormous step-up in the revenue and audience of entertainment shows.[36] Before television, live shows reached on average 1,165 people, while the median TV station could reach around 75,000 households.

Additional details on the data collection, the data processing and summary statistics are available in Online Appendices B.1 and B.3.

4 Empirical Results

The distribution of incomes in entertainment was far more equal in 1939 than it is today. Figure 4a shows the income distribution among entertainers before television, in 1939, and after the rise of television, in 1969. Over this period pay dispersion grew substantially: wages at the top grew disproportionately, many mid-income jobs disappeared, and a larger low-paid sector emerged. At the same time, employment growth in performance entertainment lagged behind the employment growth in other, non-scalable, leisure-related activities, e.g., restaurant and bar workers, fountain workers, and sport instructors (Figure 4b). This pattern of rising dispersion in log pay and the lack of employment growth is precisely what characterizes superstar effects. Yet from these aggregate patterns it is unclear whether the rise of television during this period is just a coincidence or is driving these effects.

I use a DiD regression across local labor markets to identify the effect of television. The variation comes from the local deployment of TV stations during the 1940s and early 1950s and from the subsequent demise of local filming in the mid-1950s. I track changes in the local entertainer wage distribution during this period. I run the regression at the a disaggregated labor market (m), year (t), occupation (o) level

[35] According to the guide, "Information has been sought from every source obtainable–even from the Mayors of each of the cities" (p. 81). Undoubtedly the coverage was imperfect and small or pop-up venues were missed, but since we focus on star venues these omissions may be of lesser concern. I use the largest available audience in the local labor market to proxy for a star's show audience. I probe the reliability by manually comparing specific records with information from archival data, and the data seem reliable.

[36] For details on revenue data, see Online Appendix B.3. For TV shows, prices are imputed based on estimates of the demand elasticity in a subset of 451 markets where data are available.

and control for occupation-year fixed effects to capture potential time fluctuations in the occupation definition. The athlete occupation, for instance, is subsumed in the nec entertainer category in 1960. The standard errors ϵ_{mot} are clustered at the local labor market level so that running the analysis at the disaggregated level will not artificially lower standard errors. The full sample thus includes 722 local labor markets, 5 occupation groups and 4 Census years (2 for athletes) and hence uses 13,718 observations and 722 cluster:

$$Y_{mot} = \alpha_m + \delta_{ot} + \gamma X_{mt} + \beta TV_{mt} \cdot D_t^{local} + \epsilon_{mot}. \tag{5}$$

Y_{mot} measures labor market outcomes (e.g., the share of entertainers in the top 1% of the wage US distribution), α_m and δ_{ot} are labor market and occupation specific year fixed effects, and X_{mt} is a vector of control variables and includes the control for filming hubs of the post-videotape period. The treatment variable, TV_{mt}, is the number of local TV stations, and D_t^{local} is a dummy that takes the value 1 when TV stations film locally, before the rise of the videotape recorder in 1956. TV_{mt} thus captures the staggered rollout, while D_t^{local} captures the eventual decline of local filming. In addition to a standard DiD set-up, we here observe both the rollout and the removal of local TV filming and thus have access to a third "diff" that helps with identification.

Consider how the variation in TV_{mt} relates to previous production technologies. Before television radio, newspapers, and movies where already popular mass-media formats and the time invariant effects of pre-existing production hubs will be absorbed by fixed effects. Additionally, note that we would not be able to detect effects of television if entertainers' audience reach was unaffected by the launch of television.[37] In practice, television produced a sharp shift in audiences – which we will discuss in detail below – and this shift provides sufficient power to pick up superstar effects.[38]

A major advantage of this setting is that a government deployment process drives variation in TV_{mt}. The government deployment rules generate variation in TV that is orthogonal to local demand shocks. This deployment process therefore breaks the

[37]The validity of the test would be unchanged but the power of the test would be reduced.

[38]Television expanded audiences for two principle reasons: First, new entertainment formats became scalable, particularly ones that relied on visual broadcasts. The study focuses on the group of entertainer occupations that were mainly affected by the launch of television. Second, the regional variation of the rollout is largely orthogonal to existing hubs. In places with pre-existing hubs, the permanent effects will be absorbed by location fixed effects and the impact of television is identified through local changes in inequality.

direct link between local economic conditions and television launches and avoids the simultaneity problem that arises from ordinary, endogenous technology adoption.

There is, however, still potential for endogenous variation if the government decisions themselves are influenced by local demand conditions. I investigate this possibility in archival records of the government decision rules. These records show that decisions rules were indeed independent of local demand and fairly rigid. The 1952 "Final Television Allocation Report," for instance, prioritized locations by their local population in 1950 and the distance to the nearest antenna.[39] The government priority ranking is thus based on pre-determined location characteristics and is by construction unresponsive to local demand shocks.

A further advantage of this setting is the rare use of television outside the entertainment industry. Television was hardly ever used in other industries. Many similar SRTCs, by contrast, simultaneously affect multiple sectors and thus generate several simultaneous changes that make it difficult to identify superstar effects. The near exclusive use of television in entertainment makes this setting a particularly clean case of SRTC.

4.1 Results: Rising Returns at the Top

The first set of tests study Proposition (a) and tests the impact of SRTC on the top of the income distribution. A first outcome is the share of entertainers among the top 1% highest-paid Americans.[40] Growing right skew of the distribution implies that the share of entertainers at such extreme income levels increases. Indeed, a local TV station roughly doubles the share of local entertainers in this income group, increasing the size of this group by about 4 percentage points (Table 1, Panel A).

Similar results hold for the number of top-paid entertainers per capita (Panel B). These estimates confirm prediction (a) and show that TV creates a group of extremely highly compensated entertainers. The gains among top entertainers are particularly remarkable in the context of the historical period. Top income growth in the overall economy was low in the mid-20th century and the growth among entertainers thus stand out.

[39] Published as part of the FCC's *Sixth Report and Order* (1952)

[40] Top earners in entertainment were notably more diverse than other sectors. Women made up 15% and non-white minorities 1.5% of top earners, while other high-paid occupations where essentially closed to women and minorities. Less than 2% of top paid managers, lawyers, medics, engineers and service sector professionals were women and less than 0.5% where from minority groups.

A potential identification concern are differential local trends. As a first pass, I use two specifications to check for such effects. The first specification, column 2, adds control variables that proxy for local economic changes (i.e., local median age and income, % female, % minority, population density, and trends for urban areas). These estimates yield very similar results. The second specification, column 3, is less parametric and allows for location-specific time trends. Such a specification will capture differences in local trends, independent of their source. It is a very demanding specification that adds more than 700 coefficients and while standard errors increase, the results remain remarkably close to the baseline. Both results thus indicate that differential local trends are not driving the findings.

Finally, in Panel C I restrict the sample and compare places with television to places that narrowly missed out on television launches during the roll-out interruption (the "rollout interruption sample"). The advantage of this approach is that it drops more rural areas from the analysis and makes the control group more similar to the treated areas.[41] Results with this sample are very similar to the baseline results and suggest that the previous estimates based on the general roll-out rules are valid.

The Rollout Interruption

The key identification assumption of the DiD is that TV launch dates are unrelated to local shocks or trends. The previous results with added local trends alleviate concerns about spurious local trends, but we may still worry about shocks in unobservable variables that are not captured by these controls.

The interruption of the television rollout provides a powerful test for such spurious effects. Recall that all planned launches were blocked in an indiscriminate fashion and this interruption thus generates variation that is independent of local economic conditions. We can test if spurious effects arise at the time of planned television launches and thus check if the rollout process is correlated with local labor market shocks. Figure 5 plots the number of approved TV licenses over time and shows the sudden drop in approvals at the time of the interruption. I use such places for a placebo test that compares untreated and blocked locations. This is implemented in a dynamic DiD regression that uses blocked stations ($TV_{mt}^{blocked}$) as treatment:

[41] Note that differences between treatment and control areas are not necessarily a problem for identification as location fixed effects will account for time-invariant differences. More broadly, the weighted regressions give relatively little weight to less populated observations, which implies that such observations don't matter much for the results.

$$Y_{mot} = \alpha_m + \delta_{ot} + \gamma X_{mt} + \sum_t \beta_t TV_{mt}^{blocked} + \epsilon_{mot}. \tag{6}$$

Here, β_t captures spurious shocks in places that were meant to be treated but narrowly missed out. Figure 6a plots these coefficients and shows a strikingly parallel trend. Blocked locations show no sign of spurious changes neither before, after, or during the time of blocked launches. These results are precisely estimated and rule out even relatively small violations of the parallel trends assumption. This result also confirms that the rollout rules, which did not take local demand condition into account, were followed through in practice. Also notice that this result goes beyond conventional pre-trend checks. Pre-trend checks focus on trends before the treatment, but with the blocked station experiment, we can additionally test for spurious shocks at the time of and after the planned TV launch date. For completeness, I also perform alternative robustness checks with placebo occupations (Online Appendix B.2.1) and conventional pre-trends (Online Appendix B.2.2) and both also find no spurious effects.

We can additionally test parallel trends *within* treated labor markets by comparing the period before the launch of television and after the decline of local television. Such a test is similar to a pre-trend test but additionally leverages that we observe the removal of local filming. We can probe whether the treatment effect arises and disappears with the rise and fall of local filming. I implement this test in a dynamic DiD regression, using equation 6 with local TV filming as the treatment variable. The results confirm the expected pattern; differences between treated and untreated areas appear when TV stations are launched, and disappear again as such stations lose their importance for filming (Figure 6b plots β_t). In 1969 differences between treatment and control groups reverted to the pre-treatment level.[42] This finding again supports the parallel trend assumption and rules out even relatively complex deviations from parallel trends. For instance, exponential and linear growth rates might look similar at the start of the trends and pre-trend checks might not pick up any differences. By leveraging the post treatment period, we can check for spurious differences that only emerge in the longer run and can thus rule out such non-linear differential trends.

[42] National filming hubs emerged in this period and those locations saw fast top income growth in this period (results for hubs are available upon request).

Migration of Entertainers

For the interpretation of the results, it will be useful to distinguish between two potential mechanisms: migration of entertainers and changing returns to talent. The Census asks individuals if they migrated recently and I use this information to test for the entertainer migration response and find very small effects. The point estimates are negative and confidence intervals are tight (Table 2). Migration thus appears to contributes little to the results. A potential explanation for the limited mobility response is that early shows tended to focus on local events, following the tradition of vaudeville, and thus did not translate easily to other locations. We can use the mobility estimates to bound the impact of migration. The central estimates suggest that mobility plays next to no role in the results and at the upper bound of plausible values, the migration channel can explain a quarter of the total effect.

A related concern are commuting across local labor market boundaries. Such behavior would downward bias the estimates by spreading the impact of local shocks beyond the boundary defined by commuting zones. Commuting is arguably easiest between neighboring areas, and we can thus alleviate the impact on the results by excluding areas that are adjacent to television launch locations from the analysis. Results that exclude such neighboring areas show very similar effects to the baseline, indicating again that migration plays a minor role in these findings (Table 2, Panel B).

Entertainer Talent Distribution

The analysis implicitly assumes that the distribution of entertainer talent is unchanged by the technological environment. While talent cannot be measured directly, we can probe the assumption with panel data and test if TV stations hired people who were already top earners in the pre-television era. A stable distribution of talent implies that star entertainers remain at the same income rank and entertainers who appear at the top of the distribution after television should also appear at the top before television. Instead, we would expect leapfrogging in the distribution if stations relied on a different type of talent. The panel data shows no leapfrogging and instead reveals that television stars where already disproportionally high-paid before television (see Figure 7).

These empirical results align with historical accounts of this period. Scholars of early television highlight that early television relied heavily on established show formats and often broadcast vaudeville shows (for an overview see, Murray 1999). Television stations poached stars from existing shows and *Variety* magazine reported about the resulting tensions in 1949: "Criticism is being advanced in the trade that television so far has not kept its promise of developing its own talent." The television industry responded to this criticism and actively encouraged poaching, arguing that "stars are not going to be made by television. Television is going to be made by stars. So—let's go out and get them!"[43] These historical sources thus also confirm that early television targeted the same type of talents as the pre-TV era.

The 99th Percentile of the Distribution

It is useful to quantify the change in incomes at the top of the income distribution directly. I compute the top percentile of local entertainer wages and restrict the sample to larger labor markets. In most cases, this approach uses the highest observed entertainer wage in the local labor market as proxy for the top percentile.[44] I then regress the log of these wages on the number of television stations in the local labor market.[45] The unit of analysis is the CZ-year level and unlike in previous specifications, we cannot disaggregate the results by occupations because quantiles are not additively separable into sub-groups.[46] The baseline results use the "rollout interruption sample" and compare places where television was launched to ones where launches where blocked during the roll-out interruption and results are robust to alternative sample choices (see Appendix B.2.4 for alternative samples).

I find a sharp and sizable increase in top entertainer incomes with the launch of a local TV stations. Panel A in Table 3 shows an increase in the 99th percentile by 18 log points, or approximately 20%. A 20% wage increase is large in any context, but it is a particularly striking increase given that the regression includes year fixed effects and the results are thus on top of average wage growth. The 95 percent confidence interval ranges from a 5% growth to 35% and is thus relatively large. Allowing for broader samples that introduce additional control areas increases the precision and yields similar point estimates (see Appendix B.2.4).

[43]See, respectively, Bob Stahl, "Where's that New TV Talent? Medium Scorned for it's Laxity," *Variety*. 26 Oct. 1949:1, and "Video Needs Comedy: Tele-viewers Prefer Variety Show," *Television World*. 24 May 1948:3.

[44]If over 100 individuals are sampled, I use the sample weights to compute wages at the 99th percentile (19% of observations).

[45]This approach amounts to a quantile DiD estimate (Chetverikov, Larsen, and Palmer 2016).

[46]Note that the aggregated regressions use fewer observations but have the same power as disaggregated specifications as the number of CZ clusters stays the same.

In 10% of cases the 99th percentile wage exceeds the top code, and I show that results are robust to using alternative methods from the literature to adjust for top-coding. The first set of specifications in Panel A make no adjustments for the top code and thus ignore earnings growth beyond the top-code level. This will underestimate the true top earning growth and, as a result, likely provides a conservative estimate for the magnitude of superstar effects. In Panel B I use the fixed-multiple approach to top-coding and assume a constant multiplier of 1.5 (see e.g., Juhn, Murphy, and Pierce 1993; Lemieux 2006; Autor, Katz, and Kearney 2008). In Panel C I use local Pareto approximations to impute the top coded wages.[47] As expected, imputing incomes beyond the top code raises the magnitude of the effects somewhat. The estimates remain in the same ballpark; at the 99th percentile income growth is 20% to 30%. Specifications that add controls for demographics or location specific trends yield similar results.

4.2 Distinguishing the Superstar Mechanism

4.2.1 Results: Demand for Non-Stars

A second implication of the superstar economy is the shift of labor markets towards a winner-take-all market. Rising demand for stars is accompanied by declining interest in ordinary local live entertainment and thus reduces total employment in the industry (Proposition (d)). These effects should occur in all areas that receive TV signal. Since television signal often extends beyond the local labor market where television filming takes place, more areas are affected by signal, than by filming. To measure the impact of TV on local entertainer employment I therefore use variation in the local exposure to television signal. Such data is not available at an individual channel level from 1960 and I instead use a dummy that takes value 1 when TV signal is available in the local area.

The corresponding DiD regression shows severe adverse effects on local entertainer employment. Around 13% of jobs are lost when TV can be watched locally (Table 4, Panel A, column 1). This confirms that SRTC generates employment losses and is sharply at odds with models where technological change causes a positive demand shock, which would raise employment.

Since these specifications use variation from TV signal rather than from TV filming, it is salient to probe the identifying assumption again. As before, results are

[47] For details on the procedures, see Online Appendix B.3.

robust to the inclusion of controls and local trends (Table 4, columns 2 and 3).[48] Further, common trend tests also suggest that the setup is valid. First, we again leverage the placebo tests with stations that were blocked by the rollout interruption. These places again show no sign of spurious effects (Table 4, Panel B). A second test focuses on differential pre-trends in treatment and control areas right before the treatment by including a lead of the treatment in the regression. To perform this test, we expand the sample period backward by a decade. This is feasible since consistent employment data had already been collected in the 1930 Census. Re-running the baseline regression for the 1930–1970 period yields nearly identical results (Table 4, Panel C). Turning to the pre-trend check, the point estimate on the lead variable coefficient is small and insignificant and thus shows parallel pre-trends in the lead up to TV-signal (column 4).[49]

I next test a third consequence of superstar effects, the decline in mid-paid jobs (see Proposition (c)). First, consider entertainers at the upper end of this spectrum, between the 75th and 90th percentile of the US wage distribution (Figure 8). These are entertainers who receive above-average pay but are outside the very top of the entertainer distribution. The share of entertainers with pay in this range declines by around 50% after the launch of a TV station. The results look similar for entertainers between the median and the 75th percentile. Mid-paid entertainer jobs thus disappeared with the launch of television and made it substantially worse to be an entertainer outside the group of stars during the TV era.

The corollary to disappearing mid-paid jobs is the growing low-paid sector. Analyzing the share of entertainers paid below the median, we observe a modest rise in entertainers with wages at the very bottom of the distribution and little change in the second quartile.

The driver of the employment losses in the superstar model is a shift in demand away from traditional live shows. This channel can be traced directly by quantifying the shift in expenditure at local live venues. Data on spending at county fairs, a form of entertainment widely available throughout the US, is available at annual frequency between 1946 and 1957. I aggregate the spending information at the local labor market and county levels and regress spending on the number of television channels

[48]Median income is not available in 1930 and controls in the extended sample use the remaining variables.

[49]With local filming we observed the introduction and disappearance of local filming. With TV signal there is less variation, TV stations continue to broadcast signal after the end of local filming, and we thus have to rely on the pre-period for common trend checks.

available in the local area.[50] The results show that television leads to a 5% decline in audiences and spending (Table 5, Panel A, column 1 and 2). However, these estimates are noisy and hide substantial heterogeneity across types of entertainment. Substantial negative effects occur among spending categories that are similar to TV shows (e.g., grandstand shows), while demand for entertainment that is different from TV shows (e.g., candy sales and amusement rides) holds up (Table 5, Panel A, related regression at the county level are reported in Panel B). The rising popularity of television shows thus came at the expense of traditional performance shows and hurt local live entertainment.

4.2.2 Results: Fractal Inequality

A third implication of superstar effects is an increased right skewness of the wage distribution (see Proposition (b)). A non-parametric test of this prediction studies whether very high income jobs grow disproportionally faster than slightly lower paid jobs. I start this test by repeating the baseline DiD regression for jobs with income below the top 1% but still among the top 5% of the US wage distribution. Figure 8 shows that television has a positive effect on this income range but the effect is only one-tenth the size of the effect at the very top. To confirm this pattern we can look at the next-lower wage bin, between the 90th and 95th percentiles. Already at this point television stops having a positive impact, confirming that the gains from television fade quickly as we move away from the top. TV appearances generated a small group of superstar earners, a moderate group of backup stars, and had no discernible benefit for other top earners.

This growing fractal inequality is also reflected in increasing top income dispersion within entertainment. Table 6 shows the impact on the income shares of top entertainers.[51] To compute such top income shares, we need information on the full population or a parametric assumption about the shape of the top income tail. In line with the wider literature on top incomes shares and Table 3, I use Pareto approximations to compute such shares.[52] As discussed above, such imputations

[50] Data on TV signal is available at the individual channel level between 1946 and 1957 and we thus do not have to reduce the variation to a dummy, as done for the longer sample period of table 4.

[51] Top income shares are widely used to measure inequality at the top. See, for example, Piketty and Saez 2003; Piketty 2014.

[52] Table 3 uses Pareto approximations for top-coded observations only, here we additionally require such approximations in all cells without information on the full population.

are less reliable in small samples and the regressions use weights that put more weight on larger CZs. Additionally, I show that the results are robust to alternative sample restrictions. Panel A computes top income shares in all cells with at least 20 entertainers and Panel B uses the "rollout interruption sample," focusing on areas with local television filming or affected by the interruption.

The launch of a TV station increased the top 1% income share by 45 log points, or 57% (Panel A of Table 6). In line with Proposition (b)—which suggests that the growth in these shares escalates toward the top of the distribution—I find that income gains for the top 1% are substantially bigger than among the somewhat broader top 10%. The income share of the top 10% increases by a quarter, or 23 log points. The biggest gains, however, occur in the very top tail of the distribution among the top 0.1%. This group nearly doubles its top income share. A formal test of Proposition (b) tests whether these growth rates are equal. The data confirms Proposition (b) and strongly rejects the equal growth rate hypothesis (see Table 6). Similar results hold in the more restricted sample of Panel B.

5 Magnitudes, Monopsony, and the Labor Share

Important previous work on superstar effects has evaluated how well a calibrated superstar model can explain top income growth and thus focused on the potential magnitude of superstar effects (see e.g., Gabaix and Landier 2008; Terviö 2008). To simulate wages, such studies calibrate the superstar model's critical structural parameters to estimates of the elasticity of top pay to market size, using equation 2. In the entertainment setting this elasticity can be estimated with an instrumental variable (IV) approach. I estimate this elasticity with the following regression

$$ln(w^{99}_{m,t}) = \alpha_0 + \alpha_1 ln(s^{99}_{m,t}) + \epsilon^{99}_{m,t}, \quad (7)$$

where α_1 is the relevant elasticity, $w^{99}_{m,t}$ is the 99th percentile of the entertainer wage distribution in market m and year t, and $s^{99}_{m,t}$ is the effective size of the market that such entertainers can reach. An attractive feature of the entertainment setting is that we observe audience sizes and can thus measure workers' market reach directly.[53]

The IV estimate of equation 7 instruments $s_{m,t}$ with TV launches. The first

[53] Using market size as regressor isolates the market size effect from the market price effect and thus is preferable to the conventional approaches that use Dollar value based proxies, such as total firm value

stage of the IV estimates the effect of TV on audience size. Data on audience size is not available in all local labor market and to maximize power, the specification uses all cells where data is available and does not restrict the control group. The results are however robust to narrower control groups (see Online Appendix B.2.4). The DiD shows that the launch of a TV station increases the audience of the largest shows by 104 log points, or roughly a three fold increase in market size (Table 7, Panel A). The increase in audience, of course, also translates into a major change in revenues. A DiD regression on revenues shows that revenues of stars' shows roughly doubled (Panel B). These estimates are highly significant and thus provide a strong first stage. The critical value of the associated F-test is larger than 20 and well above conventional cutoff levels.

Before turning to the IV results, I estimate a benchmark OLS regression. This treats the data as repeated cross-sections and does not use an instrument. The OLS estimate for α_1 is highly significant with a point estimate for α_1 of 0.23 (Table 8, Panel A). Top entertainers thus earn 23% higher wages in local labor markets where theaters are twice as large. The OLS estimate may however be biased by confounding differences between local labor markets, and in fact the wage premium disappears almost entirely once we control for local labor market characteristics (column 2).

The IV results show an elasticity of 0.17, which still is a sizable effect but approximately 30% lower than the cross-sectional OLS estimate. This result thus suggests that the OLS estimate potentially overstates the magnitude of the superstar effect.

I use these estimates for a back of the envelop calculation of the impact of superstar effects on entertainers top-income growth. Between the beginning of the century and 2010 show audiences of major shows multiplied roughly 200 fold and the estimates would imply that this multiplied top incomes 34 fold.[54] In practice, top incomes converted to 2010 prices increased from around \$70,000 in 1939 to a little over \$3.5 million in 2010, a 50-fold increase. Superstar effects can thus account for two-thirds of the top income growth in the entertainment sector.[55]

[54] Audience proxies for 1939 are based on the *Cahn-Gus Hill Guide* venue capacity data, assuming two shows per venue per day (audiences are 6,000 people at the 99th percentile). Audience estimates for 2010 are based on Nielsen ratings for TV series and use Pareto extrapolations to the 99th percentile of the show size distribution (audiences at that percentile are 1.2 million people).

[55] I compute the 99th percentile of the entertainer wage distribution based on Census micro data (in 1939) and Forbes celebrity lists (2010). Round number bunching and top-coding makes the 98.5th percentile the closest percentile that can be computed and results are based on this percentile. I use Pareto extrapolations to compute the top percentile wages in 2010 and use data from OES

A growing literature discusses the relation of superstar effects, market power and the fall in the labor share. To link my results to this literature, I estimate a rent sharing equation that captures how workers and employers share the returns from the rising market value of top talent. I implement this by estimating the pass through of rising show revenues of stars to the wages of this group and instrument revenue changes with local television deployment. Since television provided a massive boost to top shows' revenues, the first stage is strong (the corresponding F-statistic is between 28 and 57). The two-stage least squares results shows that one dollar growth in revenue leads to 22 cents higher pay for star workers (Table 8, Panel C).[56] A constant labor share, by contrast, would require that pay grows proportionally to revenues, i.e., an elasticity of 1.[57] The results thus indicate a substantial decline in the labor share.

A possible driver of such effects is the simultaneous rise in superstar firms and monopsony power. In many modern contexts SRTC may be associated with rising monopsony power, since a small number of technology companies control access to such technologies. The entertainment setting offers a unique setting to test this interaction of superstar effects and monopsony power. Government entry restrictions generate quasi-experimental variation in the number of competing local TV stations and thus allow me to identify the impact of labor market competition. The results show a marked difference in the superstar effects in monopsonistic and competitive labor markets. Markets with a single TV station see almost no top income growth, while in markets with competing TV stations top incomes increase sharply. These results also hold when I narrow in on the variation from the rollout interruption experiment. Places where the entry of competing stations is blocked continue to look like monopsony locations (Table 9). These findings emphasize the importance of competition for superstar effects. The growing market scale only translates into rising top pay if employers are competing for talent.

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reports on employment in the entertainment industry in the five relevant occupations (27-2011, 27-2021, 27-2042, 27-2023, 27-2090) to measure total employment.

[56]Note that this estimate is bigger than the elasticity with respect to audience size. This difference arises because the launch of television reduced the cost of top entertainment for consumers, which implies that the first-stage effect on revenues is relatively smaller than the one on audience size. The smaller first-stage effect increases the IV estimate.

[57]Estimates of this elasticity among CEOs range between 0.1 and 1. My IV estimate thus falls into the lower half of this range (Gabaix and Landier 2008; Frydman and Saks 2010).

6 Conclusion

It has been forty years since Sherwin Rosen (1981) presented his elegant superstar theory. In this enormously influential work, Rosen shows how how scale related technological change can serve as a distinctive driving force in the generation of income inequality, particularly at the top end of the income distribution.

This paper provides the first direct test of this theory, using a simple natural experiment, and finds clear evidence that scale related technological change can generate superstar effects, including income concentration at the top. The basis for the test is the increase in the market reach of entertainers that arose during the staggered introduction of television. The launch of a TV station increased audiences of star entertainers roughly threefold and led to sharp income concentration at the top. Income growth escalates as we move up towards the top of the wage distribution, and the share of income going to the top 1% grew roughly 50%. At the same time, the share of entertainers with average incomes declined significantly, and many lesser stars lost their jobs.

In the market I study, the magnitude of superstar effects are substantial—income in the top percentile rises 17% when workers' market reach doubles. At such elasticity rates, superstar effects could explain approximately two-thirds of the top income growth for entertainers in recent decades. Superstar effects thus play an important role in the sector.

The impact of superstar effects is unlikely to be universal across the economy. Superstar effects arise only in sectors where talent is heterogeneous and unique; while we expect superstars to be less important (or not important at all) in settings where individual-level talent is highly substitutable. The production technology thus plays an important role, and not all scale related technologies lead to superstar effects. My findings additionally highlight that the competitive structure of the labor market plays an important role for the magnitude of superstar effects. Future research should therefore explore what other sectors meet the conditions for superstar effects, and quantify how the magnitude of these effects vary across different sectors of the economy.

A broad literature has recognized that a better understanding of superstar effects is important not only from a scientific standpoint, but also for policy decisions. Top earners are one of the main sources of tax revenue, and recent research shows that superstar effects could have substantial effects on the optimal level and progressivity of taxes (Scheuer and Werning 2017). Moreover superstar effects might influence the potential benefits from breaking up economic concentration and may explain economic divergence between regions (Eckert, Ganapati, and Walsh 2019). Finally, it is important to remember the full social welfare implications of SRTC include not only effects on the distribution of income—potentially via superstar effects—but also on the value attached to greater equality in access to goods and services (Acemoglu, Laibson, and List 2014). Scale related technological change is shaping many sectors of the economy, and it is important that we improve our understanding of the far-reaching economic consequences.

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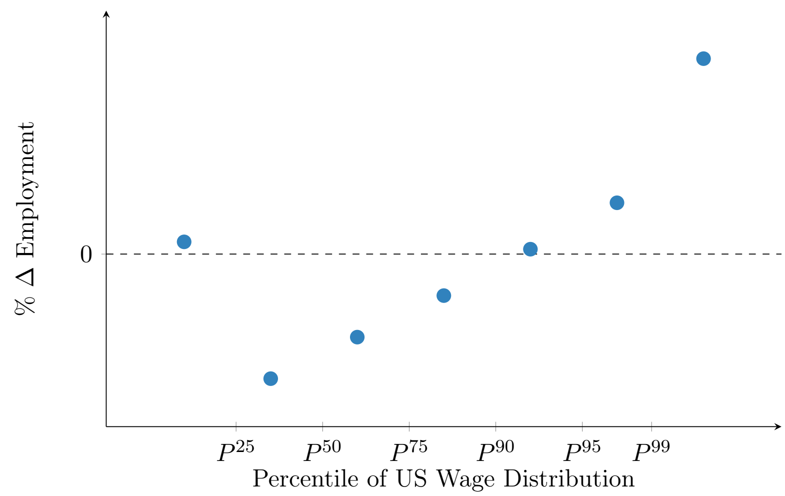
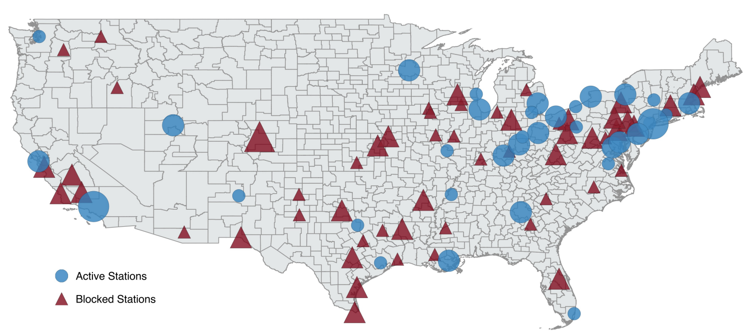
7 Figures

Figure 1: SRTC Effect: Entertainer Employment at Different Wage Levels



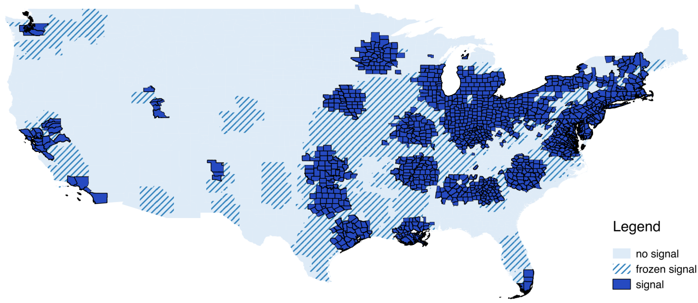
[Notes] The figure shows the impact of SRTC on employment growth at different wage levels. The wage levels are chosen from the US wage distribution to simplify comparison with the empirical results. The figure illustrates first differences of equation 3 with a scale parameter of 1.3 and the intercept of 0.2. Wages outside the range of previous support are grouped with the final bins to avoid undefined growth rates.

Figure 2: Location of Licensed and Blocked TV Stations in 1949



[Notes] Symbols show the location of television filming, and the size of a symbol indicates the number of TV stations per local labor market. Licensed stations are blue circles, and blocked stations red triangles. Source: Television Factbook 1949.

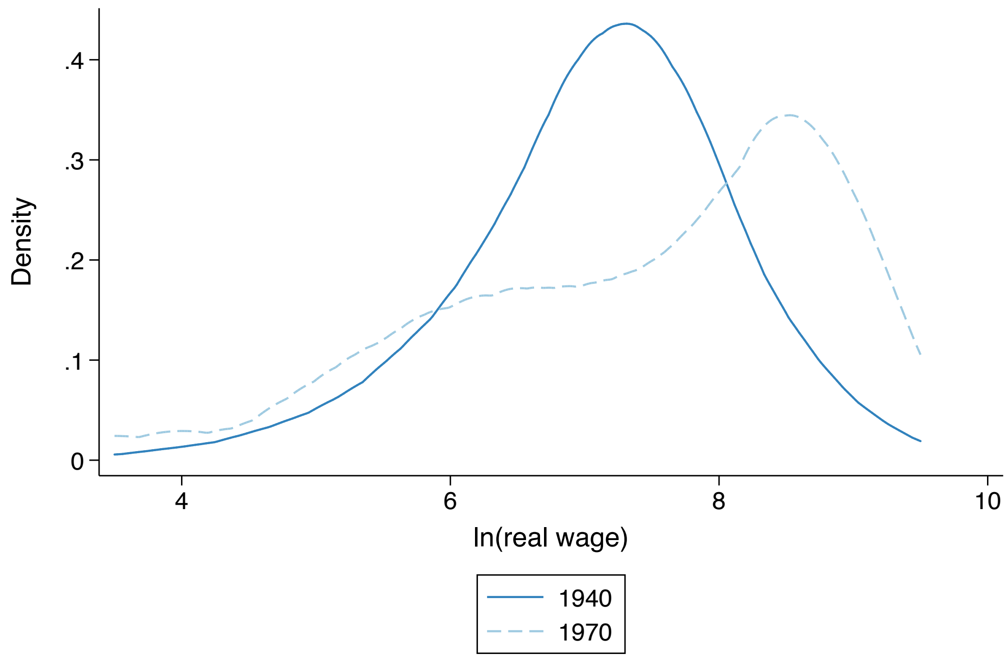
Figure 3: TV Signal of Licensed and Blocked Stations in 1949



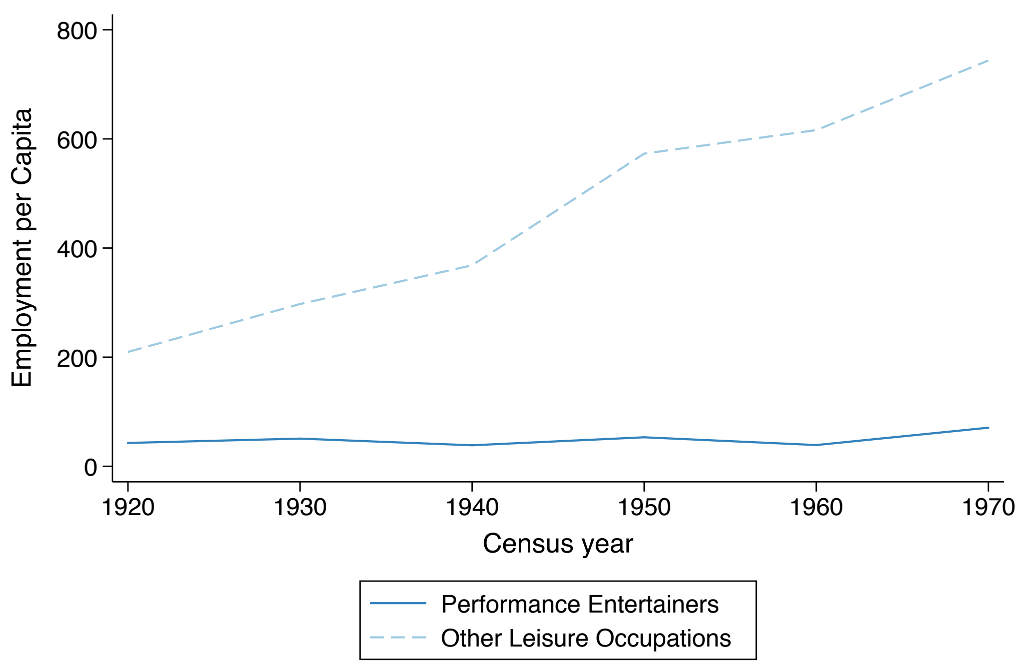
[Note] Areas in dark blue can watch TV, while shaded areas would have had TV signal from blocked TV stations. Signal coverage is calculated using an irregular terrain model (ITM). Technical station data from FCC records, as reported in *TV Digest* 1949, are fed into the model. Signal is defined by a signal threshold of -50 of coverage at 90% of the time at 90% of receivers at the county centroid. Source: Fenton and Koenig (2020).

Figure 4: Change in Entertainment 1940–1970

(a) Entertainer Wage Distribution

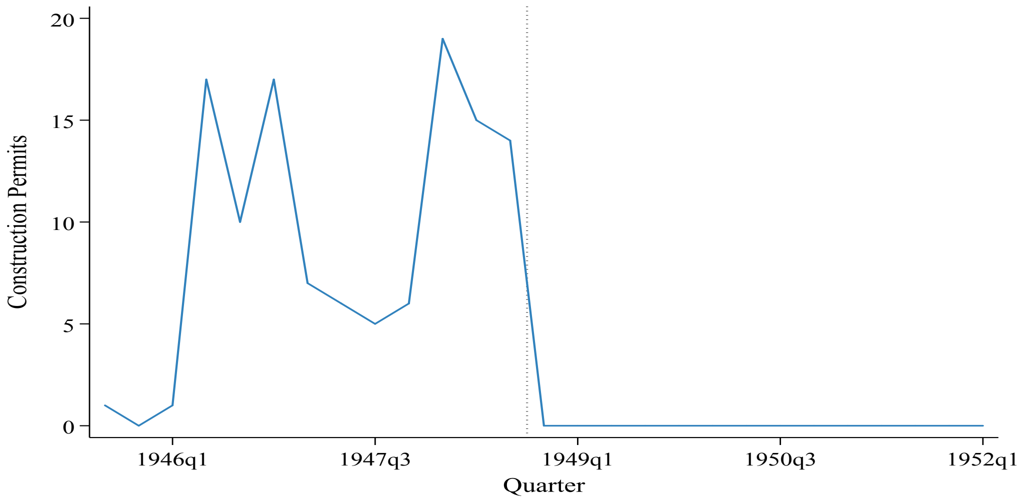


(b) Entertainer per Capita



[Notes] Panel A shows the entertainment log real wage distribution in 1940 and 1970 from the lower 48 states. Dollar values are in 1950 USD. Density is estimated using the Epanechnikov smoothing kernel with a bandwidth of 0.4 and Census sample weights. Common top code applied at $85,000. Panel B shows employment per 100,000 inhabitants of performance entertainers (defined in text) and other leisure-related occupations (bars & restaurants and "other entertainment occupations"). The mean for performance entertainers is 49 and for other leisure occupations 468. Sources: US Population Census.

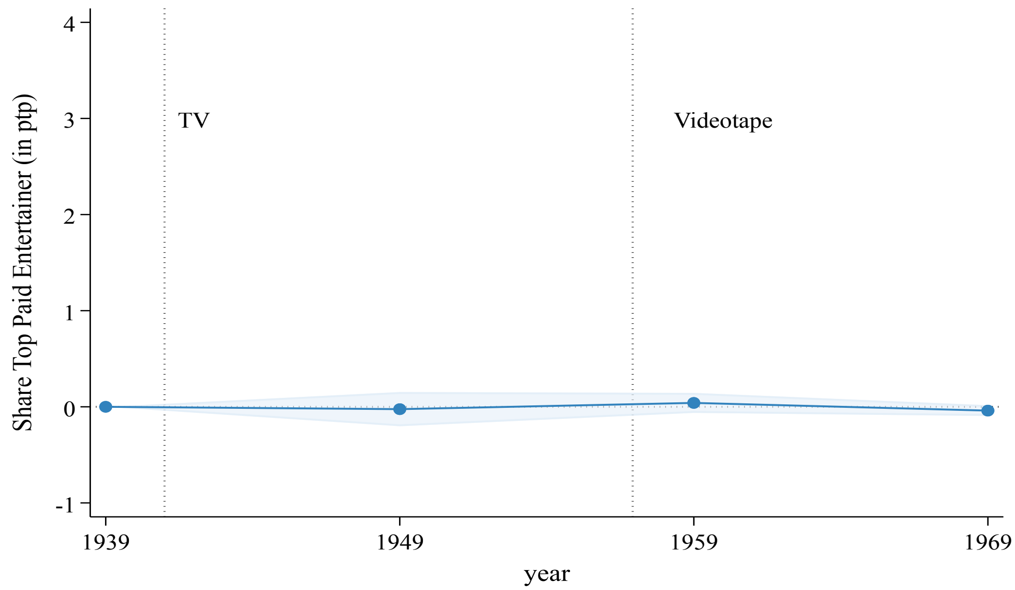
Figure 5: Number of TV Licenses Granted



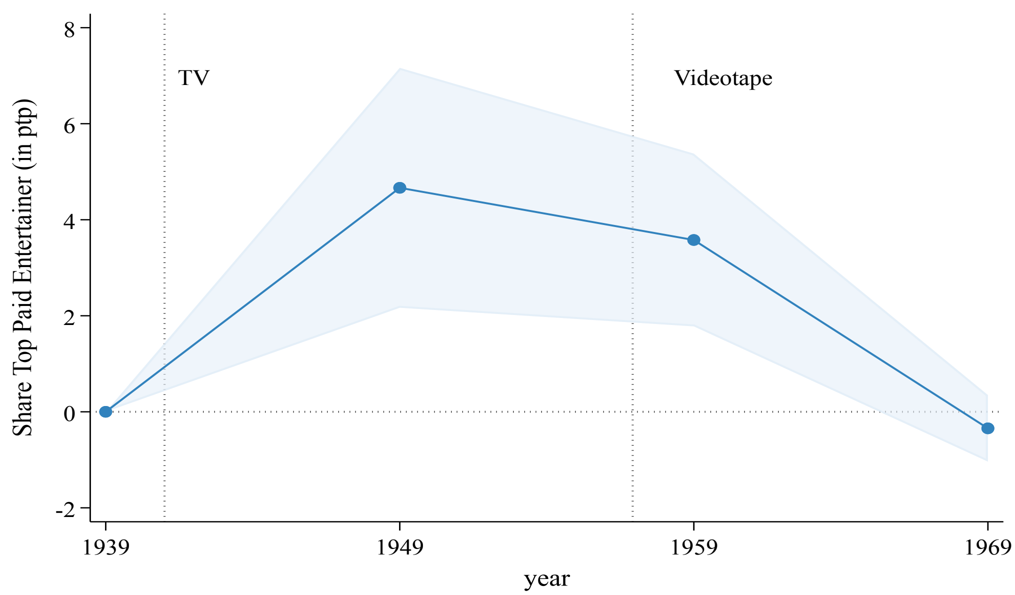
[Note] Missing issue dates of construction permits are inferred from start of operation dates. Source: *TV Digest* 1949.

Figure 6: Dynamic Treatment Effect of TV on

(a) *Blocked TV Stations*

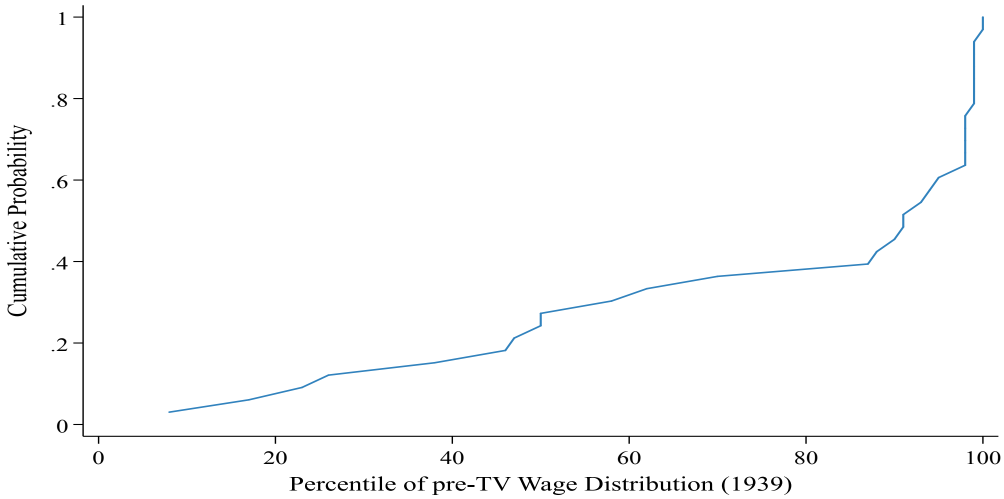


(b) *Licensed TV stations*



[Note] Figure plots treatment coefficients from two DiD regressions. Panel (a) shows the coefficient on $FrozenTV_{m,t}$ (comparison groups are untreated areas) and Panel (b) shows the coefficient on $TV_{m,t}$. Top-paid entertainers are in the top 1% of the US income distribution. Vertical lines labelled *TV* and *Videotape* mark the beginning and end of local TV filming respectively. The area shaded in light blue marks the 95% confidence interval. Standard errors are clustered at the local labor market level.

Figure 7: Position of Future TV Stars in the 1939 US Wage Distribution



[Note] The Figure shows the CDF of wage-distribution ranks of TV stars before they became TV stars. TV stars are defined in the 1949 “Radio and Television Yearbook” These individuals are linked to their 1939 Census wage records. 1939 wages are corrected for age, education, and gender using a regression of log wages on a cubic in age, 12 education dummies, and a gender indicator. Source: See Text.

Figure 8: TV Effect: Entertainer Employment at Different Wage Levels

[Note] Each dot is the treatment effect estimate of a separate DiD regression. It shows a TV station’s effect on entertainer jobs at different parts of the wage distribution. Percentile bins are defined in the overall US wage distribution. Dashes indicate 95% confidence intervals. See Table 1 for details on the specification. Sources: US Census 1940–1970.

8 Tables

Table 1: Effect of TV on Top Earning Entertainers

	(1)	(2)	(3)
	Panel A: Entertainer among Top 1% of US Earners (% of Entertainers)		
Local TV stations	4.14	4.31	5.93
	(1.26)	(1.27)	(2.21)
Increase on baseline	92%	96%	132%
No. of cluster	722	722	722
	Panel B: Entertainer among Top 1% of US Earners (Per Capita in 100,000s)		
Local TV stations	0.40	0.40	0.31
	(0.10)	(0.10)	(0.10)
Increase on baseline	133%	133%	103%
No. of cluster	722	722	722
	Panel C: Rollout Interruption Sample (Outcome as in Panel A)		
Local TV stations	4.63	4.81	6.39
	(1.41)	(1.40)	(2.48)
Increase on baseline	103%	107%	142%
No. of cluster	113	113	113
Year-Occupation FE	Yes	Yes	Yes
CZ FE	Yes	Yes	Yes
Demographics	–	Yes	–
Local labor market trends	–	–	Yes

[Note] The table shows DiD estimates, regressing the respective outcome variables on the number of TV stations in the local area, each cell is a separate regression. Outcomes: Panel A, share of local entertainers among the top 1% of the US income distribution; Panel B, local entertainers among the top 1% of the US income distribution per capita in 10,000s; Panel C, restricts the sample to locations that are either affected by local television filming or blocked by the regulatory interruption, outcomes are as in Panel A. All regressions control for commuting zone (CZ), occupation specific time fixed effects and local filming cost in years after the invention of the videotape. Entertainers are actors, athletes, dancers, entertainers not elsewhere classified, musicians. Column 2 controls for median age & income, % female, % minority, population density, and trends for urban areas. Column 3 controls for a separate linear trend for each CZ. Sample: Panel A & B include 13,718 observations in 722 CZs, 5 occupations over four years, except for the athlete occupation, which is available for three years. The "Rollout Interruption sample" in Panel C restricts the sample to areas that film locally for television or are blocked from receiving television during the rollout interruption and covers 113 CZs and 2,147 CZ–year–occupation observations. Demographic data is missing for one CZ in 1940 and thus reduces the sample in column 2. *Increase on Baseline* reports treatment effects relative to the baseline value of the outcome variable. Observations are weighted by local labor market population. Standard errors are reported in brackets and are clustered at the local labor market level. Sources: US Census 1940–1970.

Table 2: TV and Migration Between Labor Markets

	(1)	(2)	(3)
		Panel A: Share Entertainers who Migrated	
Local TV stations	-0.014	-0.017	-0.010
	(0.015)	(0.015)	(0.020)
		Panel B: Entertainer among Top 1% of US Earners (excl. neighboring areas)	
Local TV stations	4.30	4.46	6.16
	(1.31)	(1.30)	(2.27)
Year–Occupation & CZ FE	Yes	Yes	Yes
Demographics	–	Yes	–
Local labor market trends	–	–	Yes

[Note] The Table tests the effect of local TV launches on entertainer migration. Outcomes: Panel A, the fraction of entertainers who moved; Panel B, share of entertainers among the top 1% of the US wage distribution, excluding labor markets that neighbor treated labor markets. Specification and sample are as in Table 1: 13,718 observations and 722 CZs. The exclusion of CZs in Panel B reduce the sample to 10,792 observations and 568 CZs. Observations are weighted by local labor market population. Standard errors are reported in brackets and are clustered at the local labor market level. Source: US Census 1940-1970.

Table 3: Effects on the 99th Percentile

	(1)	(2)	(3)
	Ln(99th Percentile of Entertainer Wages)		
	Panel A: No Imputation		
Local TV stations	0.177	0.186	0.144
	(0.078)	(0.079)	(0.116)
	Panel B: Fixed Multiple Imputation		
Local TV stations	0.204	0.212	0.163
	(0.085)	(0.086)	(0.123)
	Panel C: Pareto Imputation		
Local TV stations	0.283	0.277	0.237
	(0.095)	(0.089)	(0.131)
Year & CZ FE	Yes	Yes	Yes
Demographics	–	Yes	–
Local labor market trends	–	–	Yes

[Note] The Table tests the effect of local TV launches on entertainer top incomes and uses the quantile DiD estimator developed by Chetverikov, Larsen, and Palmer (2016). Outcome: ln(99th percentile of local entertainer wages) computed at the CZ-year level. The panels differ in how they adjust for top-coding: Panel A makes no adjustments, Panel B uses the fixed multiple approach and multiplies top-coded observations by 1.5, Panel C uses local Pareto approximations. The control variables are as in Table 1. The sample uses the "Rollout Interruption sample" of Table 1 Panel C and covers 113 CZ cluster over 4 years and 452 CZ-year observations. Regression run at the CZ-year level since the 99th percentile cannot be disaggregated by occupation. Missing wage data and cell size restrictions in computing Pareto imputations reduce the observations by 33 in Panel A & B, and by an additional 19 cells in Panel C. Observations are weighted by local labor market population. Standard errors are reported in brackets and are clustered at the local labor market level. Source: US Census 1940-1970.

Table 4: Effect of TV on Entertainer Employment

	(1)	(2)	(3)	(4)
	Ln(Employment in Entertainment)			
		Panel A: Sample 1940—1970		
TV signal$_t$	-0.128	-0.114	-0.134	
	(0.061)	(0.061)	(0.063)	
		Panel B: Sample 1940—1970		
Placebo TV signal$_t$	0.053	0.044	0.053	
	(0.083)	(0.083)	(0.084)	
		Panel C: Sample 1930—1970		
TV signal$_{t+1}$				0.039
				(0.033)
TV signal$_t$	-0.133	-0.127	-0.125	-0.123
	(0.059)	(0.059)	(0.061)	(0.060)
No. of cluster	722	722	722	722
Year–Occupation & CZ FE	Yes	Yes	Yes	Yes
Demographics	-	Yes	-	-
Local labor market trends	-	-	Yes	-

[Note] The table shows the effect of television signal on local entertainer employment. Dependent variable *ln(Employment in Entertainment)* is the inverse hyperbolic sine of employment in entertainment. *TV signal* is a dummy that takes the value 1 if signal is available in a CZ, and *Placebo TV signal* if blocked stations would have brought TV signal. Subscript *t+1* refers to the lead of the treatment variable. Specifications are as described in Table 1, except that demographic controls exclude median income to extend the sample period. Panel A & B include 13,718 CZ-year-occupation observations and Panel C 17,328 observations. Standard errors are reported in brackets and are clustered at the local labor market level. Sources: TV signal from Fenton and Koenig (2020) and labor market data from US Census 1930–1970.

Table 5: Effect of TV on Spending at Local County Fairs

	(1) *Ln(Fair Visits)*	(2) *Ln(Entry Ticket Receipts)*	(3) *Ln(Grandstand Show Receipts)*	(4) *Ln(Carnival Receipts)*
		Panel A: Local Labor Market Level		
TV channels	-0.051	-0.047	-0.059	0.014
	(0.031)	(0.024)	(0.022)	(0.022)
No. of cluster	722	722	722	722
Year & CZ FE	Yes	Yes	Yes	Yes
		Panel B: County Level		
TV channels	-0.013	-0.014	-0.018	0.001
	(0.010)	(0.007)	(0.007)	(0.006)
No. of cluster	3,111	3,111	3,111	3,111
Year & county FE	Yes	Yes	Yes	Yes

[Note] The table shows the effect of television signal on attendance and revenues at local county fairs. Outcomes: Sum of results among county fairs in location m in year t from 1946 to 1957. All variables use the the inverse hyperbolic sine transformation to approximate the log function, while preserving 0s. Monetary variables are converted to 1945 US Dollars. Treatment is the number of TV stations that can be watched in the CZ. Data on carnival receipts (column 4) are unavailable for 1953 and 1955. Panel A uses 8,664 CZ-year observations (7,220 in column 4), while Panel B uses 37,332 county-year observations (31,110 in column 4). Standard errors, reported in brackets, are clustered at the local labor market level in Panel A and at the county level in Panel B. Source: *Billboard Cavalcade of Fairs,* 1946–1957 and Fenton and Koenig (2020).

Table 6: Effect of TV on Top Income Shares in Entertainment

	(1)	(2)	(3)
	Ln(Share of Income)		
	Top 0.1%	Top 1%	Top 10%
	Panel A: Full Sample		
Local TV stations	0.68	0.45	0.23
	(0.19)	(0.12)	(0.06)
P-value: same growth as top 1% share	0.02	—	0.00
	Panel B: Rollout Interruption Sample		
Local TV stations	0.47	0.32	0.16
	(0.20)	(0.14)	(0.07)
P-value: same growth as top 1% share	0.24	—	0.00
Year & CZ FE	Yes	Yes	Yes

[Note] The table shows the effect of local TV stations on top income shares in entertainment. Outcomes: The top p% is the share of income going to the top p percent of entertainers in a given local labor market–year. Estimates are based on a DiD specification across CZ-year cells. In Panel A top income shares are calculated using local Pareto approximations in all CZ-year cells with at least 20 entertainers. This leads to a sample of 1,061 CZ-year observations and 346 CZ cluster, while Panel B uses the "rollout interruption sample," as in Table 3 Panel C. *P-value* refers to a test of equal growth rates in top income shares, which is implemented in a regression with the ratio of top income shares as outcome variable. Observations are weighted by cell-size. Standard errors are clustered at the local labor market (CZ) level. Sources: US Census 1940–1970.

Table 7: Effect of TV on Market Reach of Local Stars

	(1)	(2)	(3)
		Panel A: Ln(Show Audience)	
Local TV stations	1.04	1.08	0.74
	(0.18)	(0.21)	(0.14)
Increase on baseline	184%	194%	109%
		Panel B: Ln(Show Revenue)	
Local TV stations	0.74	0.79	0.74
	(0.10)	(0.13)	(0.14)
Increase on baseline	109%	120%	109%
No. of cluster	686	686	686
Year & CZ FE	Yes	Yes	Yes
Demographics	–	Yes	-
Local labor market trends	–	–	Yes

[Note] The table shows the effect of local TV stations on the audience and revenues of top entertainment shows. Each table cell reports results from separate DiD regressions across local labor markets and the first stage to Table 8. Outcome: Panel A, potential show audience of the largest show in the commuting zone, computed from venue seating capacity and TV households in transmission area; Panel B, potential revenue of largest show. For details on the sample, control variables and weights see the IV regression in Table 8. Sources: See text.

Table 8: Elasticity of Top Entertainer Wages to Market Reach

	(1)	(2)	(3)
	$Ln(99^{th}\ Percentile\ of\ Entertainer\ Wages)$		
	Panel A: Repeated Cross-Section OLS		
ln(Audience size)	0.232	0.049	
	(0.029)	(0.019)	
	Panel B: IV		
ln(Audience size)	0.166	0.149	0.149
	(0.017)	(0.019)	(0.024)
First-stage F-statistic	33.3	25.7	20.0
	Panel C: IV		
ln(Value of market ($))	0.220	0.192	0.198
	(0.028)	(0.022)	(0.036)
First-stage F-statistic	57.10	38.1	28.7
Demographics	–	Yes	–
Local labor market trends	–	–	Yes

[Note] The table shows the effect of market reach on the income of top entertainers. Outcomes: The entertainer wage at the 99th percentile in a CZ year cell, as defined in Panel A of Table 3. Panel A uses an OLS regression that treats the data as repeated cross-sections and controls for year fixed. Panels B and C use an IV regression that uses the TV roll-out as instrument and control for year and CZ fixed effects. The sample are all cells where the relevant first- and second-stage variables are available: nbr of observations are 2,148 and 686 CZ cluster. The control variables are the same as in Table 1 and observations are weighted by cell-size. The first-stage F-statistic is the Kleibergen-Paap F-statistic that allows for non-iid standard errors. Standard errors are clustered at the local labor market level. Sources: See Table 3 and 7.

Table 9: Effect of Competition in Local Labor Markets

	(1)	(2)	(3)
	Entertainer among Top 1% of US Earners		
Local TV station (dummy)	5.90	0.75	-0.57
	(3.06)	(1.91)	(0.36)
Multiple local TV stations (dummy)		9.07	10.37
		(4.99)	(4.70)
Blocked competitor (dummy)			1.43
			(2.10)
No. of cluster	722	722	722
Year–Occupation & CZ FE	Yes	Yes	Yes

[Note] The table shows the effect of competition between local TV stations. The regressors are a dummy with value one, respectively if a location has a TV station (Local TV station), a location has multiple TV stations (Multiple local TV stations) and a location has the entry of a second station blocked by the rollout interruption (Blocked competitor). For other specification details and sources see Table 1, Panel B.

ONLINE APPENDIX

A APPENDIX: Derivations

A.1 Equilibrium of the Superstar Model

Each firm maximizes profits by hiring a worker with talent t, taking its own firm characteristic as given. The firm problem is therefore given by

$$max_t Y(s_i, t) - w(t),$$

where $w(t)$ is the wage for a worker with talent t. The equilibrium is characterized by the incentive compatibility condition, the participation condition, the assignment function of workers to firms, and market clearing.

The optimal assignment $\sigma(S_i) = t$ matches the best actor with the biggest theater. This PAM results follows from the comparative advantage assumption $\frac{\partial Y}{\partial t \partial S} > 0$, which implies better actors have a comparative advantage in bigger theaters. PAM guarantees that the percentiles of talent and size distribution are the same for a matched pair $p_s = p_t$. Since the equilibrium is competitive, the optimal assignment is also the market outcome and hence the first equilibrium condition.

Incentive compatibility guarantees that for each firm i the optimal worker p meets,

$$Y(s_i, t) - w(t) \geq Y(s_i, t') - w(t') \quad \forall \;\; t' \epsilon [\underline{t}, \overline{t}]. \tag{8}$$

The number of incentive compatibility (IC) constraints can be reduced substantially. If the IC holds for the adjacent t' all the other ICs will hold as well. We can therefore focus on the percentiles just above and below t. The IC for the adjacent $t' = t + \epsilon$ can be further simplified if Y is differentiable in t. Divide equation 8 by ϵ and let $\epsilon \rightarrow 0$.

$$\frac{w(t) - w(t+\epsilon)}{\epsilon} \leq \frac{Y(s_i, t) - Y(s_i, t+\epsilon)}{\epsilon}$$

$$\frac{\partial w}{\partial t} = \frac{\partial Y(S_i, t)}{\partial t}. \tag{9}$$

The IC condition can thus be written as a condition on the slope of the wage schedule and proves the IC condition in the text.

I extend the model and allow for entry and exit. This gives rise to a fourth equilibrium object, the participation threshold $\bar{p}$, which is defined by the participation constraints (PC). Denote the reservation wage of workers $w^{res}$and the reservation profits ψ^{res} and hence the PC condition is

$$Y(s_i, t) - w(p) \geq \psi^{res} \quad \forall \; p\epsilon[\bar{p}, 1] \tag{10}$$

$$w(p) \geq w^{res} \quad \forall \; p\epsilon[\bar{p}, 1]. \tag{11}$$

The marginal participant is indifferent between participating and hence the PC binds with equality: $w(\bar{p}) = w^{res}$ and $Y_i(\bar{p}) - w(\bar{p}) = \psi^{res}$. Individuals with lower levels of skill will work in an outside market where pay is independent of talent and given by w^{res}.

Finally, talent prices will clear the market. In equilibrium revenues equal total expenditure, denoted by $D(\pi)$. Summing over all firms, we can derive the total supply in the economy: $S(\pi) = \int^{\bar{p}} h'(t)Y(\sigma(t), t)dt$. Supply is increasing in π (since $\frac{\partial \bar{p}}{\partial \pi} < 0$), hence there is a unique market clearing price $\hat{\pi}$, as long as demand is downward sloping $D'(\pi) < 0$. The economy therefore has a unique equilibrium.

Using the functional form assumptions in the text, we can rewrite 9 as

$$\frac{\partial w}{\partial t} = \frac{\pi}{\phi} s^{\frac{1}{\phi}} t^{\frac{1}{\phi}-1} = \frac{\pi}{\phi} t^{1/\beta\xi - 1},$$

where $\xi = \frac{\alpha\phi}{\alpha+\beta}$ the last equality uses the size distribution and $p_s = p_t$. Integrating and normalizing $w(\underline{t}) = 0$ gives the wage:

$$w(t) = \int_{\underline{t}}^{t} \frac{\partial w}{\partial t} = \pi \frac{\beta\xi}{\phi} t^{-1/\beta\xi} = \pi \frac{\beta\xi}{\phi} p^{-1/\xi}. \tag{12}$$

A.2 Technological Change and Superstar Effects

This section derives the four parts of the Proposition in the text.

Part a. Compare the employment share that pays above ω (denoted by $ln(p^{\omega})$) before and after SRTC by evaluating equation 3 at the two values of $\phi, \tilde{\phi}$ respectively before and after SRTC:

$$\Delta ln(p^{\omega}) = \tilde{\gamma_0} - \gamma_0 + \gamma_1^{\omega}(\phi - \tilde{\phi}).$$

This captures the change in $(ln(p^{\omega}))$. When $\omega \to \infty$, then $\gamma_1^{\omega} \to \infty$ and since SRTC implies $\phi > \tilde{\phi}$, this implies that the right hand side is positive. SRTC therefore produces a growing fraction of highly paid workers. This effect is bigger at higher income levels since γ_1^ω increases in ω and this concludes Proposition (a).

Part b. The top income share is defined as the sum of incomes of individuals in the top percentile ranks p divided by total income (G):

$$s_p = \int_0^p w_j dj/G. \tag{13}$$

For a Pareto distribution the top income share is given by $s_p = (1-p)^{1-\psi}$, with ψ^{-1} the shape parameter of the distribution. Notice that equation 12 implies that the wage distribution here follows a Pareto distribution.[58] We can thus evaluate the impact of SRTC by considering a decrease of scale costs by factor κ: $\phi' = \kappa\phi < \phi$. Using 13, the growth in the top income share is

$$g^{s_p} = \frac{s_p^{t+1}}{s_p^t} = \frac{(1-p)^{1-\kappa\frac{\xi}{\beta}}}{(1-p)^{1-\frac{\xi}{\beta}}} = (1-p)^{-(\kappa-1)\frac{\xi}{\beta}}.$$

The second step uses the property of a Pareto variable described above and the final equality collects terms. Since $\kappa < 1$, the exponent is positive and top income shares thus grow. The expression also implies that the growth is largest towards the top of the distribution (where p is small). In other words, the income share of the top 0.1% grows faster than that of the share that goes to the top 1%, which in turn growths faster than the share of the top 10%.

The Pareto distribution simplifies the previous result, but the general pattern holds more broadly. As it becomes feasible to serve bigger markets, the wage-talent profile pivots and becomes steeper. We can show this for the general case by differentiating equation 9 with respect to s:

$$w_{pS}(t^*) = Y_{pS}(t^*) + Y_{pp}(t^*)\frac{\partial t}{\partial s} = \frac{w''(t^*)}{\sigma'(t^*)} > 0. \tag{14}$$

Which shows that wages grow more quickly with talent when s increases. The second equality uses positive assortative matching to invert the assignment function $t^* = \sigma^{-1}(S)$ and differentiates to yield $\frac{\partial t}{\partial s} = \frac{1}{\sigma'(t)}$. We can sign the resulting expression because the wage schedule in the superstar economy is convex ($w''(t) < 0$) – see Rosen's 1981 article for a discussion of this result – and because positive assortative

[58] The top income share equation approximately holds for a broader class of distribution. For variables that do not follow a Pareto distribution, there is still a value ψ_p that satisfies the equation, but it now varies with p. For many distributions ψ_p varies only slowly and the result thus holds approximately.

matching implies $\sigma'(t) < 0$. Note that we do not use the envelope theorem here, this theorem does not hold in the context of assignment models. We can, however, still sign the impact of market reach as long as the assignment function is invertible.

Part c. Define a mid-income workers as having a wage between w & w' and denote the share of mid-paid entertainers by M. This share can be derived by rearranging equation 12:

$$M = p(w) - p(w') = (\frac{\beta\pi}{\alpha+\beta})^{\xi}[w^{-\xi} - w'^{-\xi}].$$

Differentiating with respect to ϕ gives the impact of SRTC: $\partial M/\partial\phi = -\varepsilon_D\kappa_1 + \partial M/\partial\xi$, where ε_D is the elasticity of inverse demand and $\kappa_1 = \frac{\xi}{\phi}(\frac{\beta\pi}{\alpha+\beta})^{\xi}$. Mid-income jobs will decline when $\partial M/\partial\phi < 0$, which occurs when demand is sufficiently inelastic (i.e., if the elasticity of the inverse demand curve is $\varepsilon_D > \partial M/\partial\xi/\kappa_1$).[59] Note, however, that the previous equation only holds for wages that are in the support of the income distribution both before and after SRTC. Given that the wage distribution spreads out with SRTC, we may reach wage levels that were previously unattained and thus violate this condition. In such wage ranges, the growth rate is undefined. The share of entertainers in the baseline period is 0 and to compute a growth rate we would have to divide by 0. To get around this, I group newly emerging pay ranges together with the nearest wage that occurred before SRTC. In that case, employment shares at the extremes of the distribution increase unambiguously, and as a result we may see growth in low-paid employment.

Part d. In the model with entry and exit the participation constraint (PC) ensures that the marginal participant ($\bar{p}$) is indifferent between working and the outside option (w^{res}) and the marginal employer breaks even:

$$w(\bar{p}) = w^{res},$$

$$Y(\sigma(\bar{p}), \bar{p}) = w(\bar{p}).$$

A period of SRTC is such case that decreases $Y(\sigma(\bar{p}), \bar{p})$ by reducing π. To reach

[59]Notice that if M declines for an income range w to w', it will also decline for all lower income ranges. This follows since $\partial M/\partial\xi/\kappa_1$ is larger at higher values of w and therefore the elasticity condition will hold for lower wage ranges if it holds at M. The result that $\partial M/\partial\xi/\kappa_1$ increases with income follows because κ_1 increases with income at a rate proportional to $[w^{-\xi} - w'^{-\xi}]$, while $\partial M/\partial\xi$ increases at a faster rate, proportional to $[w^{-\xi} - w'^{-\xi}] + [w^{-\xi}(ln(w) - 1) - w'^{-\xi}(ln(w') - 1)] > [w^{-\xi} - w'^{-\xi}]$.

equilibrium, $\bar{p}$ has to adjust. Recall that low p implies a high level of talent and hence $dY(\sigma(\bar{p}), \bar{p})/d\bar{p} < 0$. The SRTC induced fall in Y therefor results in a in lower $\bar{p}$, which confirms Proposition (d).

B APPENDIX: Empirics

B.1 Summary Statistics

Table B1 reports summary statistics for the baseline local labor market sample. This covers the 722 local labor markets for four Censuses (1940-1970), and thus 2,888 observations. The first set of results report statistics on the availability of television. The table reports averages for the full sample period. Since local filming only took place for a relatively short time period, the variable is zero in most years and the average number of TV stations is 0.02. At the time of local filming in 1949, filming occurred in around 5% of local labor markets through on average 1.78 stations. TV signal covers 60% of locations on average and signal coverage expands from no signal in 1939 to full coverage in 1969. The suitability of a location for filming is summarized by "local filming cost," and the data show the strong pull to concentrate filming when location decisions are unconstrained. The proxy for local comparative advantage is the number of movie productions in this local labor market in 1920. Most places had no movie sets, and only 16 locations produced at least 1 movie, with only LA producing more than 20 films. The average audience entertainers could attain was 72 million individuals. This is however skewed by the huge audiences in the national TV era. Before national TV, the average market reach is 62,000 individuals in 1949, while theater capacity of the pre-TV era only ranges from 400 to 12,000 individuals. Data on theater capacity is missing for 116 local labor markets, 16% of the sample.

Turning to entertainers, the average local labor market employs 177 performance entertainers during the sample period but there is again considerable heterogeneity across local labor markets (see demographics). Most important in the analysis are the local labor markets where TV filming took place, which have on average a little over 2,000 performance entertainers. Employment in all other leisure-related activities (i.e., including in bars and restaurants and in interactive leisure activities) is about 2,500 individuals in an average local labor market. The 99th percentile of the entertainer wage distribution averages close to $5,700. Data on county fairs reports average attendance and spending in three categories: entrance tickets, shows, and rides and carnival purchases (e.g., candy, popcorn). These data show that county fairs are a popular event, with the average fair attracting about 25,000 visitors. These data are available at higher frequency and spans over 8,000 local labor market–year observations. Finally, the table reports demographic information on the population in the local labor markets. The average local labor market has 229,000 inhabitants and 86,000 workers, earning on average $1,698. Median income is missing for one observation.

B.2 Robustness checks

B.2.1 Placebo Occupations

Since television only changed the production function of a handful of occupations, we can therefore use selected alternative occupations as placebo groups. An ideal placebo group will pick up changes in top income in the local economy and I use the main high pay occupations as placebo groups (i.e., medics, engineers, managers and service professionals). If TV assignment is indeed orthogonal to local labor market conditions, we would expect that such placebo occupations would be unaffected. Results for the placebo group are reported in Table B2. The point estimates are close to zero. In the entertainment sector, the share of workers with pay in the 99th percentile roughly doubled with the launch of television, and comparable numbers in the placebo occupations are an order of magnitude smaller and in no specification do they exceed 20 percent. The fact that placebo occupations do not show large top income growth around the time of TV launches provides further evidence that the results are identifying the causal effect of television.

We can combine placebo and entertainment occupations to run a triple difference analysis. In a first step I pool placebo and entertainment occupations and allow a TV station launch to have different effects on the two groups. Results show that only entertainers benefit from the TV launch (Table B3, Column 1). The estimated effect on performance entertainers remains similar to the baseline DiD regression. Column 2 allows for a separate impact of television for each occupation of the placebo occupations, which shows that entertainers are indeed different from all other placebo occupations. Finally, I run the full triple difference regression. In this regression, the treatment varies at the time, labor market, and occupation level, which allows me to control for pairwise interactions of time, market, and occupation fixed effects and thus capture local demand shocks that happen to coincide with TV launches. An example where this might be necessary is if improved local credit conditions result in greater demand for premium entertainment and simultaneously lead to the launch of a new TV channel. Such shocks could lead to an upward bias in the estimates of a DiD set up but will now be captured by the location-specific time effects.

Column 3 shows the results. The effect on performance entertainers remains close to the baseline estimate. The additional location-specific time and occupation fixed effects therefore don't seem to change the findings. This rules out a large number of potential confounders. The introduction of a "superstar technology" thus has a large causal effect on top incomes, and this effect is unique to the treated group.

B.2.2 Pre-Trend

A challenge for estimating pre-trends with this sample is that wage data in the Census is first collected in 1940. Since the Census is decennial this only allows for a single pre-treatment period. To estimate pre-trends I therefore combine the Census data with data from IRS tax return data. In 1916 the IRS published aggregate information on top earners by occupation-state bins, including data for actors and athletes. I link the Census data with the tax data and run the regressions at the state level. Table B4 reports the results. Column 1 repeats the baseline estimate with data aggregated at the state level. Despite the aggregation at the state level the effect remains highly significant. Column 2 adds the additional 1916 data from the IRS. The results stay unchanged. Column 3 shows the differences in top earners in the treatment and control groups for the various years. The results show a clear spike in 1950, the year of local television filming. Looking at pre-trends, there is no significant pre-trend, in part because the standard errors are large. If anything, the treated areas seem to be on a slight relative downward trend in the pre-period, in line with the well known aggregate decline of top incomes during the 1930s. Even if we take this insignificant trend at face value, the pre-trends could go in the opposite direction and cannot explain the identified positive effect of TV launches.

B.2.3 Top Income Metrics

The baseline outcome variable normalizes the number of top earners by aggregate employment in entertainment. This has the convenient effect that the result is a percentage change. As the numerator doesn't vary at the local labor market level, changes in this variable should therefore be captured by the year fixed effect. We may worry, however, that since the variable enters multiplicatively, the additive year fixed effect does not completely control for changes in the denominator. In Panel A of Table B5 I therefore rerun the baseline regression using the count of top earners as outcome. In an average labor market 16 individuals are in the top percentile. A TV launch almost triples the number of top earners. This aligns with the baseline results and confirms that the normalization has no substantive effect on the result. Panel B uses the same normalization for all observations and again shows consistent results. Finally, Panel C uses a different top income metric and considers what fraction of top earners are entertainers. This now considers the frequency of entertainers in the pool of top earners and again we find similar results.

B.2.4 Sample Definition and Control Group

This section reports robustness checks that exclude smaller CZs. A potential concern is that less populated cells will lead to biased estimates. The baseline results already address this by weighting areas by their population and thus putting a smaller weight on small cells. I report additional robustness checks here that exclude different labor markets from the analysis. Since most of the low population areas do not receive television, they do not contribute materially to the identification of the superstar effects. They do still help to identify other parameters and thus increase the power of the specifications. As a result, excluding small control areas does not substantially affect the estimates but does affect the power of the estimates.

Table B6 repeats the estimates of Table 3 for alternative samples. The first column is an event study of areas where local television takes place and excludes all control areas. The estimates are very similar to the baseline and confirm that we are indeed picking up effects in the treated areas, rather than spurious changes in control areas. Since the sample is reduced substantially here, the standard errors are large. The second column introduces control areas, specifically this specification includes areas that narrowly miss out on television during the interruption and neighbors to television areas. The specification thus repeats the specification reported in Table 3 and adds neighbors of filming areas as control areas. The next columns step-by-step broaden the control group. The sample in the third column are the 25% most populated CZs. Column 4 uses a balanced panel of all areas where we have at least four observations in all years. And finally Column 5 is the unbalanced panel version of this. Power increases as we increase the sample size and the most precise estimates are reported in Column 5. Since the point estimates are very stable throughout, this adds confidence that we are indeed identifying the effects of interest.

Table B7 repeats the estimates in Table 8 in the narrower sample that uses only places as control group where television was held up by the regulator interruption, the "interruption sample". Table B8 additionally uses Pareto imputations for top-coded observations. Results are similar to the main text.

B.3 Data construction

B.3.1 Local labor markets

The analysis defines a local labor market as a commuting zone (CZ). A labor market comprises an urban center and the surrounding belt of commuters. The CZs fully cover the mainland US. The regions are delineated by minimizing flows across boundaries and maximizing flows within labor markets, and are therefore constructed to yield strong within-labor-market commuting and weak across-labor-market commuting. David Dorn provides crosswalks of Census geographic identifiers to CZs (Autor and Dorn 2013). I use these crosswalks for the 1950 and 1970 data and build additional crosswalks for the remaining years. For each Census, I use historical maps for the smallest available location breakdown. I map the publicly available Census location identifiers into a CZ. No crosswalk is available for the 1960 geographic Census identifier in the 5% sample and the 1940 Census data. Recent data restoration allows for more detailed location identification than was previously possible, using mini public use microdata areas (mini-PUMAs). To crosswalk the 1940 data, I use maps that define boundaries of the identified areas. In geographic information system (GIS) software I compute the overlap of 1940 counties and 1990 CZs. In most cases counties fall into a single CZ. A handful of counties are split between CZs. For cases where more than 3% of the area falls into another CZ, I construct a weight that assigns an observation to both CZs. The two observations are given weights so that together they count as a single observation. The weight is the share of the county's area falling into the CZ. The same procedure is followed for 1960 mini-PUMAs. Carson City County (ICSPR 650510) poses a problem. This county emerges only in 1969 as a merger of Ormsby County and Carson City, but observations in IPUMS are already assigned to this county in 1940. I assign them to Ormsby County (650250). CZ 28602 has no employed individual in the complete count data in 1940.

B.3.2 Worker data

Data is provided by the Integrated Public Use Microdata Files (IPUMS, Ruggles et al. 2017) of the US decennial Census from 1930 to 1970 (excluding Hawaii and Alaska). Prior to 1930, the Census used a significantly different definition of employed workers than in my period of interest, and from 1980 onwards the Census uses different occupation groups. This limits the potential to expand the sample. During the sample period most variables remain unchanged, and where changes occurred, IPUMS has aimed to provide consistent measures. For each of the years, I use the largest publicly available sample with granular spatial data; before 1950, data on the full population is available, and I use samples for recent years. In 1970 the biggest available dataset combines data from Form 1 and Form 2 metro samples.

- There are 722 CZs covering the mainland USA. These regions are consistently defined over time.
- There are 37 relevant occupations. 1950 occupation codes are
  - Treatment group: 1, 5, 31, 51, 57
  - High income placebo group: 0, 32, 41, 42, 43, 44, 45, 46, 47, 48, 49, 55, 73, 75, 82, 200, 201, 204, 205, 230, 280, 290, 480
  - Workers in other leisure activity placebo group: 4, 6, 77, 91, 732, 750, 754, 760, 784.
- Aggregates are calculated using the provided sample weights.
- Variables used: incwage, occ1950 (in combination with empstat), wkswork2, hrswork2.
- To match TV signal exposure to the Census, I map county-level TV signal information onto geographic units available in the Census. The geographic match uses the boundary shapefiles provided by the National Historical Geographic Information System (NHGIS) (Manson et al. 2017). I then identify how many TV-owning households are in each TV station's catchment area. This allows me to construct a measure of potential audience size.

B.3.3 Employment

Number of workers are based on labforce and empstat. Both variables are consistently available for those aged 16 years and older. Hence the sample is restricted to that age group. Occupation is recorded for ages older than 14. I use this information for all employed. This is available consistently, with the exception of institutional inmates, who are excluded until 1960. The magnitude of this change is small and the time fixed effect will absorb the effect on the overall level of employment. The definition of employment changes after the 1930 Census. Before the change, the data doesn't distinguish between employment and unemployment. In the baseline analysis I therefore focus on the period from 1940 onwards. For this period the change doesn't pose a problem. An alternative approach is to build a harmonized variable for a longer period that includes the unemployed in the employment count for all years. I build this alternative variable and perform robustness checks with it. The results remain similar. For two reasons the impact of this change on the results is smaller than one might first think. First, most unemployed people do not report an occupation and thus do not fall into the sample of interest.[60] Second, the rate of unemployment is modest compared to that of employment and thus including the unemployed does not dramatically change the numbers.

I use the IPUMS 1950 occupation classification (Occ1950). This data is available for years 1940–1970. For previous years, the data is constructed using IPUMS methodology from the original occupation classification. Occupational definitions change over time. IPUMS provides a detailed methodology to achieve close matches across various vintages of the US Census. Luckily the occupations used in this analysis are little affected by changes over time. More details on the changes and how they have been dealt with are as follows: The pre-1950 samples use an occupation system that IPUMS judges to be almost equivalent. For those samples IPUMS states that as: "the 1940 was very similar to 1950, incorporating these two years into OCC1950 required very little judgment on our part. With the exception of a small number of cases in the 1910 data, the pre1940 samples already contained OCC1950, as described above." For the majority of years and occupations IPUMS therefore relies on the raw data. There are, however, a few changes that do affect the occupation classifications:

- *Changes for the 1950–1960 period:* Actors (1950 employment count in terms of 1950 code: 14,921 and in terms of 1960 code: 14,721), all other entertainment professions are unaffected. Among the placebo occupations, a few new

[60] The unemployed may report an occupation if they have previously worked. I construct an alternative employment series that includes such workers for the entire sample period. This measure is a noisy version of employment as some job losers continue to count as employed. Since the share of these workers is small, the correction has only small effects on the results.

occupations categories are introduced in 1950.

- Changes *for the 1960–1970 period:* Pre-1970 teachers in music and dancing were paired with musicians and dancers. In 1970 teachers become a separate category. My analysis excludes teachers and thus is unaffected by this change. The athletes category is discontinued in 1970 and the analysis therefore only uses this occupation until 1960. For the “Entertainers nec” category roughly 9,000 workers that were previously categorized as “professional technical and kindred workers” are added along with a few workers from other categories in 1970. These added workers account for roughly 40% of the new occupation group. The occupation-specific year effect ought to absorb this change. I have performed additional robustness checks excluding 1970 or occupation groups and find similar results and the results are robust to this. Among placebo occupations, the “floor men” category is discontinued in 1970.

The industry classification also changes over time. The analysis uses the industry variable to eliminate teachers from the occupations "Musicians and music teacher" and "Dancers and dance teachers." The Census documentation does not note any change to the definition of education services over the sample period; however, the scope of the variable fluctuates substantially over time. From 1930 to 1940, the employment falls from around 70,000 to 20,000; from 1950 to 1960, it increases to around 200,000; and from 1960 to 1970, it falls back to around 90,000.

B.3.4 Wage data

The wage data is collected in the US population Census, and refers to wages in the previous calendar year. This data is first available in the 1940 Census. And in 1950 the income questions are only filled in by a subset of “sample-line” individuals. The IPUMS extracts are mostly sampled from these sample-line individuals and hence wage data is largely available. I convert the wage variables to real 1950 USD. The top-code bites above the 99th percentile of the US wage distribution in all years and we can therefore compute the share of workers in the top percentile. I calculate measures for top income dispersion in entertainment for each market by year. Some measures (e.g., income dispersion) are not additive across occupations and to calculate those, I pool the entertainer micro data and calculate a single dispersion coefficient per year–local labor market.

B.3.5 Pareto Interpolation

Extrapolation is required to compute top income shares in local labor markets and information beyond the top code. The literature has used Pareto approximations to approximate the top tail of the income distribution (e.g., Kuznets and Jenks 1953; Atkinson, Piketty, and Saez 2011; Atkinson and Piketty 2010; Blanchet, Fournier, and Piketty 2017; Piketty and Saez 2003; Feenberg and Poterba 1993). If wages are Pareto distributed the distribution is pinned down by two parameters, the "Pareto coefficient" and the scale parameter. The cumulative distribution function of a Pareto distribution is: $1 - F(w) = (w/\omega)^{-1/\alpha}$, which is linear in logs. And the expected income for a person with top-coded income $\bar{y}$ is $E(y) = \frac{\alpha}{\alpha-1}\bar{y}$. For a top-coded observation, we can thus compute the expected income: it is k times the top-code and k is pinned down by the Pareto coefficient of the income distribution. The shape parameter conventionally used for the US income distribution is around $\alpha = 3$ and hence $k = 1.5$ (see e.g., Juhn, Murphy, and Pierce 1993; Lemieux 2006; Autor, Katz, and Kearney 2008).

An alternative approach is to estimate the α coefficient in the relevant data. Such coefficients can be calculated in a relatively straight-forward manner, since the wage distribution is log linear and the slope and intercept of this line capture the two key parameters of the distribution (α, ω). In principle, only two data points are enough data to recover the slope and intercept of the Pareto distribution. In practice, however, such estimates are extremely noisy and to improve the precision of the estimation, I restrict the sample to locations with at least 20 entertainers. The Pareto coefficient is given by $\alpha_{i,j} = [ln(income_i) - ln(income_j)] \, / \, [ln(rank_i) - ln(rank_j)]$. Using observations below the top code, I compute these Pareto coefficients for each local labor market and year and then impute unobserved incomes between observations from the estimated income distribution. With this approach I obtain the full entertainer wage distribution for each local labor market and year. I then use the data to calculate local top income shares, making use of the the fact that top income shares of a Pareto distribution are given by $S_{p\%} = (1-p)^{\frac{\alpha-1}{\alpha}}$.

B.3.6 Television Data

Data on the TV rollout is documented in publications of the FCC. The FCC decided how to prioritize areas during the TV rollout. I digitize the location of the approved launches. The data on TV launches is published in the annual *Television Yearbooks* and I collect this information and identify the CZ of each TV launch.[61] For TV signal, I use data from (Fenton and Koenig 2020) which compute signal catchment areas of historic TV stations. To compute similar signal reach for stations that were blocked, I additional collect records on the technical features of planned antennas. These details were recorded by the FCC to compute transmission areas and potential signal interference. I use this data to reconstruct the signal of TV stations that narrowly missed out on launches. The relevant FCC records are published as part of the *TV Digest* 1949.

B.3.7 Data on Market Reach of Entertainment Shows

Data on potential show audiences is collected from the *Julius Cahn-Gus Hill Theatrical Guide*. For each local labor market I compute the potential maximum audience. For physical venues this is the seating capacity of the largest venue.

Show revenues in theaters are the price of tickets multiplied by the audience. I use the average price if multiple ticket prices are reported. For TV shows, I collect price data from rate cards. Such cards specify the price for sponsorship of a show at a local station, which allows me to compute the price charged for a TV show. From the price per show I can compute a price per TV viewer, analogous to a ticket price, which quantifies the marginal return to reaching one more customer. Price data is only available for a subset of observations. I infer prices based on data from TV station ad-pricing in 1956 and theater ticket prices in 1919. I use them to estimate a demand elasticity for TV audiences, taking the supply of TV hours as given. The demand curve for a TV viewer is estimated as $ln(price) = 4.051 - 0.460 * ln(TV households)$. The negative elasticity indicates that, as expected, the marginal value of reaching a household is declining. The negative demand elasticity in turn implies that TV station revenues do not increase 1:1 with audience, and the revenue elasticity is 0.54.

The potential audience of TV shows is the number of TV households that can watch a local TV station. This is computed using information on TV signal catchment areas (from Fenton and Koenig 2020) and TV ownership records from the Census.

B.3.8 Migration

The Census includes questions about geographic mobility. For each labor market, I compute the share of entertainers who move. Note that the definition of mobility

[61] Called *TV Digest* in earlier years.

varies across Census vintages. Moreover, it does not distinguish between moves within and across labor markets. IPUMS aims to harmonize differences across Census vintages, and I use their harmonized variable. While such a measure is noisy, classic measurement error will not bias the results but rather inflate standard errors, as we use the variable as an outcome variable.

B.3.9 Controls

Control variables are: median age & income, % female, % minority, population density, and trends for urban areas. Most variables are available consistently throughout the sample period. Income and education are only available from 1940 onwards. The Census race question includes changing categories and varying treatment of mixed-race individuals. I use the IPUMS harmonized race variable that aims to correct for those fluctuations.

B.3.10 IRS Taxable Income Tables

Data from the IRS allows me to extend income data backward beyond what is feasible with the Census.[62] To obtain records for entertainers, I digitize a set of taxable income tables that list income brackets by state and occupation. This breakdown of the data by occupation and state is only available for the year 1916 and is used in robustness checks.

[62] Such tax tables have been used by Kuznets and Piketty to construct time series of top income shares for the US population.

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B.4 APPENDIX: TABLES

Table B1: Summary Statistics

	No. of observations	Mean	S.D.
		Television	
Local TV stations	2,888	0.02	0.25
Local filming cost	2,888	0.14	1.36
Show audience (1,000s)	2,656	72,811	66,719
Show revenue ($1,000)	2,656	4,182,516	3,834,174
TV signal (%)	2,888	60	0.49
		Entertainment	
Employment in leisure activities	2,888	2,468	8,540
Employment in performance entertainment	2,888	177	936
Wage 99th percentile of entertainers ($)	1,435	5,704	4,576
Fair visits (thsd.)	8,664	25	109
Fair ticket receipts ($1,000)	8,664	2.94	1.89
Grandstand show receipts ($1,000)	8,664	1.64	0.97
Rides & carnival receipts ($1,000)	8,664	0.92	7.50
		Demographics	
People (1,000)	2,888	229	658
Workers (1,000)	2,888	86	264
Median income ($)	2,887	1,698	747
Population density	2,888	2.5	7.8
Urban (%)	2,888	17	37
Minority (%)	2,888	9.6	13
Male (%)	2,888	50	2
Age	2,888	27.4	3.27

[Note] The table reports summary statistics for the 722 commuting zones (CZs) over four decades. The 99th wage percentile is only computed for the larger local labor markets, see the text for details. The data is decadal, except *Fair* data, which is annual from 1946 to 1957. *Show audience* and *Show revenue* refers to the largest shows feasible in a CZ (see text for details), and no data are available for some CZs. *Median income* is missing in one CZ in 1940. *Urban Share* and *Filming Cost* are held fixed throughout the sample. Source: US Census 1940–1970, *Billboard* magazine 1946–1956.

Table B2: Effect of TV on Top Earner—Placebo Occupations

	(1)	(2)	(3)
	Panel A: Entertainer among Top 1% of US Earners (% of Entertainers)		
Local TV stations	0.21	0.66	1.09
	(0.52)	(0.89)	(0.52)
Increase on baseline	4%	12%	20%
No. of cluster	722	722	722
	Panel B: Entertainer among Top 1% of US Earners (Per Capita in 10,000s)		
Local TV stations	0.44	0.52	0.87
	(0.22)	(0.23)	(0.32)
Increase on baseline	4%	5%	8%
No. of cluster	722	722	722
	Panel C: Rollout Interruption Sample (Outcome as in Panel A)		
Local TV stations	0.13	0.74	1.33
	(0.58)	(1.02)	(0.80)
Increase on baseline	2%	13%	24%
No. of cluster	113	113	113
Year-Occupation FE	Yes	Yes	Yes
CZ FE	Yes	Yes	Yes
Demographics	–	Yes	–
Local labor market trends	–	–	Yes

[Note] The table shows the effect of local TV stations on top earners in placebo occupations, see the notes on Table 1 for details on specifications. Placebo workers are other high income workers, as described in the text. Observations are respectively 62,042 and 58,837 and 9,718 in Panel A, B and C. Sources: Census 1940–1970.

Table B3: Earning Effect of TV Launch—Triple Difference Analysis

	Share in Top 1%		
	(1)	(2)	(3)
Local TV station × Placebo occupation	-0.41		
	(0.47)		
Local TV station × Performance entertainer	4.87	4.87	4.17
	(2.16)	(2.16)	(1.57)
Local TV station × Interactive leisure		-3.40	
		(1.29)	
Local TV station × Bars & restaurants		-3.80	
		(1.84)	
Local TV station × Professional services		5.23	
		(4.86)	
Local TV station × Medics		-3.24	
		(1.52)	
Local TV station × Engineer		-1.12	
		(1.23)	
Local TV station × Manager		3.55	
		(2.21)	
Year–Occupation & CZ FE	Yes	Yes	–
Pairwise interaction: Location, year, occupation FE	–	–	Yes

[Notes] The table shows triple difference results of local TV stations on top earners. Data and specification are as in 1. The number of CZ–occupation–year observations is 100,308.

Table B4: Effect of TV on Top Earning Entertainers—State Level

	Share in Top 1%		
	(1)	(2)	(3)
Local TV station ×(1916)			8.31
			(5.97)
Local TV station × (1940)			0
			-
Local TV station × (1950)	20.94	20.18	23.32
	(8.09)	(7.36)	(7.27)
Local TV station × (1960)			1.70
			(2.60)
Local TV station × (1970)			8.90
			(2.95)
Years	1940–1970	1916–1970	1916–1970
Year & State FE	Yes	Yes	Yes
No. of observations	912	1008	1008

[Notes] The Table shows results of pre-trend tests. Data and specification are as in 1, Panel A except that the data is now aggregated at the state-year-occupation level. Standard errors are clustered at the state level and appear in parentheses. Each row represents a separate DiD regression. Column 1 estimates the baseline specification of Table 1 in the aggregated data, column 2 extends the time period and column 3 introduces leads and lags of the treatment. The regressor is the number of TV stations in 1950 in the state, allowing for time varying effects. In column 3 the omitted year is 1940. Source: US Census (1940–1970) and IRS in 1916.

Table B5: Effect of TV on Top Earning Entertainers—Alternative Top Income Measures

	(1)	(2)	(3)
	Panel A: Count Entertainer in US top 1%		
Local TV station	30.91	32.09	19.31
	(8.92)	(9.92)	(8.31)
Outcome mean	15.53	15.53	15.53
	Panel B: Share Entertainer in US top 1% (denominator fixed)		
Local TV station	6.51	6.73	9.21
	(1.90)	(1.89)	(3.44)
Outcome mean	6.39	6.39	6.39
	Panel C: Percentage US top 1% from Entertainment		
Local TV station	0.178	0.193	0.194
	(0.025)	(0.038)	(0.063)
Outcome mean	0.28	0.28	0.28
Cluster	722	722	722
Year–Occupation & CZ FE	Yes	Yes	Yes
Demographics	–	Yes	–
Local labor market trends	–	–	Yes

[Note] This table shows the impact of television on top incomes in entertainment and extends Table 1 to additional top income measures. The outcome variable in Panel A is the raw count of entertainers in the top percentile of the US wage distribution. Panel B shows the effect on the share of entertainers in the top percentile and C the fraction of top 1% workers from entertainment. Panel B and C keep the denominator fixed at 1940 levels. Sources: Census 1940–1970.

Table B6: Effects on the 99th Percentile - Alternative Samples

	(1)	(2)	(3)	(4)	(5)
	Filming CZs	(1) + (1)'s Neighbors + Blocked CZs	Largest 25% CZs	Balanced Panel	Full Sample
	Ln(99^{th} Percentile of Entertainer Wages)				
	Panel A: No Imputation				
Local TV stations	0.166	0.185	0.177	0.154	0.135
	(0.085)	(0.078)	(0.078)	(0.077)	(0.044)
	Panel B: Fixed Multiple Imputation				
Local TV stations	0.217	0.209	0.197	0.175	0.173
	(0.096)	(0.085)	(0.085)	(0.084)	(0.061)
	Panel C: Pareto Imputation				
Local TV stations	0.289	0.286	0.283	0.255	0.263
	(0.107)	(0.094)	(0.094)	(0.092)	(0.070)
Year & CZ FE	Yes	Yes	Yes	Yes	Yes

[Note] The Table replicates Table 3 for alternative samples. Column 1 restricts the sample to an event study and thus places that become treated (133 observations), Column 2 adds blocked CZs and CZs that neighbor treated areas (803 observations), Column 3 shows results for the most populated 25% CZs (909 observations), Column 4 uses a balanced panel of CZs (960 observations), Column 5 uses all cells where the 99th percentile can be calculated (1387 observations). Data for Panel C is missing for 0, 3, 5, 3, 7 observations for the respective columns because the Pareto coefficient cannot be estimated. Other specification details are the same as column 1 in Table 3.

Table B7: Elasticity of Top Entertainer Wages to Market Reach – Rollout Interruption Sample

	(1)	(2)	(3)
	$Ln(99^{th}\ Percentile\ of\ Entertainer\ Wages)$		
	Panel A: Repeated Cross-Section OLS		
ln(Audience size)	0.130	0.031	
	(0.027)	(0.027)	
	Panel B: IV		
ln(Audience size)	0.222	0.206	0.176
	(0.037)	(0.040)	(0.038)
First-stage F-statistic	10.6	9.1	7.4
	Panel C: IV		
ln(Value of market ($))	0.315	0.282	0.246
	(0.066)	(0.048)	(0.063)
First-stage F-statistic	13.3	11.7	9.1
Demographics	–	Yes	–
Local labor market trends	–	–	Yes

[Note] The table repeats the regressions in Table 8 for the "rollout interruption sample." Panel A uses an OLS regression on repeated cross-sections, Panels B and C use an IV regression where audience size is instrumented by TV launches. For further details and specifications, see Table 8.

Table B8: Elasticity of Top Entertainer Wages to Market Reach –
Rollout Interruption Sample & Pareto Imputations

	(1)	(2)	(3)
	$Ln(99^{th}\ Percentile\ of\ Entertainer\ Wages)$		
	Panel A: Repeated Cross-Section OLS		
ln(Audience size)	0.125	-0.021	
	(0.027)	(0.049)	
	Panel B: IV		
ln(Audience size)	0.371	0.330	0.318
	(0.075)	(0.074)	(0.076)
First-stage F-statistic	10.5	8.9	7.3
	Panel C: IV		
ln(Value of market ($))	0.526	0.452	0.445
	(0.137)	(0.092)	(0.125)
First-stage F-statistic	13.0	11.5	9.0
Demographics	–	Yes	–
Local labor market trends	–	–	Yes

[Note] The table repeats the regressions in Table 8 for the "rollout interruption sample." Different from Table B7 it uses Pareto imputations for top coded data (one observation is missing because the Pareto parameter cannot be calculated). For further details and specifications, see Table 8.