Automation and the Future of Young Workers: Evidence from Telephone Operation in the Early 20th Century

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Abstract:

Telephone operation, one of the most common jobs for young American women in the early 1900s, provided hundreds of thousands of female workers a pathway into the labor force. Between 1920 and 1940, AT&T adopted mechanical switching technology in more than half of the U.S. telephone network, replacing manual operation. Although automation eliminated most of these jobs, it did not affect future cohorts' overall employment: the decline in demand for operators was counteracted by growth in both middle-skill jobs like secretarial work and lower-skill service jobs, which absorbed future generations. Using a new genealogy-based census linking method, we show that incumbent telephone operators were most impacted by automation, and a decade later were more likely to be in lower-paying occupations or have left the labor force entirely.

JEL Classification: J21, J24, J62, J63, M51, M54, N32, O33

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Over the past few years, automation anxiety has surged in the U.S. and other developed economies, fueled by warnings of a sweeping and permanent reduction in labor demand. Especially concerning is that automation often replaces routine, entry-level jobs which provide young adults a pathway into the workforce (OECD 2018). The elimination of these jobs risks increasing unemployment, but may also erode chances of upward mobility and could give rise to employment sclerosis, long-term labor force detachment, or—at the extreme—political dislocation and social unrest. Rising entry-level workers may also be an especially vulnerable subset of the labor force, lacking the employment protections, unions, or job transfers available to many incumbent workers. How the next generation of young adults entering the labor force adjusts to automation may thus bear even greater economic consequence than technology's effects on existing, incumbent workers.

What might the future of work look like when major entry-level jobs are automated? In this paper, we study one of the largest youth-specific automation shocks in modern history: the automation of telephone operation. In the 1920s, *telephone operator* was among the most common jobs for young women, at its peak accounting for around 4% of the nearly three million young, white, American-born women in the workforce. But between 1920 and 1940, telephone exchanges serving more than half of the U.S. telephone network were mechanized, replacing most functions of local operators. The fraction of U.S. female employment exposed to this shock was comparable to the fraction of the modern U.S. workforce employed as cashiers, office clerks, or customer service workers today— common entry-level jobs which the U.S. Bureau of Labor Statistics projects will decline over the next decade due to automation (BLS 2019a), and similar to the recent decline in executive assistant employment, which exceeds that of U.S. manufacturing (Feintzeig 2020).[1]

Using census data on the complete U.S. population, a longitudinally-linked sample of women, and a newly-constructed dataset measuring local adoption of mechanical call switching, we document its effects on both incumbent workers and future generations. To be clear, this exercise comprises two distinct but closely related analyses, on two distinct (but related) samples, answering distinct (but related) questions: (i) how did automating telephone service affect incumbent telephone operators, and (ii) how did it affect future generations of young women entering the labor market? We work in reverse, showing first that after a city was "cut over" to mechanical operation, the number of 16 to 25 year old women in subsequent cohorts employed as telephone operators immediately, permanently fell by 50 to 80%. These jobs comprised around 2% of total employment for this demographic, and even more for those under age 20. Given the relatively high turnover of telephone operators, this shock may have foreclosed entry-level job opportunities for as much as 10-15% of peak cohorts. Next, we show that, despite its magnitude, the shock did not reduce employment

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[1]Telephone operation accounted for roughly 1.9% of female employment in 1920 and 2.1% in 1930. For comparison, in May 2017, cashiers, office clerks, and customer service representatives comprised 2.3%, 1.9%, and 1.7% of U.S. employment, respectively (BLS Occupational Employment Statistics for SOC 41-2011 (Cashiers), 43-9061 (Office Clerks, General), and 43-4051 (Customer Service Representatives)).

rates. It was instead offset by growth of other middle-skill jobs with a similar wage profile (typists and secretaries) and lower-skill, lower-paying service industry jobs, which absorbed future workers. Our analysis of incumbent operators, however, paints a slightly different story. Telephone operators in cities with cutovers were subsequently less likely to be working the next decade we observe them, and conditional on working, these women were more likely to have switched to lower-paying occupations, though a few became operators at private switchboards or took up other jobs in the telephone industry. The magnitudes of these effects are tempered by the fact that many women exited the workforce as they aged, but because telephone operation—despite high turnover—was one of the few opportunities for women with the potential to be a career, the loss of these jobs was costly for those who would have otherwise chosen to keep them.[2]

To understand this paper, it is useful to first reflect on how AT&T came to employ so many young women, and why it ultimately decided to replace them. From AT&T's founding in the mid-1870s to the late 1910s, telephone calls were manually connected by operators working the switchboards at telephone exchanges around the country. Though initially male, by the early 1900s, operators were almost entirely young women. By 1920, not only was AT&T the largest U.S. employer—at roughly one percent of the U.S. workforce (similar to Walmart today)—but telephone operation in the telephone industry was the third-largest occupation-industry pair for white, American-born women under age 25, and the single largest for those under 20. Around this time, however, AT&T began advising its operating companies to automate telephone operation, beginning in large cities. Under the automatic technology, telephone sets were given rotary dials, and each turn of the dial actuated switching equipment at the telephone exchange, allowing users to place their own calls. In a parallel paper (Feigenbaum and Gross 2020), we combine a rich historical record with quantitative evidence to study AT&T's impetus for automation, which included the increasing complexity of manual operation in large markets, and AT&T's view that the limited supply of qualified operators was rapidly being exhausted as the network grew. Quantitatively, network size (city population) explains much of the variation in local cutover timing. The effect of adopting mechanical switching was to nearly eliminate an entire major category of entry-level work, one city or exchange at a time. By 1940, 60% of telephone exchanges in AT&T's system were dial.

We combine individual-level census data for the complete U.S. population from 1910 to 1940 with a new, hand-collected dataset of cutovers to mechanical switching to study the effects of this shock. The complete count census data provide the information recorded by census enumerators, including occupation and industry, which we (i) aggregate up to a fine-grained city-demographic group panel

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[2]Because telephone operation was a high turnover occupation, as we discuss in Section 1, many more women were ever operators than we see in any single year working as operators. This also means that the effects of automation on incumbent operators are intrinsically likely to be muted, whereas future cohorts who use telephone operation as a springboard into the labor force may be much more at risk. The same reasoning would apply to modern entry-level occupations like clerks and customer service representatives today.

for population studies, and (ii) link across censuses for longitudinal analysis of existing operators. Because traditional census record-linking techniques are not capable of following young women over time (due to name changes prompted by marriage), we introduce a new, generalizable approach to census linking: to build our linked sample, we use public genealogical data from the genealogy platform FamilySearch, match to complete count census records, and reweight to account for the representativeness of FamilySearch data and our linking procedure.[3] We measure cutovers to dial service across the continental U.S. using AT&T archival records and data collected from thousands of newspaper articles. Of roughly 3,000 cities reported as a city of residence in all years from 1910 to 1940 with at least 2,000 people in 1920, 332 have their first cutover by 1940. Throughout the paper, we focus our attention on the subset of these with less than 100,000 people in 1920, where subscribers were typically converted to dial all at once.

Our goal is to provide a unified view of the effects of automation on both existing workers *and* future generations of workers who might have been likely to enter this occupation were it not automated away (an even larger at-risk population). The analysis is thus organized around two complementary but distinct questions, data structures, and empirical designs. To study the effects of dial on future generations of young women who might have been operators were it not for mechanization, we use an event study design, comparing outcomes for successive cohorts before versus after a city's first cutover. To trace the effects on incumbent operators, we link women in 1920 and 1930 to the next decennial census and compare operators to (extremely) similar working women—matched on age, race, nativity, marital status, fertility, and neighborhood—initially living in cities where telephone operation was or was not automated over the following decade.

We find that the automation of telephone operation led to a large, swift, and permanent decline in the number of young, white, American-born women in future cohorts working as operators. Yet we do not find that the shock reduced employment rates: the negative shock to telephone operator demand was instead counteracted by growth in other occupations, especially secretarial work and restaurant work, which absorbed the women who might have otherwise been telephone operators. Incumbent operators, however, were more directly affected. We find that these women

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[3]Four other recent papers attempt to link women census to census. Price et al. (2019) and Marchingiglio and Poyker (2020) link only women whose marital status does not change across censuses. Drawing on marriage certificates as an intermediate step, Withrow (2020) studies rural women in the early 20th century and Craig et al. (2019) focus on one state (Massachusetts) with good marriage record coverage in the late 19th century. In our case, linking already-married telephone operators in 1920 or 1930 to their census record ten years later may be feasible, but it would restrict our analysis to a small, non-modal population of operators. In addition, women who change their name with divorce or remarriage would be systematically not linked, leading to unknown bias. A similar systematic bias may affect a linked sample of only the single telephone operators who remain single in future censuses, as links would be conditioned on a potentially endogenous outcome (marital status). For our national sample of telephone operators, the uneven coverage of marriage certificates across states would be very challenging for our analysis (for example, in the Withrow (2020) sample, half of known marriages are in Virginia and another 20% are in Indiana, though the sample starts with ten southern states and not Indiana). Operators not linked to a marriage record could remain single or have been married in a state with poor marriage record coverage.

were significantly less likely to be telephone operators ten years later, and while some became operators at private switchboards, others left the workforce, and those who remained employed were more likely to have switched to lower-paying occupations.

Collectively, our results suggest local economies can adjust to automation shocks over relatively short horizons and continue to absorb the steady stream of young workers entering the labor market, consistent with arguments made by Acemoglu and Restrepo (2018). In the 1920s and 1930s, much like now, contemporaries feared these opportunities were gone and never coming back—but those fears proved to be misplaced, as other jobs grew to take their place. Yet in contrast to, e.g., Bessen (2015), our finding that incumbent operators were affected also demonstrates that automation is not entirely benign to the workers whose functions it performs.

Public concern about automation has powered a recent surge of academic research on the effects of labor-saving innovation. Much of the recent literature focuses on the effects of industrial robots on labor productivity and employment (e.g. Graetz and Michaels 2018, Acemoglu and Restrepo 2020, Humlum 2019), even though robots are only a subset of the automating technologies used in manufacturing, and are even less common in the service sector.[4] The challenge is that in most settings, automation is hard to measure directly, to the level of what machinery was purchased, how it was used, and what (if any) jobs were displaced. Even when adoption is well-measured, it is often difficult to ex-ante specify whether, when, or for whom a given technology is labor-augmenting or replacing, and scholars can at best infer net effects from changes in wages or labor intensity. An advantage of our setting is that we can measure adoption precisely and that the technology is well-understood. We thus have a unique opportunity to study what happens when firms automate an entire class of entry-level jobs away, especially a firm as big as AT&T in the 20th century, or in service industries which comprise a majority of U.S. employment today.

Our results also contribute to the wider literature on skill-biased technical change (SBTC). Much of the SBTC scholarship has focused on the computerization of routine jobs, wage inequality, and the hollowing out of middle-skill employment in advanced economies (e.g., see Autor 2019), including in white-collar jobs that primarily employ women (as telephone operation did; Dillender and Forsythe 2019). Scholars have also documented a history of SBTC throughout the 20th century (Goldin and Katz 2008), especially as related to industrial electrification (Gray 2013, Fiszbein et al. 2020). In contrast to the modern literature, our evidence suggests that the elimination of telephone operation (a middle-skill job) had only modest effects on wage inequality among young women, primarily because other middle-skill jobs were growing to take its place.

The historical context could play a role in explaining why the shock was well-absorbed by future generations. Whether this is true remains an open question. The first half of the twentieth cen-

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[4]Also see Dauth et al. (2018), Dixon et al. (2019), and Koch et al. (2019). For research on more general automation investments see Bessen et al. (2019), Bennett (2020), or Zator (2019).

tury was a time of major growth in high school completion and labor force participation among unmarried white women (Goldin 2006, Goldin and Katz 2008), accompanied by increasing demand for women workers in white-collar occupations (Goldin 1984). Had the demand for workers who would have otherwise become operators—but for automation—been stagnant or falling or the had the educational investments of the following generations not been made, it is possible the effects of automating such an ubiquitous job might have been more severe. Yet an alternative interpretation—suggested by Acemoglu and Restrepo, and others—is that automation was (directly or indirectly) part of the reason why demand was growing in new jobs in the first place. Though we cannot fully discern these explanations, we view them as interesting open questions.

Though telephone operation may seem like a distinctive case, we think it is important for several reasons. One is scale: as a large, entry-level job, automating telephone operation could potentially have aggregate effects. Because AT&T's system was geographically distributed, and its automation well-documented, we can exploit variation across time and space to study how these discrete events affected the "future of work". There are few other such examples we know of. We also believe AT&T's sheer size as America's largest employer for much of the 20th century makes it inherently important. As a technology company and the 20th century archetype for monopoly, it provides a historical comparison point for large firms like Amazon and Walmart—even the modern AT&T—which often have market power with customers or suppliers and have been rapidly adopting labor-saving technology in core activities (e.g., Amazon order fulfillment). Historical study can help us presage what may happen as automation at large employers recurs.

We proceed as follows. Section 1 reviews the history of the U.S. telephone industry, the automation of telephone operation, and concurrent trends in education and work for young women. Section 2 introduces our data on telephone operators, local labor markets, and mechanical switching. Section 3 describes characteristics of telephone operators and cities with cutovers. In Section 4, we confirm that cutovers significantly reduced the number of telephone operators in a city. In Section 5, we examine what happened to subsequent generations of young women after these jobs were automated away. In Section 6 we contrast these results with the outcomes of incumbent telephone operators. Section 7 concludes with lessons and remaining questions.

1 Historical Background

1.1 AT&T and the U.S. telephone industry

The history of the U.S. telephone industry is largely the history of AT&T, the dominant service provider in the U.S. for most of the 20th century. Bell Telephone (AT&T's predecessor) was founded in 1877, a year after Alexander Graham Bell's successful demonstration of the telephone. One year

later it opened its first telephone exchange in New Haven, CT, and within a few years it had licensed exchanges in all major U.S. cities, begun building long-distance connections between them (under its AT&T subsidiary), and acquired a manufacturing company (Western Electric). In 1899, AT&T became the parent of the Bell system, which eventually comprised dozens of subsidiary operating companies serving different geographic territories around the country.

For its first 17 years, AT&T was a patent-protected monopolist, but the expiration of the original Bell patents in 1894 attracted entry by thousands of "independent" operating companies, which built competing networks in large cities and entered markets (especially rural areas) where AT&T had not. By the 1920s, the U.S. telephone industry employed over 300,000 people, served nearly 15 million telephones, and connected more than 65 million calls per day (Appendix Table A.1). AT&T served around half of telephones in the early 1900s, after which it began acquiring independents across the U.S. in a drive to provide coast-to-coast universal service, and its national share was back up to 79% by the early 1930s. AT&T market shares were even higher in urban markets, where Bell companies were typically the sole telephone service provider.

1.2 Telephone operators and manual call switching

The functional units of each operating company were individual telephone exchanges, each typically connecting to up to 10,000 subscribers in its immediate vicinity. These exchanges in turn connected to each other via trunk lines. All subscribers' lines fed into a switchboard at their local telephone exchange, where human telephone operators physically connected calls by plugging wires into and out of jacks on the board—a task referred to as "call switching". Their work was fast-paced and labor-intensive, with millions of calls connected across the U.S. telephone network each day. It was also costly to scale: every Nth subscriber created N-1 new possible connections, requiring operators to learn more switchboard positions and calls to pass through more operators and switchboards. In large cities, the number of users implied billions of potential connections. As the network grew, the number of operators needed to keep up with call volume swelled.[5]

Although the first generation of telephone operators was mostly male, AT&T decided early on that young women were more likely to have the qualities it sought in operators. By 1910, operators were almost exclusively women. Based on its employment criteria and position in the wage distribution for young women, telephone operation was effectively middle-skill work. Erickson (1946) summarized the job requirements as follows:

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[5]During this period, demand for operators was also growing in other industries, especially at large organizations that sought operators to work private switchboards (e.g., large firms, hospitals, hotels). These switchboards connected telephones within an organization to each other as well as to an outside line of the local telephone company, through which external calls (incoming or outgoing) could be routed.

[T]o be considered for employment as a student operator, an applicant was expected to be a high school graduate, at least 18 but not much older, in good physical condition, and living at home or with close relatives. Good eyesight and good hearing are always specific physical requirements and are carefully checked in the general examination for physical soundness. Some companies further screen applicants by means of mental and aptitude tests. A pleasing voice, alertness, manual dexterity for handling equipment and tools of the job, legible penmanship, ability to make simple calculations rapidly and accurately, a sense of teamwork for cooperating with other operators in establishing connections, a stable disposition not easily ruffled by irritable customers, and courteousness are among the personal characteristics listed as qualifications for the job of operator.

Besides the minimum age requirements, most of these qualifications appear to have applied throughout the 1910 to 1940 period we study in this paper. Contemporary accounts from former operators suggest it was seen as a desirable job, offering higher wages, greater challenge, and more human interaction than alternatives like factory work (Best 1933), though the physical and mental demands of rapid-fire call switching for hours at a time were also high, and internal AT&T memos describe operator turnover rates as high as 40% per year (O'Connor 1930).

In 1920, telephone operators were roughly 2% of the U.S. female workforce and 4% of nearly three million young, white, American-born working women. With 40% turnover rates, as much as 15% of cohorts born at the turn of the century might have *ever* been an operator.[6] Among those age 16 to 20, "telephone operator" was the fifth largest job (Appendix Table C.1), and given its concentration in one industry, "telephone operator in the telephone industry" was the most common occupation-industry pair (Appendix Table C.2). AT&T as a whole was the largest U.S. employer of women in the 1910s, and by the mid 1920s was by far the country's largest employer overall (Appendix Table A.2), with telephone operators comprising around half its workforce.

1.3 Transition to mechanical switching

The first mechanical switching system was invented and refined in the early 1890s. The "automatic" system added a rotary dial to telephone sets and mechanical switching equipment at telephone exchanges. Each turn of the dial transmitted an electrical pulse, which actuated a sequence of selectors at the exchange until a circuit was completed between the caller and the telephone dialed, without manual intervention. Over the next 25 years, mechanical switching was adopted by only a handful of independents. Though AT&T began experimenting with mechanical equipment in 1902, the technology did not compare favorably to manual operation on cost or performance, and AT&T

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[6]Taking the population and age distribution of operators in the 1910 to 1940 censuses, interpolating the intercensal years, and imputing the number of incumbent versus new operators each year, we estimate that 13.7% of white, American-born women in cohorts born circa 1900 was a telephone operator at some point between 1910 and 1940. The basic logic is that for a telephone company to maintain a set of 100 operators, 40 new operators must be hired each year, and over the course of ten years, 400 unique women might be employed.

continued with manual operation until improvements in the technology and rising costs of manual operation made automation more attractive (Feigenbaum and Gross 2020).

In 1917, AT&T's engineering department began recommending that its operating companies adopt mechanical switching for local service in large, multi-exchange cities and continue with manual operation in smaller, single-exchange cities (Gherardi 1917), though ultimately operating companies' management decided whether and when to automate every individual exchange. The automation of telephone operation thus took place gradually, one or two exchanges at a time. Preparing an exchange for mechanical switching typically required 2-3 years of preparation—e.g., to get regulatory approval, prepare the mechanical equipment, distribute dial telephone sets, and draw up new telephone numbering plans and directories. Operationally, however, cutovers from manual to dial (when the wires were cut from the manual switchboards and connected to the mechanical equipment) were discrete events which took only a few minutes.

Mechanical switching specifically replaced operators in connecting local calls, and AT&T records from the 1910s projected that the automatic equipment would reduce the number of operators in large cities by up to 70 to 80% (Gherardi 1917). But even after automation, operators were still needed for long distance calling, information and emergency services, and any remaining subscribers with manual service. Because these were more complex tasks, the residual operating needs required better trained, more experienced operators, who tended to be older. Automatic switching also increased demand for technicians to maintain the automatic equipment, who tended to be men. Technological change in this instance was thus not only skill-biased, but also age and gender-biased, due to occupational sorting by both AT&T and its workforce.[7]

In Figure 1 we illustrate the aggregate diffusion of mechanical switching across the Bell system, using administrative data from AT&T records. Adoption began in the late 1910s and accelerated rapidly—with 32% of Bell telephones on dial by 1930 and 60% by 1940—but it took almost 60 years (to 1978) to diffuse through the entire network, by which time AT&T had already begun adopting digital switching. Our focus for this paper is the 1910 to 1940 period.[8] In Section 2 we document cross-sectional variation, and in Section 3 we will return to discussing the drivers of automation, vis-à-vis both narrative and empirical evidence, in more detail.

[Figure 1 about here]

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[7] Contemporary reports from the Bureau of Labor Statistics provide detailed studies on the effects of dial on local telephone industry employment, which document significant reductions in the number of operators. Across 18 such examples, BLS (1932) reports an average reduction in Bell System operators of over 60% relative to a counterfactual in which labor productivity (operators per call) had remained at 1921 levels.

[8] We use complete count census data to measure local population outcomes, which at the time of writing were only available through 1940. But even with data from later years, 1940 is also a natural stopping point, since World War II brought cutovers to a standstill due to restrictions on the use of copper wire for telephone installations, and also presented a distinct shock to female labor demand (e.g., Goldin and Olivetti 2013, Jaworski 2014).

By 1940, telephone operation in the telephone industry comprised <1.5% of employment for young, white, American-born women (down from its peak of ≈4%) and had fallen to the 11th most common occupation-industry for those under age 20. The demise of the telephone operator continued for the rest of the twentieth century, and only ≈5,000 operators remain in the U.S. today, for information services and collect or emergency calling (BLS 2019b).

1.4   Broader context: Trends in female labor force participation and education

Early in our period, the stigma of being a working woman was quite high, and income effects dominated; few married white women worked and those who did were on average less educated and in poorer households than the rest of the population. Even among unmarried women, work was relatively uncommon, as just over one-third of unmarried white women were working in 1880. Goldin (2006) stresses the unappealing nature of the work as the reason why so few white women— married or unmarried—were working in the first decade of the twentieth century: "jobs were often dirty, dangerous, repetitive, and long in hours per day and days per week."

Over the following decades, the environment changed steadily in the labor market for unmarried women. From the 1900s to mid-century, there was a large, steady increase in demand for clerical and office workers (Goldin 1984).[9] Office work was "nicer, cleaner, shorter-hour, and thus more 'respectable'" (Goldin 2006), though it could be repetitive, and turnover was still high and returns to experience low. Telephone operation was clearly part of this trend. With the rise in demand for these new jobs came an increase in the share of unmarried white women working. More than half of unmarried white women were working in 1910, rising steadily to 60% in 1940.

Our study period also covers the era when the graduates of America's High School Movement hit the labor market (Goldin 1998). Across women born from 1890 to 1925, average educational attainment grew rapidly: from just over 8 years (common school) for the 1890 birth cohort to nearly 11 years for the 1925 cohort (Goldin and Katz 2008). For much of this period, women's education was on average a bit higher than men's, at least prior to the cohorts born around 1920. This growth in education was primarily driven by increasing rates of high school completion; though college attendance was close to parity in this period, 3% of women in the 1890 cohort graduated from college, increasing only to 5% for the 1925 cohort (Goldin 2006).

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[9]In 1900, only around 20% of non-farm working women were in occupations we approximate as white-collar jobs. This group rose to around 35% of working women by 1920 and nearly 50% by 1950.

2 Data and Geographic Coverage

In this section, we describe our new, hand-collected dataset on local cutovers to mechanical switching compiled from AT&T archival records and historical newspaper articles, data aggregated from the complete count decennial censuses that allow us to measure populations in precise demographic cells from 1910 to 1940, and a longitudinally-linked sample of women telephone operators which we use to study individual adjustments.

2.1 Data on local adoption of mechanical switching

Telephone operation was mechanized one exchange at a time. Because these investments were made independently by AT&T's local operating companies, there is no consolidated, administrative list of cutovers across the AT&T system (Hochheiser 2017). However, we located in the AT&T corporate archives a single document from 1937 which lists the earliest cutover and percent of subscribers on dial service for 164 U.S. cities (and seven Canadian cities) with a population of over 50,000, 120 of which were partially or fully dial by the end of that year (AT&T 1937).[10]

To expand the sample to more cities, we turn to historical newspapers. Dial cutovers were nearly always locally reported, due to the public's need to know when to begin using their dial telephones and public interest in the technology and in the fate of displaced operators. We developed two targeted search terms and searched for reports of cutovers between 1917 and 1940 in three online, searchable repositories of digitized historical newspapers—Newspapers.com, NewspaperArchive.com, and GenealogyBank.com—with the goal of maximizing our geographic coverage. Appendix B describes the data collection in detail, including maps of our newspaper locations. In total, we reviewed over 26,000 newspaper pages to locate articles describing cutovers and record three pieces of information: (i) when each took place, (ii) the cities affected, and (iii) whether it was a telephone company exchange or private branch exchange (a.k.a. PBX, or private switchboard).[11,12]

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[10]We extend these data to 1940 with manual research, after which we have 126 U.S. cities in this list with cutovers by the 1940 Census. We use these data for "large-city" robustness checks in Appendix E.

[11]Although many articles provide the exact date of a cutover, some only provide an approximate (or anticipated) date, week, month, season, or year. To smooth over approximation error, we measure cutovers at a monthly frequency, recording the exact month when known and inferring approximate month where otherwise possible (see Appendix B). Because our outcomes are measured at decadal frequency (using census data), our analysis will not be sensitive to small measurement error. Most cutovers are reported multiple times, either in multiple newspapers or in multiple issues of the same newspaper, such that the data are largely internally-validated; in all such cases, we review and harmonize the collected information, using duplicate reports to fill in gaps.

[12]This search effort is centered around Boolean search terms which return a list of candidate articles on dial cutovers, which were then read by research assistants, who were instructed to determine whether the article does in fact describe a cutover, the city or cities affected and timing, whether it affects local telephone service (versus long-distance, or private branch exchanges), and any other pertinent details. This search is powered by two search terms chosen after extensive testing, one of which is very targeted and designed to minimize false positives, the other of which is broad and designed to minimize false negatives. See Appendix B for details.

Combining these data sources, our final sample contains 688 U.S. cities that were cut over to dial before the 1940 census. The vast majority of cutovers are in the Bell system, although a few are by independents, including a handful before 1919, the year AT&T first began adopting mechanical switching. Figure 2 maps the cities with cutovers in the AT&T and newspapers data (Panels A and B, respectively). Merging these data with 1940 city populations from the census (see Appendix B), we find that by 1940, 86 of the largest 100 U.S. cities, and 40% of the largest 500, had at least one cutover in our data. 53.8% of the U.S. urban population lived in cities where telephone service was partially or fully mechanical. The fraction of the urban population exposed to dial was greatest in the Northeast (at 58.9%) and lowest in the South (47.8%).

[Figure 2 about here]

Using the AT&T administrative data, we verify that our newspaper-derived cutover dating is accurate and that cutovers in small- and medium-sized cities were typically one-shot events. As Appendix Figure B.8 shows, for cities in both the AT&T and newspaper datasets, the earliest cutover we identify in newspapers is nearly always the same as that reported in the AT&T data (the few cases where a newspaper-reported cutover preceded an AT&T cutover were independents). Appendix Figures B.2 and B.3 provide evidence that cities of under 100,000 people in 1920 typically had one cutover in which the entire service area was converted to dial, whereas larger cities were converted in a more piecemeal fashion.

2.2 Data on local outcomes

We use IPUMS complete count U.S. census data (Ruggles et al. 2019) to measure local outcomes between 1910 and 1940. Throughout this paper, we restrict attention to the adult (16+) non-farm population in the continental U.S. only. We aggregate this population up into a fine-grained panel, measuring city-level outcomes by sex, age, race, ethnicity, birthplace (U.S. or foreign), occupation, and industry. Importantly for our purposes, telephone operator is one of 283 coded occupations in the IPUMS data (code 370), and the telephone industry is one of 162 coded industries (code 578), making it possible for us to measure the precise size of the local operating force and identify workers directly exposed to cutovers. For each of these subgroups, we measure several outcomes, including employment, educational status, marriage, and fertility.[13]

The IPUMS data report individuals' state and county, a raw city string (as it was transcribed from the original manuscripts), and an IPUMS-standardized city name, where applicable. Because

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[13]In preparing these data, we create a new occupation code that identifies individuals who are reported as either (i) not being in the labor force or (ii) having a non-working occupation (e.g., housewives, students, retirees, disabled persons, inmates) or unknown occupation, and we define the working population as all others, i.e., all persons who both (i) report as being in the labor force, and (ii) have a working occupation.

standardized city names are not always provided or fully consistent, we undertake an independent, manual effort to harmonize city spellings (see Appendix B). We then identify the cities that (i) are observed in each census from 1910 to 1940, and (ii) have at least 2,000 people in the complete count data in 1920. We drop 14 cities with ≤500 people in any year, 56 cities with anomalous reporting of occupation (Appendix B), 31 cities with ambiguous cutover timing, and all New York City boroughs, yielding a final balanced panel of 2,922 cities, of which 332 are in our data as having their first cutover by April 1, 1940 (the date of the 1940 Census).[14]

2.3 Linked sample of female telephone operators

To understand the long-run effects of telephone cutovers on the operators themselves, we have to follow the operators over time. However, linking women across censuses is extremely challenging. Census linking—whether automated or manual—is based on "stable" features recorded in the census like first name, last name, year of birth, and place of birth (Abramitzky et al. 2020). Because most women changed their names at marriage, these features are only stable for men, and most studies following individuals over time in the early twentieth century therefore focus only on men.[15] To link the women in our sample, we develop and implement a novel linking procedure, making use of a popular genealogy platform and the "work" of many expert family historians linking the women in their family trees across censuses and marriage; in effect, we rely on genealogists and descendants, rather than prediction, to tell us which records belong to the same person.

We link in four steps. First, we identify all women working as telephone operators in the telephone industry in the 1920 and 1930 complete count census data (Ruggles 2002). After limiting to women in our focal cities, we have 96,183 women in 1920 and 61,110 women in 1930.[16] Second, we look for each of these women on FamilySearch, a public genealogy platform with an open wiki-style family tree (Price et al. 2019), where users create pages for deceased individuals—usually their own ancestors but not always—and attach links to historical records, including entries from Federal Censuses,

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[14]We drop the handful of cities with a population ≤500 in 1910 to eliminate those where inference is made difficult by small samples—though this is immaterial to our analysis, which will be weighted on population. In addition, in a handful of (primarily small) cities, there was at least one year in the data with zero or near-zero working-age adults reporting an occupation. Many of these cities are geographically adjacent—such as Bangor, ME and Brewer, ME in 1920 (Appendix Figure B.9)—suggesting these are attributable to enumeration errors and should be excluded. We drop New York City because it is difficult to discern cutovers in different boroughs in newspaper articles and because it is an outlier in the sheer number of cutovers experienced.

[15]A few papers make use of innovative techniques to avoid linking women on surnames, such as Olivetti and Paserman (2015) who pseudo-link people over time using the socio-economic content of first names.

[16]This sample omits a small number of male operators from our analysis as well as a small number of operators younger than 16 or older than 60. Only operators in cities with cutovers after 1920 are included. We further limit to operators in cities with population ≤100,000 in 1920, where cutovers were typically one-shot events, matching the sample we use when we study the next generation of potential operators (see Section 4). For the 1930 sample, we further restrict the sample by filtering out cities with cutovers before 1930, as these women are selected on being operators *after* their city was cut over to dial service.

marriage records, and birth certificates. We search FamilySearch in our base years by name, age, sex, location, and state of birth. Because the 1920 and 1930 census transcriptions in the IPUMS complete count data are based on the same original manuscripts as those on FamilySearch and we can use names, addresses, and other characteristics to link them, matching is straightforward. However, not all telephone operators have a page on FamilySearch. We are able to find 34.6% of operators in 1920 and 37.0% in 1930 with a FamilySearch page.[17]

Third, we query the FamilySearch tree for links to the next census. That is, we begin with the set of operators who were attached to the tree in year $t \in \{1920, 1930\}$, the census in which they were an operator. We check whether or not each operator's profile on FamilySearch has been linked to a record from the census in $t+10$. Conditional on being on the tree, 48.8% of records in our sample from 1920 are linked ahead to the 1930 census and 49.6% of 1930 records to 1940.

Finally, for the set of operators with FamilySearch records attached to censuses in t and $t+10$, we use census record metadata—reel, page, and line number—to make links back to the complete count, restricted-use IPUMS data. This process yields a sample of 16,253 operators linked from 1920 to 1930 and another 11,220 linked from 1930 to 1940, the latter number lower because we exclude operators in cities already cutover to dial. For all of these operators, we observe the full set of census covariates in t and $t+10$, allowing us to study what happens to operators a decade later, including their occupation, industry, marital status and fertility.

These data would be sufficient for comparing incumbent operators in cities with versus without cutovers, but because cutovers affect all local operators, we would not be able to control for city-specific trends. We thus supplement these data by identifying, for each operator, a matched comparison set of women from the same census enumeration district (akin to a neighborhood of roughly 1,000 residents) who were also working and of the same age (± 5 years), sex, race, nativity (U.S. versus foreign-born), parental nativity, marital status, and with or without children, and we apply the same linking procedure to track them from a base year to the next census. This effort produces matched controls for about three-quarters of operators in 1920 and 1930, with an average of 4.7 control women per operator. With this expanded sample we can add operator-specific fixed effects to condition comparisons to between treated operators and their matched controls.

An example can clarify why linking women is difficult, and why the FamilySearch data can help. Suppose we start with a telephone operator in 1920 in New York named Daisy Fay. We see in the

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[17]Whether or not an operator—or anyone else—is attached to the FamilySearch tree is inevitably nonrandom. Pages are built, and records attached, by people working on family history today, and the FamilySearch platform is affiliated with the Church of Jesus Christ of Latter-day Saints. As long as the bias in who is likely or not likely to be on the tree is uncorrelated with the timing of cutovers, our event study strategy—comparing operators across cities and before and after cutovers—should produce an unbiased estimate of the cutover treatment effect. In Appendix B.3.3, we describe in more detail what predicts whether or not an operator is on the tree and shows that match rates are not a function of our treatment in Table B.3.

1920 census that Daisy was born in 1902 in Kentucky. With traditional census linking methods like Abramitzky et al. (2020) or Ferrie (1996), we would search for records in the 1930 Census with the name Daisy Fay, born in 1902 in Kentucky, likely with some tolerance for transcription errors or enumeration errors in these fields. However, if Daisy marries Tom Buchanan in 1922, we would have no way of knowing that Daisy Fay is likely known as Daisy Buchanan in 1930. Worse, if another woman named Daisy born in Kentucky around 1902 marries and takes a surname of Fay, we could falsely match two women who are not the same person. With our FamilySearch-based approach, we instead search for Daisy Fay on FamilySearch in 1920. If her 1920 record is attached to a page, we consider her on the tree. We then look to see if a FamilySearch user has also attached her to the census in 1930, possibly triangulating with knowledge of her name after marriage or her marriage date, either from personal knowledge or an attached marriage or birth certificate (or in Daisy's case, a prominent work of American literature). If she is linked to both the 1920 and 1930 censuses, she will make our sample.

The set of operators in this sample is inevitably not random. However, selection in linking is not a problem unique to our source and setting. Bailey et al. (2017) document the general unrepresentativeness of most historical linked samples made via algorithms (Abramitzky et al. 2020). To account for this bias, we follow Bailey et al. (2017) and construct inverse propensity weights (IPW). We describe the process in more depth in Appendix B, but in short, we use initial covariates to predict which records are more likely to be linked ahead. Key features include age, race, middle initial and name, name commonness, name length, marital status and fertility. We also control for differential linking by state of birth and state of residence in the base year, which helps us account for selection into the FamilySearch platform vis-à-vis descendants or genealogists today, among other issues. Many features have the predicted signs. Women with more common first names are less likely to be linked. Women from larger families or with children are more likely to be linked, likely because larger families increase the chance a descendant uses the FamilySearch platform. In addition, because FamilySearch is affiliated with the Church of Jesus Christ of Latter-Day Saints, women living in Utah or born in Utah are significantly more likely to be on the tree and linked ahead. As we show in Appendix B.3, rates of being on the tree or being linked from one census to the next are not correlated with our cutover treatment, and the use of IPWs enables us to recover an unbiased estimate of the effect of cutovers on incumbent operators.

3 Characteristics of Telephone Operators and Cutover Cities

3.1 Characteristics of telephone operators

Table 1 gives a summary view of the young, white, American-born female population from 1910 to 1940, splitting the sample into 16-to-20 and 21-to-25 age groups. Labor force participation fell sharply for the younger group in this period (from 42.5% to 28.3%), as more completed high school, while rising for the older group (from 37.7% to 45.2%). In the 1920s, around 4 to 4.5% of working 16 to 20 year-olds at any given time were telephone operators, but this figure masks heterogeneity, as it approached 7% in western states. Considering that many women were operators for only a short period, usually early in their careers, the fraction of young women in the labor force that was ever an operator—and thus, the fraction of future cohorts that might suffer from the loss of these opportunities—would have been substantially larger (see Section 1).

[Table 1 about here]

Conversely, we can measure the characteristics of telephone operators directly in the census data—including counting how many were young women. Table 2 reports the total population of telephone operators age 16+ from 1910 to 1940, split out by industry (telephone industry versus others), along with their demographics. The total number of operators working in the telephone industry was growing rapidly at the beginning of the century and peaked in 1930, at 180,000. Roughly 90% of these operators were white, American-born women throughout the period, but from 1910 to 1940, the occupation went from employing primarily younger ($\leq$25) to older (26+) women, who were also more likely to be married and have families—suggesting that for some women, telephone operation was not just a job, but a career. Although non-telephone industries employed only 2,400 switchboard operators in 1910 (mostly men), by 1940 this population had grown to over 41,000 workers and mirrored the demographic characteristics of operators in the telephone industry. Telephone operation thus went from being a young women's job to an older women's job over the period covered in this paper, as local service was automated.

[Table 2 about here]

3.2 Characteristics of cities with cutovers

Why did different cities adopt dial when they did? Understanding this variation is an essential step for us to identify the effects of cutovers on labor market outcomes. In concurrent work (Feigenbaum and Gross 2020), we are studying what propelled AT&T's automation of telephone operation and

why it took nearly 60 years to complete. Drawing on both the historical record in AT&T's internal archive and quantitative analysis, we find that automation was primarily a response to the demands of a large and growing telephone network, not a reaction to broader labor market conditions. The complexity of manually operating large markets—where the number of possible connections grows quadratically in subscribers—meant that AT&T's operator needs were growing far faster than the labor supply. This problem was compounded by its commitment to hiring operators from a narrow subset of the population, which it was exhausting. Altogether, as Lipartito (1994) writes, manual switching was viewed by AT&T as "a potentially serious bottleneck."

We show in this concurrent research that automation was propelled by economies of scale—and in turn, reinforced them by supporting AT&T's continued growth in local markets. Importantly, we do not find evidence that automation was related to concurrent trends in employment for young, white, American-born women or the population overall. However, we do find that automation correlated with the presence of operator unions, which bargained for higher pay and better working conditions, and at times struck. Because operators were the fulcrum of the telephone system, these strikes could cause near-total disruption to telephone service (Lipartito 1994), and may have made automation more of an imperative for telephone company managers.

We summarize this evidence in Table 3, which provides mean characteristics of cities by the timing of their first cutover, binning cities into five-year intervals from 1920 to 1940, as well as pre-1920 and post-1940. We measure characteristics in 1910—or where necessary, circa 1910—before mechanical switching was widely adopted. Panel (A) shows unconditional means of these variables for cities in each interval (across columns), estimated by regression, with robust standard errors in parentheses. Here we see that cutovers correlate strongly with population, with larger cities being automated sooner, especially in the AT&T cutover era (post-1920). Cities with earlier cutovers may have had somewhat higher labor force participation among young, white, American-born women (which we denote with the label "f/n/w/y"), and were also more likely to have had operator unions and strikes pre-1920, but are otherwise demographically similar.[18] In Panel (B), we re-estimate these regressions controlling for log population. Once we do, most of these differences vanish, with the exception of cutovers correlating with operator unions and strikes.

[Table 3 about here]

The results of this table underscore the importance of population in explaining cutovers, consistent with the unit economics of telephone service provision in large and rapidly-growing markets. We will

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[18] We measure local union chapters using historical issues of the Journal of Electrical Workers and Operators (IBEW 1915) and the Union Telephone Operator (IBEW 1921), the monthly journal of the Telephone Operator Department of the International Brotherhood of Electrical Workers, and strikes using Annual Reports of the U.S. Secretary of Labor (U.S. Department of Labor 1913) and written histories by Norwood (1990) and Segrave (2017).

thus include year-specific controls for city population throughout our analysis, which can account for concurrent trends taking place in cities of different size. Later in the paper, we will also examine pre-trends and provide balance tests in the outcomes of interest, which will reinforce our confidence in our ability to identify the effects of cutovers on other outcomes.

It is also worth noting that there is residual idiosyncrasy in the timing of cutovers. Although it is easy to think of AT&T as a monolith, recall that AT&T was a holding company, parent to two dozen regional operating companies which comprised the Bell System. It was ultimately the operating companies' managers who made the decision to 'flip the switch', and sometimes, similar cities were automated at different times for independent reasons. For example, Lawrence, Massachusetts was cut over to dial in December 1924. Lowell, Massachusetts—a similar mid-size manufacturing town only 10 miles away—was not cut over until March 1939. Worcester, Massachusetts—slightly larger, but industrially similar—was cut over in June 1930 (Appendix A).

4 Effects of Automation on Demand for Telephone Operators

Our primary goal in this paper is to understand how the technology shock of mechanical switching affected the labor markets for both future generations of young women and incumbent operators. However, any such effects could only occur if mechanical switching—and our measure of its adoption by city—led to a decline in demand for telephone operators. In this section, we establish that both the number of telephone operators and the share of young, white, American-born women who were operators fell significantly after a city adopted mechanical switching.

4.1 Empirical approach

We take two empirical approaches to studying the effects of dial. Here and in Section 5, we analyze effects on local labor markets with an event-study specification, exploiting the staggered adoption of mechanical switching and comparing outcomes before and after each city's first observed cutover. In Section 6, we turn to our linked samples and estimate the effects of cutovers on individual operators, comparing those in cities with cutovers to those without, controlling for a rich set of individual- and city-level covariates. Throughout, our focus will be the sample of cities with population ≤100,000 in 1920, where automation was typically a discrete event.[19]

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[19]Recall from Section 2 that the focal city sample contains 2,922 cities, of which 332 have a cutover in our data before the 1940 census. For our event study strategy, we pare this sample to the 2,846 cities with 1920 population ≤100,000 and without a cutover before 1917. This latter restriction is applied because our newspaper-based data collection was limited to articles published between 1917 and 1940, and our coverage of cutovers pre-1917 is therefore incomplete—although we do not consider this to be problematic, as pre-1917 cutovers were only performed by smaller, independent telephone companies (rather than AT&T), and the results are not sensitive to this choice. Of these 2,846 cities, 261 have a cutover pre-1940 census. In Appendix E, we also consider the "large" cities in

Concretely, we estimate the following event-study specification:

$$Y_{it} = \sum_s \beta_s D_{it}^s + \alpha_i + \delta_t + X_{it}\phi + \varepsilon_{it} \tag{1}$$

where i and t index cities and (census) years, α_i and δ_t are city and year fixed effects, and X_{it} are time-varying controls; s indexes event time with respect to a city's first cutover, omitting the period immediately before a city's cutover, which becomes the reference point for the event study estimates (β_s). In our primary specifications, we measure s in 10-year intervals, to be consistent with the decadal frequency with which outcomes are measured in the census. For certain analyses, we also estimate two-year intervals to better understand adjustment dynamics, with the important caveat that each bin (in event time) will contain different treated cities, since each city is measured once every ten years (and will thus be included in every fifth bin).

We measure outcomes Y_{it} at the level of city-demographic category-year, but in nearly all specifications we will restrict attention to a single subpopulation per city (typically: white/non-Hispanic, American-born women age 16 to 25, pooled or by age), such that observations are effectively at the city-year level. These outcomes generally take the form of the log number of people in that subpopulation of a certain type (e.g., the log number of telephone operators), or the fraction of that type (e.g., the fraction who are telephone operators), in which case we weight our regressions by population (the denominator). We are thus estimating pre- versus post-cutover changes across cities that had cutovers at different times, with fixed effects and other controls being estimated off of these cities as well as all other in our sample which did not have a cutover by 1940. Throughout, we cluster standard errors at the city (x demographic group) level.

Our standard set of controls (X_{it}) consists of log city population by year effects, which will account for differential trends taking place in larger and smaller cities over time, and which are especially important because city size is closely related to cutovers (Section 3).[20] Although year fixed effects alone can account for national trends, many of these forces were trending locally at the same time as cutovers, and differentially so in cities of different sizes. As an empirical matter, these controls eliminate differential pre-trends across the outcomes we study.[21]

After establishing that the immediate effect of cutovers is a permanent, approximately-level decline

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the AT&T data, for which we know the fraction of subscribers with dial service by 1940, and study long-difference outcomes as a function of this intensive measure of adoption.

[20]For regressions where the outcome is a log number of people from a given demographic of a certain type (e.g., log number of young, white, American-born women who are telephone operators), we also always control for the log size of the focal demographic (log number of all young, white, American-born women).

[21]In Appendix F, we include results with year fixed effects only, where these underlying trends are more visible. The results are also robust to controlling for state-year effects, but we are mindful to not oversaturate the model. Other trends which AT&T records identify as drivers of cutovers, such as growth in the fraction of young women who were operators, are outcomes we study, and we will be able see these pre-trends below.

in the fraction of young women who were telephone operators, which is a difference-in-difference result, we will replace event studies with difference-in-difference specifications for other outcomes, often by individual ages, with the following specification:

$$Y_{it} = \beta \cdot \mathbb{1}(\text{Post-Cutover})_{it} + \alpha_i + \delta_t + X_{it}\phi + \varepsilon_{it} \tag{2}$$

4.2   Effects of dial on operator jobs

Were operator jobs eliminated by cutovers? Appendix Figure D.1 provides event study estimates of the effects of cutovers on employment shares in telephone operation. In the full (working age) population of men and women, cutovers a small but significant decrease in this share, of around 0.2 percentage points (p.p., in blue). However, as we know from Table 2, the vast majority of telephone operators were from a specific subgroup. As we narrow our focus to these demographic groups more likely to be operators, the magnitude of the effect grows, with declines in employment of roughly 0.9 p.p. among women (red) and 1.7 p.p. among young, white, American-born women (green). Relative to baseline operator employment share of ≈3.9% for young women (and 1.8% for all women), the decline is substantial, especially for entry level workers with weaker labor force attachment. This large exposure to the shock among young, white, American-born women motivates our focus in most of the rest of the paper on this demographic group.

Figure 3 shows the effects of cutovers on the (log) number of young, white American-born women who were telephone operators in the telephone industry, first in 10-year intervals (Panel A) and then in two-year intervals (Panel B), with associated 95% confidence intervals. Cutovers caused a sharp decline in the number of young operators: though the number of young operators was on average growing moderately in the decades before a city's first cutover to dial, even conditional on overall population—consistent with AT&T's motivations for adoption (as in Feigenbaum and Gross 2020)—it subsequently dropped by 50 to 80% (Panel A). Our higher-frequency estimates indicate that the cutover effect kicked in immediately (Panel B).[22]

[Figure 3 about here]

In Figure 4 we shift our focus from the number of operators to the fraction of young women's jobs that were automated away by cutovers. Panel (A) plots the high-frequency event study estimates for the percent of young, white, American-born women who were telephone operators, where it becomes apparent that automating local telephone operation immediately and permanently eliminated nearly 2% of area jobs for the group. This effect is measured in terms of the fraction of

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[22]Because we observe very few cities 20+ years post-cutover (these are cities with a pre-1920 cutover observed in 1940), standard errors for the final event study bin are generally larger than for other periods.

young women who were operators at a moment in time (the month the census was taken), but given high turnover, eliminating 2% of jobs may cut off entry-level job *opportunities* for several times as many people. Although we have thus far been estimating event studies, this view of the data also makes clear that the effect is in essence a difference-in-difference (DID) result, motivating our use of a DID strategy throughout the rest of the paper. Panel (B) estimates this DID, splitting the sample by individual ages (16 to 25). We see that mechanical switching hit the youngest ages the hardest, workers we might expect to be most vulnerable to labor force detachment in the face of such a large and long-lasting negative shock to labor demand.

[Figure 4 about here]

In Appendix E, we evaluate the robustness of these results to other estimation methods. A flurry of recent papers has highlighted the potential drawbacks of using OLS to estimate event studies, especially in the presence of treatment effect heterogeneity and when most or all of the sample is treated. To a first order, we do not expect these threats to be problematic in our setting, for two reasons: (i) we have a very large sample of never-treated cities in the control group (over 90% of the cities in our sample are never-treated), and (ii) Figure 4 suggests this shock was a pure DID, without time-varying effects. Even so, in Appendix Figure E.1 we present the event study estimates above using the estimators of Sun and Abraham (2020), Callaway and Sant'Anna (2020), and Borusyak et al. (2021), where we find consistent results across all four approaches. In light of this evidence, we proceed with OLS for the remainder of the paper.

We present several other robustness checks in Appendix E. Though we are estimating these effects in all cities in the continental U.S. meeting our sampling criteria, our measurement of cutovers is in part dependent on the geographic coverage of our historical newspaper data sources. Incomplete measurement should in general only attenuate our results, as we would be mistakenly binning unobserved treated observations with the control group, and for this reason we believe our estimates to be lower bound effects. To allay residual concerns about selection, we estimate the same regressions on a sample of cities which we know to have continuous coverage in our data sources from 1917 to 1940, where we find similar (if not slightly larger) effects on operator employment. We also estimate the effects of dial in larger cities using the AT&T sample and a long differences strategy—exploiting the extent of local dial penetration across large cities between 1920 and 1940—and find quantitatively similar results. In addition, we present cuts by census region, city size, and ex-ante exposure to automation, as measured by the fraction of young women in 1910 who were telephone operators in the telephone industry. We find these effects are largest in cities in the top quartile of exposure, in larger cities, and cities in the West census region.[23]

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[23]These three results are likely closely related, as telephone operation employed a larger fraction of young women in larger cities and in the West region (see Appendix Table 1).

4.3 Substitution into other jobs in the telephone industry, or into telephone operation in other industries?

As we pivot to studying what happened to young women after telephone operation was automated, our first question is whether the reduction in operator jobs was countered by increased hiring of young women into other jobs in the telephone industry, or by the growth of telephone operators in other industries.[24] Contemporary AT&T sources alleged this was true for incumbent operators: in a 1931 letter to the U.S. Secretary of Labor, for example, an AT&T Vice President claimed that "Every effort is made to provide for all employees wishing to continue work after the cutover, [as] transfers are made to other telephone departments and positions found in other industries" (Carter 1931). Was this true for future cohorts, or did they have to find work elsewhere?

Our answer is a definitive no: we see no effect of cutovers on the hiring of young women into non-operator jobs in the telephone industry, or on hiring into telephone operator jobs in other industries (if anything, they slightly decline, although the magnitudes are far smaller, as a fraction of employment, than the automation shock itself). Figure 5 presents these results, first with a pooled event study (top row) and then with DIDs by age (bottom row).

[Figure 5 about here]

5 Effects on Future Cohorts of Young Women

Contemporary sources offer hints of what might have happened to incumbent operators and future cohorts of young women after dial, which we use as a guide in organizing our analysis. Newspaper articles sometimes discuss the fate of operators, including marriage (e.g., see Appendix A). A report produced by the Women's Bureau of the U.S. Department of Labor (Best 1933) provides a more nuanced view, informed by home visits to survey displaced operators in two cities, one of which experienced a one-shot cutover like those in our sample. Of the 78 women surveyed, a year later 18 were re-employed by the telephone company, and 33 in other industries—10 in retail, 8 in clerical jobs, 7 as PBX operators, 4 in factories, and others as waitresses, nurses, or beauticians—although many had spent time unemployed and subsequently had lower wages.

The report also noted that displaced operators were a "large enough group to be of public interest," and as a result, the telephone company "sought the cooperation of the [local] chamber of commerce ... in finding possible work for the operators affected" (Best 1933). This yielded jobs in "office work, sales work in stores, and counter work in restaurants." Some women moved to exchanges in other

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[24]As Table 2 shows, operator employment in other industries was rapidly growing, although as Appendix Table C.4 makes clear, most of this growth was in large cities, which are excluded from our sample.

cities. But while incumbent operators may have had this informal safety net, future generations of young women who would have been operators in a world without dial had no such protections. Our interest here is specifically how these future cohorts fared.

The question which looms largest is whether cutovers reduced employment rates. Table 4 estimates the effects of cutovers on the fraction of young, white, American-born women who are working, in school, married, and have families, breaking out the results by age group (16 to 18, 19 to 22, 23 to 25).[25] In the first column, we present the large, negative effect of cutovers on the fraction of each subgroup working as telephone operators, which serves as a reference point for evaluating effect sizes in other outcomes. We find no effects on the fraction of young women working, in school, married, or with children for any group, with relatively precise zeros. We can rule out general unemployment increases of the magnitude of the shock itself.

[Table 4 about here]

If automation did not increase unemployment, what were these young women doing instead? To discipline our analysis of the effects of cutovers on employment in other occupations, we use information from Best (1933), occupation- and sex-specific wage distributions from NICB (1926), and data on the most common occupations for young women from the complete count data itself (Appendix Table C.1). Best (1933) identifies white-collar office work, factory work, service work, and sales counter work as candidate alternatives. Several of these are also among the most common occupations for young women, and the NICB data in particular reveals that typists, stenographers, and office machine operators had similar wages to telephone operation (Appendix Table B.5), which we consider the closest substitutes. In the figures below, we restrict our attention to service sector jobs, where most of the adjustments appear to have been.

Table 5 estimates the DID effects of cutovers on the share of working young, white, American-born women in telephone operation versus in six other jobs: (i) office machine operators, (ii) typists, stenographers, and secretaries, (iii) other office clerks, (iv) sales clerks, (v) beauty parlor workers, and (vi) restaurant workers.[26] Growth in middle-skill secretarial jobs and low-skill service jobs offset most of the operator jobs lost to automation. However, the effects vary by age. Women aged 19 to 22 moved into similar-paying secretarial jobs. In contrast, women aged 16 to 18 were more

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[25]When we estimate the effects of cutovers on the outcomes in Tables 4 and 5 by age, we find distinctive patterns across different age ranges. These patterns motivate our choice to bin ages as we have.

[26]The effect of cutovers on telephone operation employment in Column (1) of both Tables 4 and 5 are of different magnitudes because each table is measuring outcomes within (slightly) different subpopulations. In Table 4, we study outcomes as a share of the white, American-born, female population, while in Table 5 we focus on white, American-born, women *who are working*. In both cases, we want the denominator in the first column to match the denominators in the rest of the table, to serve as a useful reference point.

likely to be in lower-paying service jobs, like waitressing or beauty parlor work.[27]

[Table 5 about here]

Our robustness checks mirror those presented in Section 4.2: Appendix E presents analogous results for cities with continuous newspaper coverage from 1917 to 1940, where we find qualitatively similar results. In this subsample, we see slightly more substitution into sales clerking, but standard errors are sufficiently large that we cannot rule out that the results are the same as in Table 5. We also find similar results for our large city, long-differences sample, where if anything, even more of the occupational substitution was into waitressing, including at older ages.

Throughout this section, we have estimated the effects of cutovers on labor market outcomes directly, with an implicit first stage effect on telephone operator jobs, which we showed in the previous section. In Appendix E we formalize this logic with a two-stage least squares design, regressing labor market outcomes on the share of young, white, American-born women who were telephone operators in the telephone industry, instrumenting with cutovers. The results are statistically and quantitatively similar, with first-stage F-statistics of nearly 200.

5.1 Pre-trends and balance tests

A potential threat to this analysis is the possibility of confounding trends. For example, if cutovers were more likely to occur where young women's labor demand was growing, this could have softened the impact on employment. In Appendix Figure D.2 we provide balance tests, comparing prior-decade changes for cities which (i) experienced their first cutover in the next decade, to those which (ii) would not be cut over to dial for at least another decade. Importantly, there are no systematic changes in overall employment rates in the run-up to cutovers. We see modest evidence that office work and restaurant work may grown faster from 1910 to 1920 in cities with cutovers in the 1920s, and employment rates in telephone operation were growing faster from 1920 to 1930 in cities with cutovers in the 1930s. In Appendix Figures D.3 and D.4, we plot the full event studies for these outcomes, by age group, where we see little evidence of pre-trends; any such trends are only seen ≥20 years prior to cutovers and are unlikely to be directly related.

5.2 Mechanisms: Why was employment so stable?

The lack of an effect on overall employment raises the question of why local labor markets adjusted so smoothly, and whether this result is general—or under what conditions it arises. Broadly, there

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[27]Other common occupations for women in this period include factory work, private household work, teaching, and nursing. However, we do not find that these occupations grew after cutovers.

are two families of explanations. One is that automation, while destroying some jobs, intrinsically creates demand in others which are complementary to the new technology—a direct offset. For example, when call switching was mechanized, AT&T may have needed more electrical engineers. The other is that automation reduces labor demand for some tasks (like telephone call switching), but through a mix of innovation and reallocation, the economy can endogenously create demand for labor in new tasks that might employ the otherwise-obsolete workers (in the spirit of Acemoglu and Restrepo 2018). Put a different way: under the right technological, institutional, and economic conditions, new jobs in other sectors may be endogenously created.[28]

Distinguishing these stories is difficult, though our setting presents a few opportunities to explore them further. We believe the first story—a direct offset—is unlikely to explain our findings. Though we find that cutovers increased telephone industry employment of electrical engineers, they were far fewer than the lost operator jobs, and overwhelmingly male. Likewise, although telephone dialing may have incrementally increased demand for secretaries directly (e.g., to patch through calls), we do not expect this could fully offset telephone companies' reduced demand for telephone operators. Our interpretation is instead the second story: when demand for some tasks is destroyed, demand in others endogenously emerge (over time) to assume their place. To unpack this further, we explore heterogeneity in our results across cities which varied in potentially informative ways. Although there are many more potential sources of labor market resilience than we could posit here, we focus on two: local economic dynamism, and aggregate demand.

Resilience could be rooted in the underlying strength of local demand. For example, cities with a diverse employment mix may be better insulated from shocks and primed to grow (Glaeser et al. 1992). Those with high employment in "new" or modern occupations might also be better able to withstand shocks. To evaluate these possibilities, we calculate city-level occupation and industry employment Herfindahl indexes in 1910 as measures of local economic dynamism (in the spirit of Jacobs 1970), and the fraction of employment in occupations that were "new" (locally or nationally rare in 1910), and identify cities above and below the median of the distribution—i.e., the cities with the least versus most diverse employment base, or the most modern occupation mix. Appendix Figures F.1 and F.2 show estimates of the effect of cutovers on employment in each group of cities. Point estimates remain insignificant and clustered around zero.

The effects might nevertheless vary with aggregate demand. Though our period intersects with the Great Depression, our setting is not well suited to comparing effects of cutovers in cities which had cutovers in the (expansionary) 1920s versus the (recessionary) 1930s for practical reasons like measurement (we observe outcomes in 1930, which is only the very beginning of the Great Depression, and 1940, which is in recovery) and statistical power. We instead measure cross-

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[28]We are indebted to Daron Acemoglu for suggesting and helping us frame this discussion.

sectional variation in Great Depression severity using county-level data on per-capita retail sales declines from 1929 to 1933 (Fishback et al. 2005), and in Appendix Figure F.3 and Appendix Table F.3 we estimate the effects of cutovers in cities differentially hit by the depression. We find negative effects of cutovers on young women's employment in cities with higher depression severity. Because we cannot pin these effects specifically to cutovers which coincided with the Great Depression, these results are only suggestive. But they raise the possibility that cutovers did not lead to measurable employment losses in our sample because in the years in our data, labor demand was relatively strong. If (or when) demand is weak, this may not be the case.

5.3   General equilibrium effects

Our focus thus far has been on employment rates, especially for young women. But young women's employment is co-determined with that of other groups and with wages in equilibrium. An implication of our results is that automation may have ripple effects on professions that workers transition into or take up instead. A natural question is whether these increased inflows of future cohorts of young women into other jobs displaced existing populations or drove down wages, potentially making what once was a "similar-paying" job a lower-paying one.

Because wages were first reported in the census in 1940, we cannot use the census data to study wage effects directly. But contextual evidence suggests that the elimination of telephone operation did not significantly affect wages in other jobs. The flow of young women away from telephone operation and into these occupations was small relative to these occupations' overall size, such that the wage effects are intrinsically likely to be muted. More explicit evidence is available from the NICB (1926) survey of clerical salaries, where relative wages across occupations are similar to those in the 1940 census data. Though these represent national (as opposed to local) distributions, recall that between 1926 and 1940, roughly half of the AT&T system was mechanized (Figure 1), and if these equilibrium effects were present we would have expected to see them.

The increased employment of young women in certain occupations post-cutover could nevertheless come at the expense of other categories of workers. In Appendix Tables F.4 and F.5, we estimate the change in total employment and employment rates for different worker categories in secretarial work and restaurant work. The evidence suggests different dynamics in each: because secretarial work was growing rapidly, it appears that most of the influx of would-be operators was absorbed by new demand. Restaurant work, on the other hand, did not experience statistically significant growth in total employment around cutovers, and the surge of young, white American-born women in the field may have come at the expense of black women (and possibly men). The results should be interpreted with caution, as the estimates are only marginally significant, but they suggest that the effects of automation may spill over to other populations. This possibility of cross-industry and

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cross-worker spillovers merits attention in future research.

6   Effects on Incumbent Telephone Operators

We also ask what happened to incumbent telephone operators after the installation of mechanical switching. Despite a great deal of contemporary interest, prior evidence is limited and anecdotal. In this section, we estimate effects of cutovers on women who were telephone operators in the most recent census, many of whose jobs were replaced by the new technology.

6.1   Empirical approach

Our empirical strategy is straightforward. Using our sample of women telephone operators in 1920 and 1930 (year t) linked to their next census record (in $t+10$), comparing them to a matched set of women from the same census enumeration district, and retaining our focus on women in "small" cities with population $\leq$100,000 in 1920, we estimate the effects of a cutover in the intervening decade on individual operators' outcomes ten years later:

$$Y_{ict}^{t+10} = \beta \cdot \mathbb{1}(\text{Cutover})_{ct} + \delta_{ct} + X_i\phi + \varepsilon_{ict} \tag{3}$$

where Y_{ict}^{t+10} represents an outcome in year $t+10$ for a woman i who lived in city c in year t, $\mathbb{1}(\text{Cutover})_{ct}$ indicates that city c was cut over to dial between t and $t+10$, δ_{ct} are city-year fixed effects, and X_i are individual-level controls.[29] In our most demanding specification, we replace the city-year fixed effects with operator-year fixed effects, which conditions comparisons to within individual operators and their associated control women. In the tables below, we present results pooling the 1920-30 and 1930-40 linked samples. We cluster all standard errors by city and use inverse propensity weights to account for selection in our linking procedure (Bailey et al. 2017).

6.2   Effects on incumbent telephone operators

We begin our analysis in Table 6 by studying the effects of cutovers on the probability that a year-t operator: (i) was still a telephone operator in the telephone industry in $t+10$, (ii) had a non-operator job in the telephone industry, or (iii) was an operator in another industry. We initially

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[29]This specification will thus estimate differential outcomes in the post-period of telephone operators which were versus were not subject to a cutover in the intervening decade, relative to outcomes of similar women from the same local area. The control group is matched on age ($\pm$ 5), sex, race, nativity, parents' nativity, marital status, and fertility, all measured in year t, and conditioned on having an occupation in year t. Individual controls consist of fixed effects for age, race, birthplace, and marital status in year t.

show results for year-t operators of all ages (Columns 1 and 2), and subsequently break out the effects for those age 16 to 20 (Columns 3 and 4), 21-25 (Columns 5 and 6), and 26+ (Columns 7 and 8). All columns include individual-level controls. Odd-numbered columns add city-year fixed effects, and even-numbered columns operator-year fixed effects.

[Table 6 about here]

Echoing our results from Section 4, cutovers significantly reduced the likelihood of employment as telephone operators in the telephone industry. Table 6, Panel (A) shows that women who were operators in the base year were 8 p.p. less likely to be operators ten years later if exposed to a cutover (Columns 1 and 2). This effect shaves roughly one-third off of the base rate at which these women continued working as telephone operators in non-cutover cities, relative to their matched controls. The cutover effects are largest for women aged 26+, the set of operators who were most likely to remain employed as operators without a cutover.

What did these former telephone operators do instead? Natural alternatives are other jobs in the telephone industry or working as a private switchboard operator in a different industry. However, the data reject the importance of either of these margins of adjustment. Former telephone operators were very unlikely to do either, independent of cutovers or as a result of them (Table 6, Panels B and C). Although the odds of working other jobs in the telephone industry or as a telephone operator in another industry increased modestly after a cutover for women under 25, these effects can only account for a small fraction of overall operator displacement (compare the magnitudes on the cutover interactions in Panel A with Panels B and C).

We show in Table 7, Panel (A) that cutovers pushed many incumbent operators out of the labor entirely. Operators who were over age 25 in the base year were roughly 7 p.p. less likely to still be working after a cutover, relative to peers in untreated cities—accounting for more than half of the displacement of operators in this age group. However, cutovers had smaller and less statistically precise effects on younger women's employment (those under 25 in the base year).

[Table 7 about here]

We supplement this evidence by studying in Panels (B) and (C) the likelihood that a year-t operator got married or had children between t and $t+10$ (conditional on initially having been single/having had no children in year t, respectively), since family may have been an alternative to work for this population and time period. The evidence suggests that cutovers may have increased the odds that older, unmarried operators subsequently wed, though the results are of marginal significance and small relative to base rates of entry into marriage for our sample. Cutovers had no discernible effects on marriage or fertility among younger operators.

In Table 8, we find that operators who continued working were roughly 11 p.p. (or early 40%) more likely than their peers to switch careers, and suggestive evidence that their new occupations were lower status after automation. Panel (A) estimates the effects of cutovers on the probability of changing occupation or industry, where career switching is visible. Though this change was all but implied for a job that was automated by a monopsonist employer, the results are similar when the outcome is an indicator for changing occupation alone or changing industry alone. In Panels (B) and (C), we estimate the effect of cutovers on log occupation score (a commonly-used occupation-level proxy for income, measuring occupations' median income—and which we calculate specifically for women in 1940, the first year that income is measured in the census) and the likelihood that a worker was in a lower-paying occupation in $t+10$ than in t.[30] The occupation score of operators who were exposed to cutovers and are still working a decade later on average fell by 5%, at the same time as their untreated peers' occupation scores increased 8%, with similar effects across ages. Roughly 10% of these women end up in a lower-paying job a decade later.

[Table 8 about here]

6.3 Connections to earlier results

Taken together, the results from Sections 5 and 6 suggest the effects of automation on employment may vary by age. Older incumbent workers are more adversely affected, while younger workers (including future generations not yet in the labor force, or not even yet born) are more adaptive to the changing labor market. This, in short, is where the two results meet.

These heterogeneous effects of automation by age echo findings from the larger literature on displacement. Though Jacobson et al. (1993) report that displaced workers' earnings losses "depend only slight on their age," subsequent work has documented age-based heterogeneity (Farber 1997, Deelen et al. 2018, Eliason and Storrie 2006). Older workers' skills are often firm-specific, sector-specific, or task-specific, making them difficult to transfer (e.g., Poletaev and Robinson 2008, Carrington 1993, Neal 1995, Gibbons and Waldman 2004). These difficulties are perhaps only magnified when displacement is driven by an automation event that eliminates an entire occupation and forecloses future opportunities, as is the case in this paper.

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[30]We study whether operators in year t were in higher- versus lower-paying jobs ten years later, rather than focusing on whether year-t operators transitioned into specific occupations after cutovers, because older women tended to be distributed across many more occupations. The occupation scores we construct for women in 1940 are analogous to those calculated and made widely available by IPUMS for the entire population in 1950.

7   Discussion and Conclusion

The automation of telephone operation is among the largest discrete automation shocks in U.S. history, and as such it provides a unique opportunity to study what happens when technology replaces an entire major category of work, and in particular an entire major entry-level occupation. Using panel variation in the local adoption of mechanical switching and population outcomes from complete count census data from 1910 to 1940, we show that dial cutovers presented a large negative shock to local labor demand for young, white, American-born women, with the number of young operators dropping by upwards of 80%—a near-total collapse in entry-level hiring in one of the country's largest occupations for young women—and accordingly around 2% of jobs for this group being permanently replaced by machines, essentially at the flip of a switch.

These 2% of jobs represented entry-level opportunities for several times as many young women, and the fear was that its automation might choke off future generations from the labor force. We find that this shock did not reduce future cohorts' employment rates. It appears comparable middle-skill office jobs and some lower-skill service sector jobs absorbed future generations of young workers, and did so fairly quickly, with only women of high school age on average ending up in lower-paying occupations than they would have been in otherwise. The adverse consequences of automation were concentrated in incumbent telephone operators, who were subsequently less likely to be working, and conditional on working, more likely to be in lower-paying occupations.

We consider these results to be a useful reference point in the modern discussion of the risks that automation poses to workers' and families' economic security today—and one that suggests that dislocations do occur, but that we are not likely to be staring down a "workless" future. However, an inevitable question is how much bearing this episode has for the present or the future. A second, equally important question is whether specific features of the historical setting contributed to labor market adjustments, and whether they are reproducible today.

Both questions merit attention. As we explained, the early 20th century was a time of rapid growth in women's educational attainment, labor demand, and labor force participation. Jobs which were growing in this period (like office work) were a natural source of countervailing labor demand, and in the view of some scholars, new uses for these workers may have even endogenously developed in response to automation. Although we find no evidence that mechanical switching was a response to general labor market tightening, if firms are more likely to automate work when labor demand is growing, this may intrinsically blunt any adverse effects on employment. In general, understanding how the effects of automation are mediated by local economic conditions, including secular trends or the business cycle, is an important subject for future work.

Equally important, however, may be that the necessity of complementary investments required that

cutovers be planned a few years in advance—a fact we see firsthand in newspaper reporting. This advance notice may have allowed young women nearing working age to adjust their educational investments and early career choices (or for their parents and high schools to do the same), mitigating the impact of the shock. A better understanding of how advance notice facilitates adjustments, and what form they take, is another opportunity for further research. Most automation threats today are likely to take place gradually and over longer horizons, and it seems unlikely that 2% of local jobs may spontaneously be automated with less warning.

This historical case study raises many other questions. For example, when the workplace is a key nexus for social ties (as it was for operators), automation or other shocks that eliminate jobs may also break or weaken these ties, or preclude them from forming at all. If so, industrial decline might link to declining community and social capital. Technological change may also have spillover effects from affected workers to their families, not only due to the resulting economic insecurity but also because in some blue-collar professions, jobs themselves can be intergenerationally transmitted. History provides fertile ground for further research on these and other questions, which we believe is warranted given growing concerns about automation today.

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Table 1: Employment of white, American-born women age 16-20 and 21-25, 1910-1940

	Age range: 16-20				Age range: 21-25			
	1910	1920	1930	1940	1910	1920	1930	1940
Population (1000s)	2427.6	2690.9	3618.4	4043.3	2295.9	2769.8	3509.4	4148.5
Working population (1000s)	1032.3	1215.6	1409.0	1143.0	865.8	1124.9	1556.9	1873.5
Labor force participation	42.5%	45.2%	38.9%	28.3%	37.7%	40.6%	44.4%	45.2%
Percent tel. oper. in tel. ind.	3.2%	4.5%	4.0%	1.3%	2.3%	3.3%	3.3%	1.5%
Percent tel. operators in tel. industry, by census region (%)								
Northeast	2.4	3.6	3.7	0.8	1.9	3.3	3.2	1.1
Midwest	3.6	4.8	4.2	1.4	2.5	3.3	3.3	1.6
South	3.5	5.6	4.4	1.7	2.3	3.1	3.2	1.6
West	5.2	6.8	4.0	2.4	2.8	4.4	3.5	2.4
Percent tel. operators in tel. industry, by city size (in 1920) (%)								
Population 2-5k	4.3	5.1	4.0	1.3	3.1	3.8	3.5	1.4
Population 5-10k	3.7	4.5	3.5	1.3	2.8	3.4	3.3	1.5
Population 10-20k	3.2	4.2	3.7	1.3	2.3	3.0	3.1	1.4
Population 20-50k	2.8	3.8	3.9	1.2	2.0	2.7	3.0	1.5
Population 50-100k	2.6	4.0	3.4	1.2	1.6	2.6	2.8	1.5
Population 100-200k	3.0	4.8	4.0	1.6	1.9	3.1	3.2	1.6
Population >200k	3.3	4.9	4.8	1.3	2.2	3.7	3.7	1.5

Notes: Table reports employment characteristics for white, American-born women age 16-20 and 21-25, by year. Employment rates in telephone operation are computed as a percentage of the working population. Breakdowns by city size are for the 3,027 cities in our primary sample (see Appendix B).

Table 2: Characteristics of telephone operators, 1910-1940

	Telephone industry				Other industries			
	1910	1920	1930	1940	1910	1920	1930	1940
Population (1000s)	73.03	134.63	182.04	152.70	2.40	5.74	22.83	41.17
Composition (%)								
Percent female	90.1	94.6	96.2	91.9	22.3	66.6	86.4	87.4
& native-born	86.7	90.8	92.3	88.8	20.9	62.7	82.1	83.9
& white/non-Hisp	86.3	90.5	92.0	88.2	20.8	61.8	81.3	83.1
& young (16-25)	71.8	68.5	59.2	27.8	14.4	39.8	41.0	22.2
Percent married	7.6	11.9	22.7	40.3	40.3	30.7	30.7	39.6
Percent has children	5.6	8.2	12.4	22.1	28.0	21.0	17.4	23.4

Notes: Table shows the number of telephone operators in the U.S. complete count Census data in the telephone industry and in other industries (i.e., at private company switchboards) from 1910 to 1940, as well as their demographic composition.

Table 3: Mean 1910 characteristics of cities by timing of earliest cutover

		Panel A: Unconditional means				
Characteristic	pre-1920	1921-1925	1926-1930	1931-1935	1936-1940	post-1940
Population 16+ (1000s)	38.92	116.82	43.87	18.41	9.14	4.06
	(10.14)	(31.40)	(7.49)	(3.31)	(1.71)	(0.13)
Average age	27.93	27.97	28.15	28.32	27.70	27.75
	(0.38)	(0.26)	(0.22)	(0.29)	(0.36)	(0.06)
Percent female	48.46	50.08	48.94	50.08	50.03	50.34
	(0.78)	(0.38)	(0.53)	(0.65)	(0.58)	(0.11)
Percent f/n/w/y	12.25	11.62	11.46	11.74	11.96	12.32
	(0.44)	(0.29)	(0.24)	(0.27)	(0.33)	(0.06)
Percent working	60.54	60.35	60.81	59.60	58.96	57.55
	(0.96)	(0.64)	(0.53)	(0.69)	(0.75)	(0.14)
Percent operators	0.19	0.21	0.19	0.17	0.19	0.21
	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.00)
F/n/w/y percent working	41.17	40.68	40.23	44.01	36.71	35.09
	(1.42)	(1.52)	(0.96)	(1.44)	(1.58)	(0.24)
F/n/w/y percent operators	1.16	1.36	1.19	1.02	1.12	1.21
	(0.12)	(0.14)	(0.08)	(0.08)	(0.10)	(0.02)
Unionized by 1920	0.17	0.26	0.19	0.09	0.08	0.03
	(0.07)	(0.06)	(0.04)	(0.04)	(0.04)	(0.00)
Had strike by 1920	0.07	0.10	0.09	0.03	0.03	0.01
	(0.05)	(0.04)	(0.03)	(0.02)	(0.02)	(0.00)

		Panel B: Conditional means, controlling for log adult population				
Characteristic	pre-1920	1921-1925	1926-1930	1931-1935	1936-1940	post-1940
Average age	26.78	26.64	27.07	27.49	27.09	27.37
	(0.42)	(0.32)	(0.27)	(0.32)	(0.36)	(0.08)
Percent female	48.76	50.43	49.22	50.30	50.19	50.44
	(0.86)	(0.51)	(0.65)	(0.69)	(0.61)	(0.16)
Percent f/n/w/y	13.76	13.38	12.88	12.83	12.77	12.82
	(0.49)	(0.38)	(0.32)	(0.30)	(0.33)	(0.09)
Percent working	56.67	55.83	57.18	56.80	56.90	56.27
	(1.05)	(0.79)	(0.69)	(0.73)	(0.73)	(0.22)
Percent operators	0.23	0.25	0.23	0.19	0.21	0.22
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)
F/n/w/y percent working	29.26	26.78	29.05	35.40	30.36	31.15
	(1.55)	(1.47)	(1.14)	(1.47)	(1.58)	(0.34)
F/n/w/y percent operators	1.17	1.37	1.21	1.03	1.13	1.22
	(0.14)	(0.18)	(0.11)	(0.10)	(0.11)	(0.03)
Unionized by 1920	0.01	0.06	0.04	-0.03	-0.01	-0.02
	(0.07)	(0.06)	(0.04)	(0.03)	(0.04)	(0.00)
Had strike by 1920	0.02	0.04	0.04	0.00	0.01	0.00
	(0.04)	(0.03)	(0.03)	(0.02)	(0.02)	(0.00)

Notes: Table reports mean 1910 characteristics of cities in our primary sample whose first cutover occurred in each of the periods shown (2,992 cities included in this table, omitting 31 cities with cutovers with ambiguous timing and New York City boroughs). Panel (A) reports unconditional means, and Panel (B) reports conditional means after controlling for each city's log adult (16+) population. Each row presents estimates from a regression of the variable in the left-most column on indicators for whether the city had its first cutover in the interval represented by each of the remaining columns. We also report operator unionization and strike activity (nearly all of which occurred in the 1910s). Percentages are measured in whole units (out of 100). Population and population percentages reflect the adult population only, and f/n/w/y is shorthand for female, American-born, white/non-Hispanic, and young (age 16 to 25). Note that the dial era in the AT&T system began in 1919, such that cutovers pre-1920 are nearly all by independents. The final column consists of cities that do not have a cutover in our data by April 1, 1940. Heteroskedasticity-robust SEs in parentheses.

Table 4: Changes in work, education, marriage, and fertility patterns around cutovers

Panel A: White, American-born women ages 16 to 18

| | Fraction of the group that is: | | | | |
	Tel. oper.	Working	In school	Married	Has children
Post-cutover	-0.007***	-0.001	0.001	-0.002	0.001
	(0.001)	(0.009)	(0.008)	(0.002)	(0.001)
N	11652	11652	11652	11652	11652
R^2	0.55	0.83	0.76	0.80	0.68
Cities	2913	2913	2913	2913	2913
Cut over	261	261	261	261	261
Y Mean	0.01	0.30	0.53	0.09	0.03

Panel B: White, American-born women ages 19 to 22

| | Fraction of the group that is: | | | | |
	Tel. oper.	Working	In school	Married	Has children
Post-cutover	-0.008***	-0.002	-0.001	0.003	0.004
	(0.001)	(0.006)	(0.003)	(0.004)	(0.003)
N	11656	11656	11656	11656	11656
R^2	0.51	0.81	0.79	0.84	0.79
Cities	2914	2914	2914	2914	2914
Cut over	261	261	261	261	261
Y Mean	0.01	0.48	0.11	0.36	0.19

Panel C: White, American-born women ages 23 to 25

| | Fraction of the group that is: | | | | |
	Tel. oper.	Working	In school	Married	Has children
Post-cutover	-0.005***	-0.000	-0.001	0.002	0.002
	(0.001)	(0.006)	(0.001)	(0.004)	(0.004)
N	11656	11656	11656	11656	11656
R^2	0.43	0.77	0.66	0.80	0.77
Cities	2914	2914	2914	2914	2914
Cut over	261	261	261	261	261
Y Mean	0.01	0.41	0.02	0.60	0.38

Notes: Tables present difference-in-difference estimates, by age, of the effects of local dial adoption on the fraction of young, white, American-born women in successive cohorts who are in the labor force, in school, married, and have children, for cities with population $\leq$100k in 1920. The left-most column provides the effect of cutovers on the fraction of these women who were telephone operators in the telephone industry, as a reference point. All regressions include city and year fixed effects, and log city size x year controls. *, **, *** represent significance at the 0.1, 0.05, and 0.01 levels, respectively. SEs clustered by city in parentheses.

Table 5: Changes in employment shares in select occupations around cutovers

Panel A: White, American-born women ages 16 to 18

Conditional on working, fraction employed as or in

	Tel. oper.	Off. mach.	Typist/secr.	Office clerk	Sales clerk	Beautician	Restaurant
Post-cutover	-0.022***	0.000	0.007*	-0.001	0.002	0.002**	0.014***
	(0.003)	(0.000)	(0.004)	(0.003)	(0.004)	(0.001)	(0.004)
N	11613	11613	11613	11613	11613	11613	11613
R^2	0.46	0.36	0.58	0.62	0.50	0.39	0.56
Cities	2913	2913	2913	2913	2913	2913	2913
Cut over	261	261	261	261	261	261	261
Y Mean	0.03	0.00	0.08	0.04	0.11	0.01	0.05

Panel B: White, American-born women ages 19 to 22

Conditional on working, fraction employed as or in

	Tel. oper.	Off. mach.	Typist/secr.	Office clerk	Sales clerk	Beautician	Restaurant
Post-cutover	-0.017***	0.000	0.009***	-0.003	0.003	0.001	0.006***
	(0.002)	(0.000)	(0.003)	(0.003)	(0.003)	(0.001)	(0.002)
N	11642	11642	11642	11642	11642	11642	11642
R^2	0.50	0.55	0.76	0.73	0.57	0.66	0.72
Cities	2914	2914	2914	2914	2914	2914	2914
Cut over	261	261	261	261	261	261	261
Y Mean	0.03	0.00	0.14	0.05	0.10	0.01	0.04

Panel C: White, American-born women ages 23 to 25

Conditional on working, fraction employed as or in

	Tel. oper.	Off. mach.	Typist/secr.	Office clerk	Sales clerk	Beautician	Restaurant
Post-cutover	-0.010***	0.000	0.003	-0.004	0.000	0.002*	0.004**
	(0.001)	(0.000)	(0.004)	(0.003)	(0.003)	(0.001)	(0.002)
N	11633	11633	11633	11633	11633	11633	11633
R^2	0.39	0.54	0.72	0.67	0.49	0.65	0.69
Cities	2914	2914	2914	2914	2914	2914	2914
Cut over	261	261	261	261	261	261	261
Y Mean	0.02	0.00	0.13	0.05	0.09	0.01	0.04

Notes: Tables present difference-in-difference estimates, by age, of the effects of local dial adoption on young, white, American-born women's employment shares in select occupations, across successive cohorts, for cities with population ≤100k in 1920. The left-most column provides the effect of cutovers on the fraction of these women who were telephone operators in the telephone industry, as a reference point. The other occupations across columns are: (i) office machine operators, (ii) typists, stenographers, and secretaries, (iii) other office clerks, (iv) sales clerks, (v) beauty parlor workers, and (vi) restaurant workers. All regressions include city and year fixed effects, and log city size x year controls. *, **, *** represent significance at the 0.1, 0.05, and 0.01 levels, respectively. SEs clustered by city in parentheses.

Table 6: Effects of dial cutovers on the probability of being a telephone operator or having a non-operator job in the telephone industry

	All Ages		16-20		21-25		26+	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)

Panel A: Is telephone operator in telephone industry?

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Operator × Post-cutover	-0.087***	-0.081***	-0.050***	-0.050***	-0.074***	-0.083***	-0.139***	-0.120***
	(0.013)	(0.014)	(0.012)	(0.013)	(0.017)	(0.020)	(0.021)	(0.029)
Operator	0.246***	0.240***	0.172***	0.167***	0.246***	0.253***	0.395***	0.405***
	(0.006)	(0.006)	(0.007)	(0.007)	(0.009)	(0.010)	(0.012)	(0.014)
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City × Year FEs	Yes	No	Yes	No	Yes	No	Yes	No
Operator FEs	No	Yes	No	Yes	No	Yes	No	Yes
Observations	153,752	153,752	72,760	72,760	60,306	60,306	20,686	20,686
Adjusted R²	0.22	0.44	0.15	0.35	0.21	0.41	0.34	0.55
Y Mean	0.045	0.045	0.032	0.032	0.037	0.037	0.117	0.117

Panel B: Has other job in telephone industry?

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Operator × Post-cutover	0.010***	0.010**	0.010***	0.014***	0.005	0.004	0.017*	0.023*
	(0.003)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.010)	(0.013)
Operator	0.019***	0.018***	0.012***	0.011***	0.020***	0.020***	0.034***	0.030***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)	(0.005)
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City × Year FEs	Yes	No	Yes	No	Yes	No	Yes	No
Operator FEs	No	Yes	No	Yes	No	Yes	No	Yes
Observations	153,752	153,752	72,760	72,760	60,306	60,306	20,686	20,686
Adjusted R²	0.03	0.28	0.02	0.21	0.02	0.24	0.04	0.41
Y Mean	0.007	0.007	0.006	0.006	0.006	0.006	0.014	0.014

Panel C: Is telephone operator in other industry?

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Operator × Post-cutover	0.004	0.003	0.003	0.005	0.009*	0.012*	0.008	0.005
	(0.004)	(0.005)	(0.006)	(0.006)	(0.005)	(0.006)	(0.009)	(0.010)
Operator	0.023***	0.023***	0.017***	0.016***	0.021***	0.019***	0.037***	0.037***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)	(0.006)
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City × Year FEs	Yes	No	Yes	No	Yes	No	Yes	No
Operator FEs	No	Yes	No	Yes	No	Yes	No	Yes
Observations	153,752	153,752	72,760	72,760	60,306	60,306	20,686	20,686
Adjusted R²	0.04	0.32	0.06	0.27	0.05	0.31	0.07	0.47
Y Mean	0.005	0.005	0.004	0.004	0.005	0.005	0.013	0.013

Notes: Table reports effect of cutovers on decade-later outcomes of women who reported being telephone operators in the telephone industry in a given census year, as a function of whether their city had its first cutover in the intervening decade, relative to a matched control group. Sample restricted to women in small city sample (population ≤100k in 1920) in the base year. Individual controls include fixed effects for age, birthplace, race, and marital status, all measured in the base year. Operator fixed effects apply to each operator and the associated control women. Following Bailey et al. (2017), we use inverse propensity weights to adjust for observable differences between matched and unmatched persons in our linked sample. *, **, *** represent significance at the 0.1, 0.05, and 0.01 levels, respectively. SEs clustered by city in parentheses.

Table 7: Effects of dial cutovers on the probability of working, getting married, or having children

|	All Ages		16-20		21-25		26+	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Still working?								
Operator × Post-cutover	-0.042***	-0.036***	-0.041**	-0.028	-0.026	-0.032	-0.066***	-0.075***
	(0.012)	(0.013)	(0.020)	(0.020)	(0.017)	(0.023)	(0.021)	(0.028)
Operator	0.021***	0.019***	0.018**	0.012	0.009	0.014	0.043***	0.050***
	(0.006)	(0.006)	(0.009)	(0.010)	(0.011)	(0.013)	(0.013)	(0.016)
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City × Year FEs	Yes	No	Yes	No	Yes	No	Yes	No
Operator FEs	No	Yes	No	Yes	No	Yes	No	Yes
Observations	153,752	153,752	72,760	72,760	60,306	60,306	20,686	20,686
Adjusted R^2	0.06	0.09	0.04	0.09	0.05	0.07	0.10	0.13
Y Mean	0.406	0.406	0.349	0.349	0.418	0.418	0.571	0.571
Panel B: Got married? (cond'l on unmarried in pre-period)								
Operator × Post-cutover	0.011	-0.001	0.025	0.007	-0.020	-0.020	0.031	0.063*
	(0.010)	(0.012)	(0.017)	(0.016)	(0.016)	(0.021)	(0.029)	(0.035)
Operator	0.037***	0.039***	0.028***	0.032***	0.045***	0.042***	0.032*	0.028
	(0.006)	(0.006)	(0.008)	(0.009)	(0.010)	(0.013)	(0.017)	(0.019)
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City × Year FEs	Yes	No	Yes	No	Yes	No	Yes	No
Operator FEs	No	Yes	No	Yes	No	Yes	No	Yes
Observations	142,897	142,897	71,748	71,748	56,680	56,680	14,469	14,469
Adjusted R^2	0.10	0.13	0.06	0.11	0.06	0.07	0.10	0.11
Y Mean	0.712	0.712	0.787	0.787	0.688	0.688	0.430	0.430
Panel C: Had children? (cond'l on none in pre-period)								
Operator × Post-cutover	0.020*	0.011	0.018	0.007	0.006	0.003	0.034	0.006
	(0.012)	(0.015)	(0.019)	(0.022)	(0.017)	(0.023)	(0.022)	(0.026)
Operator	0.007	0.006	0.018*	0.021**	0.006	-0.009	-0.016	-0.018
	(0.007)	(0.007)	(0.010)	(0.011)	(0.011)	(0.013)	(0.011)	(0.013)
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City × Year FEs	Yes	No	Yes	No	Yes	No	Yes	No
Operator FEs	No	Yes	No	Yes	No	Yes	No	Yes
Observations	149,371	149,371	72,635	72,635	59,521	59,521	17,215	17,215
Adjusted R^2	0.08	0.11	0.04	0.09	0.05	0.08	0.10	0.14
Y Mean	0.512	0.512	0.586	0.586	0.489	0.489	0.277	0.277

Notes: Table reports effect of cutovers on decade-later outcomes of women who reported being telephone operators in the telephone industry in a given census year, as a function of whether their city had its first cutover in the intervening decade, relative to a matched control group. Sample restricted to women in small city sample (population ≤100k in 1920) in the base year. Individual controls include fixed effects for age, birthplace, race, and marital status, all measured in the base year. Operator fixed effects apply to each operator and the associated control women. Following Bailey et al. (2017), we use inverse propensity weights to adjust for observable differences between matched and unmatched persons in our linked sample. *, **, *** represent significance at the 0.1, 0.05, and 0.01 levels, respectively. SEs clustered by city in parentheses.

Table 8: Effects of dial cutovers on the probability of persisting in the same occupation/industry and future occupation scores

	All Ages		16-20		21-25		26+	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)

Panel A: Still working in same occupation and industry? (cond'l on still working)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Operator × Post-cutover	-0.109***	-0.107***	-0.121***	-0.126***	-0.099***	-0.156***	-0.125***	-0.085
	(0.020)	(0.023)	(0.028)	(0.034)	(0.033)	(0.038)	(0.033)	(0.056)
Operator	0.319***	0.324***	0.329***	0.336***	0.318***	0.340***	0.339***	0.341***
	(0.010)	(0.012)	(0.015)	(0.019)	(0.017)	(0.021)	(0.018)	(0.022)
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City × Year FEs	Yes	No	Yes	No	Yes	No	Yes	No
Operator FEs	No	Yes	No	Yes	No	Yes	No	Yes
Observations	62,395	62,395	25,401	25,401	25,188	25,188	11,806	11,806
Adjusted R^2	0.13	0.17	0.13	0.20	0.11	0.11	0.15	0.17
Y Mean	0.278	0.278	0.209	0.209	0.292	0.292	0.394	0.394

Panel B: Log occupation score

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Operator × Post-cutover	-0.056***	-0.056***	-0.061***	-0.061**	-0.066***	-0.066**	-0.041**	-0.026
	(0.010)	(0.014)	(0.017)	(0.025)	(0.018)	(0.026)	(0.016)	(0.025)
Operator	0.076***	0.080***	0.107***	0.118***	0.058***	0.066***	0.082***	0.085***
	(0.007)	(0.008)	(0.010)	(0.014)	(0.010)	(0.011)	(0.012)	(0.014)
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City × Year FEs	Yes	No	Yes	No	Yes	No	Yes	No
Operator FEs	No	Yes	No	Yes	No	Yes	No	Yes
Observations	56,184	56,184	22,271	22,271	22,974	22,974	10,939	10,939
Adjusted R^2	0.10	0.12	0.17	0.17	0.12	0.11	0.14	0.09
Y Mean	2.071	2.071	2.032	2.032	2.099	2.099	2.095	2.095

Panel C: Decline in occupation score decile

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Operator × Post-cutover	0.105***	0.101***	0.074***	0.062*	0.125***	0.186***	0.090***	0.061
	(0.018)	(0.022)	(0.024)	(0.032)	(0.029)	(0.044)	(0.029)	(0.039)
Operator	0.014	0.001	0.063***	0.053***	-0.003	-0.010	-0.025	-0.051***
	(0.009)	(0.011)	(0.015)	(0.020)	(0.016)	(0.020)	(0.016)	(0.019)
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City × Year FEs	Yes	No	Yes	No	Yes	No	Yes	No
Operator FEs	No	Yes	No	Yes	No	Yes	No	Yes
Observations	47,736	47,736	18,446	18,446	19,612	19,612	9,678	9,678
Adjusted R^2	0.04	0.08	0.08	0.14	0.07	0.08	0.02	0.08
Y Mean	0.217	0.217	0.232	0.232	0.215	0.215	0.192	0.192

Notes: Table reports effect of cutovers on decade-later outcomes of women who reported being telephone operators in the telephone industry in a given census year, as a function of whether their city had its first cutover in the intervening decade, relative to a matched control group. Sample restricted to women in small city sample (population ≤100k in 1920) in the base year. Individual controls include fixed effects for age, birthplace, race, and marital status, all measured in the base year. Operator fixed effects apply to each operator and the associated control women. Following Bailey et al. (2017), we use inverse propensity weights to adjust for observable differences between matched and unmatched persons in our linked sample. *, **, *** represent significance at the 0.1, 0.05, and 0.01 levels, respectively. SEs clustered by city in parentheses.

Figure 1: Percent of Bell system on dial, 1913-1972
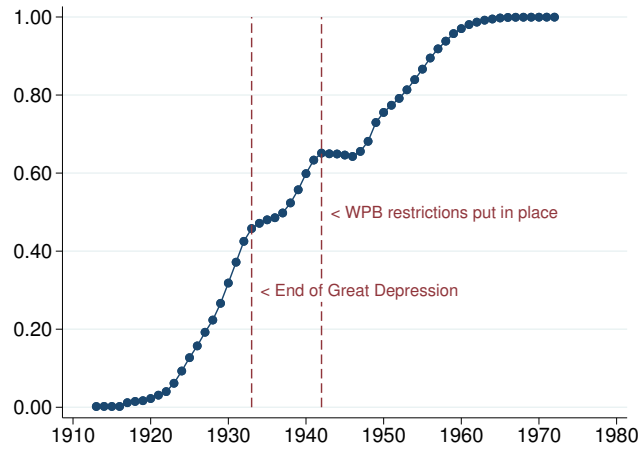
Notes: Figure shows the fraction of Bell system telephones with mechanical operation (i.e., dial) over time. Data from "Bell System Distributions of Company Telephones," AT&T Archives and History Center, box 85-04-03-02. Note that adoption investments declined during the Great Depression, leading to a slowdown in the late 1930s, and War Production Board restrictions on the use of copper during World War II effectively halted installations for the duration of the war.

Figure 2: Cities in data with cutovers by 1940

Panel (A): AT&T data (bubbles proportional to % of subscribers cut over by 1940)
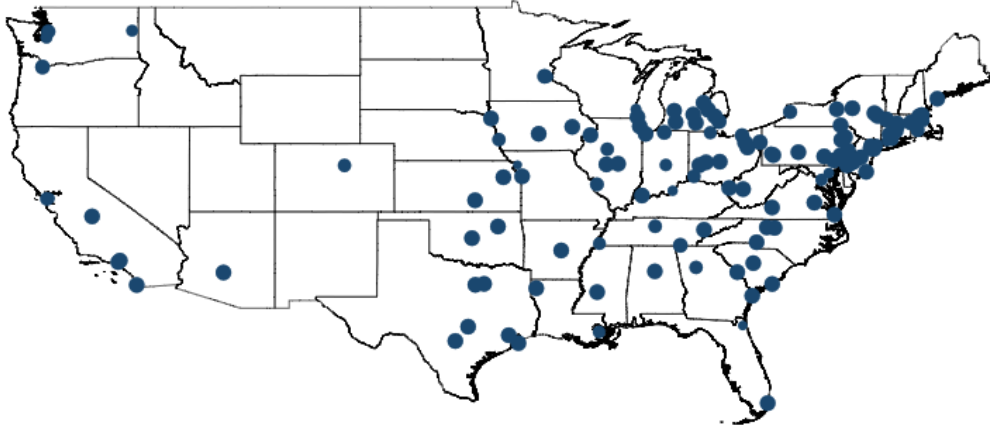
Panel (B): Newspaper data (bubbles proportional to # of cutovers through 1940)
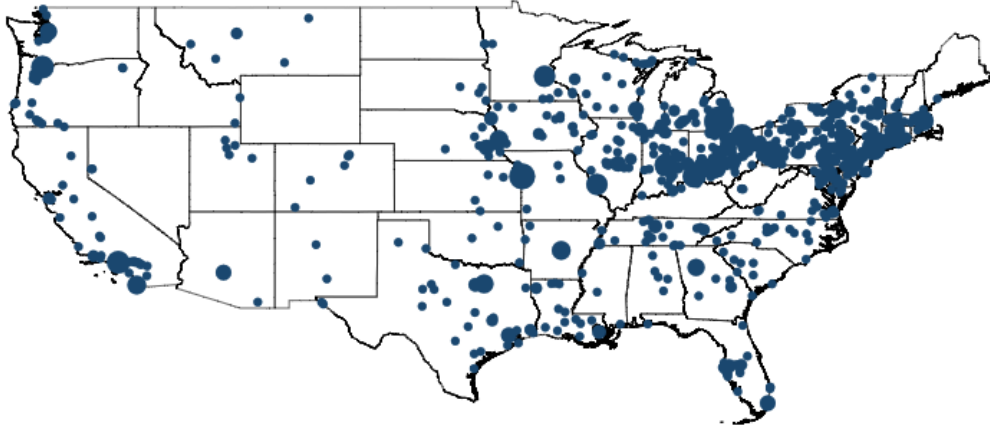
Notes: Figure maps the cities with a dial cutover in the AT&T data (top panel) and newspapers data (bottom panel) through 1940. Bubble sizes are proportional to percent of local subscribers cut over by 1940 for the AT&T data, and to the # of reported cutovers through 1940 for the newspaper data.

Figure 3: Effect of dial cutovers on the log number of young, white, American-born women who are telephone operators in the telephone industry (event study, 10- and 2-yr intervals)
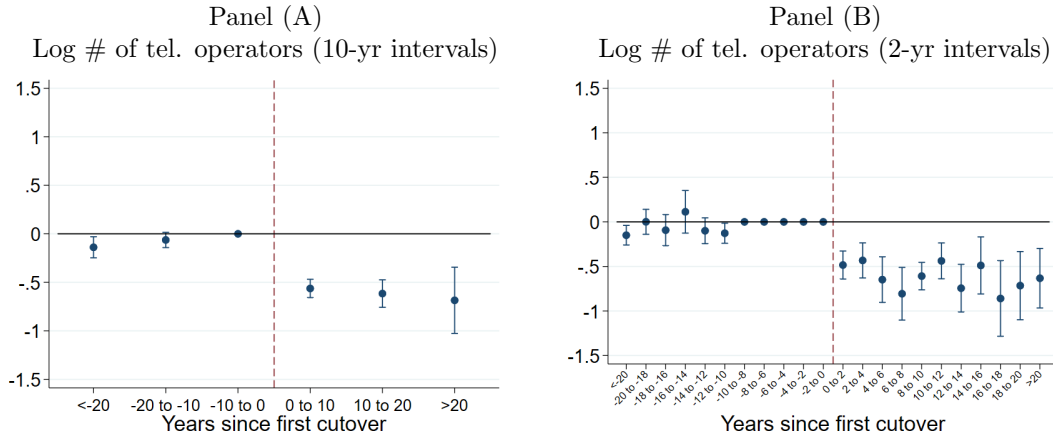
Panel (A)
Log # of tel. operators (10-yr intervals)

Panel (B)
Log # of tel. operators (2-yr intervals)

Notes: Figure shows event study estimates of the effects of local dial adoption on the (log) number of young, white, American-born women in successive cohorts who are telephone operators in the telephone industry, for the small city sample (population ≤100k in 1920), with 10- and 2-year event windows. When event windows are narrower than the 10-year frequency at which outcomes are measured, each bin contains different cities (every fifth bin represents the same set of cities). Error bars represent 95% confidence intervals, computed from SEs clustered at the city level.

Figure 4: Effect of dial cutovers on the fraction of working young, white, American-born women who are telephone operators in the telephone industry (event study and DID by age)

Panel (A)
Fraction who are tel. operators (2-yr intervals)

Panel (B)
Fraction who are tel. operators (DID by age)

Notes: Figure shows event study and difference-in-difference estimates (by age) of the effects of local dial adoption on the fraction of working young, white, American-born women in successive cohorts who are telephone operators in the telephone industry, for the small city sample (population ≤100k in 1920). Error bars represent 95% confidence intervals, computed from SEs clustered at the city level.

Figure 5: Effect of dial cutovers on the fraction of working young, white, American-born women with other jobs in the telephone industry or who are telephone operators in other industries

Panel (A)
Other jobs in the tel. industry (2-yr intervals)

Panel (B)
Tel. operator in other industries (2-yr intervals)

Panel (C)
Other jobs in the tel. industry (DID by age)

Panel (D)
Tel. operator in other industries (DID by age)
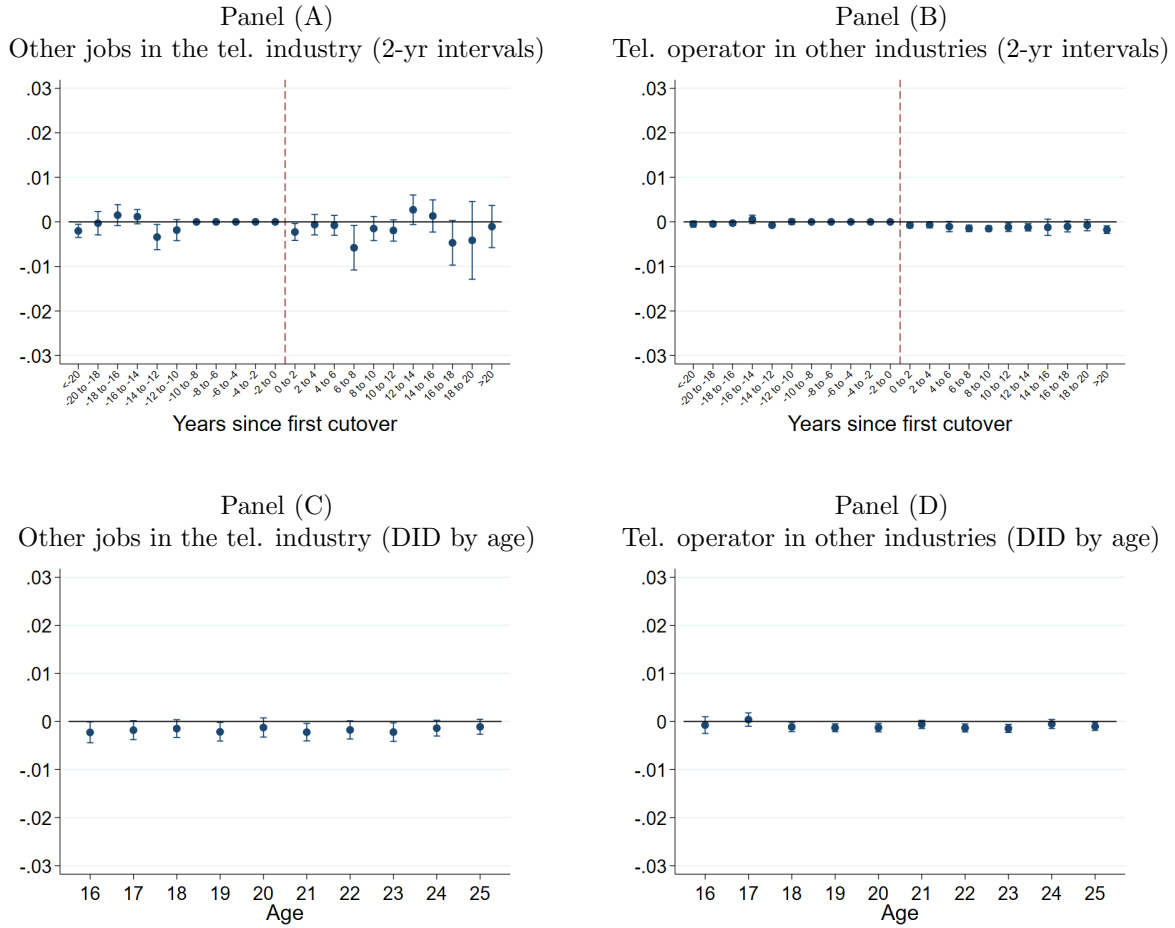
Notes: Upper panels show event study estimates of the effects of local dial adoption on the fraction of working young, white, American-born women in successive cohorts who have other jobs in the telephone industry (left) and who are telephone operators in other industries (right), for the small city sample (population ≤100k in 1920). Because event windows are narrower than the 10-year frequency at which outcomes are measured, each bin contains different cities (every fifth bin represents the same set of cities). Lower panels show the associated difference-in-differences estimates, by age. We plot the estimates on the same scale (-0.03 to 0.03) as the previous figures to ease comparison. Error bars represent 95% confidence intervals, computed from SEs clustered at the city level.

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Web Appendix

A   Historical Details

This appendix provides supplementary material to accompany the discussion of U.S. telephone industry history and mechanical switching in Section 1. Table A.1 provides descriptive statistics on the U.S. telephone industry from 1902 to 1932. Table A.2 lists annual employment in the 1920s at the three largest U.S. employers. Figures A.1 and A.2 reproduce two tables from BLS (1932), a government study of the effects of dial on operator employment. Figure A.3 shows example newspaper headlines from articles describing dial cutovers and their effects on young women. Figure A.4 shows a map of Bell System operating companies as of 1922.

Table A.1: U.S. telephone industry, 1902-1932

	1902	1907	1912	1917	1922	1927	1932
Growth of industry							
Miles of wire (1000s)	4,900	12,999	20,248	28,827	37,266	63,836	87,678
Telephones (1000s)	2,371	6,119	8,730	11,717	14,347	18,523	17,424
Telephone calls (MMs)	5,071	11,373	13,736	21,846	24,648	31,614	30,048
Telephone calls (per capita)	64	131	144	212	224	266	241
Employees	78,752	144,169	183,361	262,629	312,015	375,272	334,085
Male				91,510	104,433	131,802	128,677
Female				171,119	207,582	243,470	205,408
Labor productivity							
Employees per MM calls	15.53	12.68	13.35	12.02	12.66	11.87	11.12
Male				4.19	4.24	4.17	4.28
Female				7.83	8.42	7.70	6.84
Market share							
AT&T share	56%	51%	58%	63%	66%	74%	79%

Notes: Data from U.S. Census of Electrical Industries, 1907-1932. Enumeration covered all Bell and independent operating companies. Call volume and employment reported for 1912 is restricted to companies with >$5,000 in income (in 1912 dollars) and is thus slightly understated. Operating revenue figures in 1902 are for all companies; in 1907-1917, for companies with >$5,000 income; and in 1922-1932, for companies with >$10,000 income.

Table A.2: Three largest U.S. employers, 1920-1929

Year	Employment (1000s)		
	Pennsylvania RR	U.S. Steel	AT&T
1920	280.7	267.3	231.3
1921	218.9	191.7	224.3
1922	243.5	214.9	243.1
1923	247.4	260.8	272.0
1924	218.6	246.8	279.7
1925	213.9	249.8	293.1
1926	217.7	253.2	300.6
1927	201.6	231.5	308.9
1928	186.3	221.7	334.3
1929	183.2	225.0	364.0

Notes: Table reports employment at the three largest U.S. employers throughout the 1920s. Employment figures obtained from annual reports and represent year-end employment at each company, except for the Pennsylvania Railroad from 1923-1929, when only average monthly employment was provided.

Figure A.1: BLS (1932) Table 1, "Telephones, Telephone Calls, and Personnel of a Single-Office Exchange Now 98 Percent Dial"

Item	1925	1926	1927	1928	1929	1030
Telephones:						
Manual	18,644	20,042	380	478	487	466
Dial			20,880	22,173	22,945	22,450
Average number of calls per month:						
Local exchange	3,368,989	3,575,102	3,438,496	4,195,446	3,953,536	4,019,404
Toll	60,321	65,302	73,381	87,540	83,007	93,177
Personnel:						
Experienced switchboard operators	214	244	186	157	123	120
Operators in training	74	35	1	0	0	4
Central office installation and maintenance men	13	13	20	19	18	25
All employees (all classes):						
Male	102	155	147	115	123	113
Female	307	297	213	179	166	160
Total	400	452	360	294	280	273

Notes: Figure reproduces Table 1 from BLS (1932), describing changes in employment at a single anonymous exchange which converted from manual to mechanical operation in 1927.

Figure A.2: BLS (1932) Table 2, "Changes in Employment Opportunities for Operators"

Exchange or company	Per cent of dial telephones after change to dial system	Per cent of increase in telephone calls during transition period	Number of operators			Loss in employment opportunities for operators	
			Before change to dial system	After change to dial system	If operators had increased in same ratio as calls	Estimated number	Per cent
No. 1	84.4	[1]14.7	33	15	28	13	46.4
No. 2	96.7	10.9	60	27	67	40	59.7
No. 3	100.0	53.1	33	16	51	35	68.6
No. 4	100.0	134.2	42	24	98	74	[3]75.5
No. 5	11.3	171.4	110	270	323	53	16.4
No. 6	100.0	13.8	156	63	180	126	66.7
No. 7	72.5	26.2	99	75	125	50	40.0
No. 8	100.0	48.0	169	80	250	170	68.0
No. 9	100.0	60.6	154	94	247	153	61.9
No. 10	100.0	36.7	228	114	312	198	63.5
No. 11	100.0	24.0	165	15	205	100	[3]92.7
No. 12	100.0	114.0	127	19	272	253	[3]93.0
No. 13	100.0	33.8	213	95	285	190	66.7
No. 14	98.0	19.9	288	124	345	221	64.1
No. 15	95.6	34.6	455	111	612	501	[1]81.9
No. 16	100.0	46.4	232	17	340	323	[3]95.0
No. 17	100.0	23.0	456	182	561	379	67.6
No. 18	100.0	12.1	591	310	663	353	[4]53.2
No. 19	70.0	50.5	740	560	1,114	554	49.7
No. 20	48.0	18.3	2,705	1,656	3,200	1,546	48.3

[1] Decrease.
[2] Large displacement due to small proportion of toll calls.
[3] Large displacement due to specialized nature of business.
[4] Small displacement due to part-time and similar labor policies and to recency of change to dial.

Notes: Figure reproduces Table 2 from BLS (1932), describing changes in employment at 18 surveyed exchanges which converted from manual to mechanical operation.

Figure A.3: Sampling of newspaper headlines, 1925 to 1940

Telephone Dial Usurps Jobs
By Frederic J. Haskin

Syndicated article, published in newspapers nationwide in 1932

HELLO GIRL SAYS GOODBYE FOREVER AND CITY "DIALS"

Employed 120 Girls

There were approximately 120 girls employed by the Bell Telephone company as operators prior to today. In a few weeks, but half that number will be on the payroll.

Enquirer and Evening News, Battle Creek, MI, 1927

PHONE GIRLS TO CONTINUE JOBS

Change-Over to Dials Will Not Cause Unemployment, Says Manager

Operation of a dial system at the proposed new exchange building of the Mountain States Tel. & Tel Co. will not result in layoff of many telephone switchboard operators and may even bring about added employment, Frank D. Sawyer, district manager of the company, informed Mayor Fred M. Abbott today.

Ogden Standard-Examiner, Ogden, UT, 1940

Dials Will Replace 500 Phone Girls

New Haven, March 6.—(Special.) —Five hundred telephone operators in the New Haven district, will lose their jobs April 19 when the change from manual to dial telephones here is made. Only 200 girls will be required as switchboard operators in place of the 700 now employed.

Hartford Daily Courant, Hartford, CT, 1930

Q. To what extent do automatic telephones do away with telephone operators?—R. N. M.

A. The Automatic Electric company says that when a telephone system in a small town or city of moderate size is converted to automatic, all of the operators formerly engaged in setting up local connections are eliminated leaving only sufficient operators to take care of long distance service, information, complaint, etc. These special services require the same operating force as the former manual system. The percentage of operators used for local service is a variable and runs from 30 or 40 per cent to 90 per cent depending on the size of the exchange and the local traffic or service conditions. In a large city, when conversion is made, one or two offices at a time, the question of operator reduction becomes very complex but in a very general way the same thing may be said to be true, that is, that all operators formerly engaged in handling local traffic are eliminated.

Arizona Republican, Phoenix, AZ, 1925
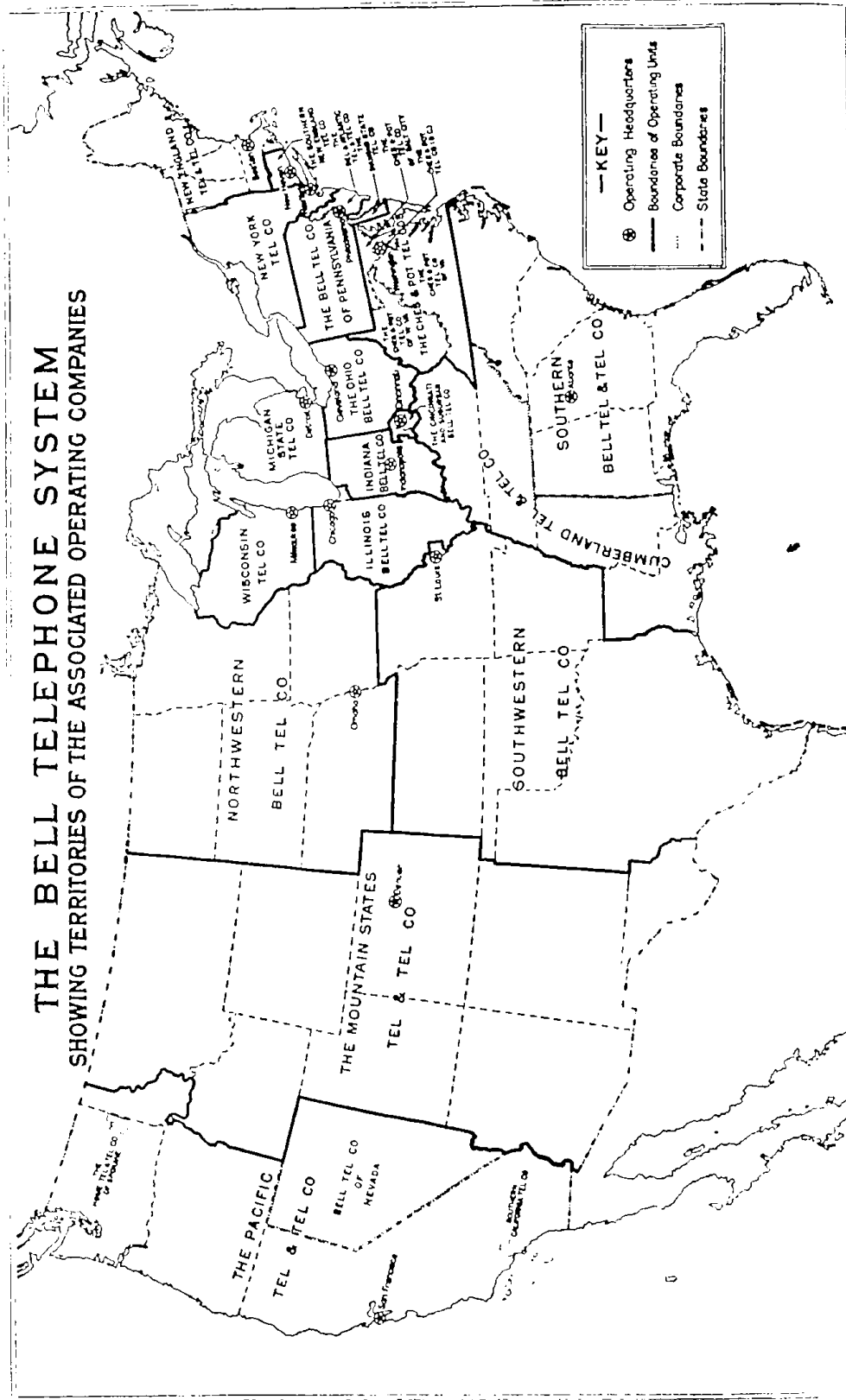
CUPID BEATS OUT OLD MAN HUNGER

FORT WAYNE, Ind., May 28.— When the Home Telephone Company changes over to the automatic system at midnight Friday, approximately 250 girls will be out of employment. Twenty-five girls, however, will not long be without "something to do." Today they announced they were soon to be united in the bonds of matrimony. Nine of the girls will be married Saturday and the others within the next two weeks.

Indianapolis Star, Indianapolis, IN, 1925

Notes: Figure reproduces newspaper headlines and/or content from the following articles: "Telephone Dial Usurps Jobs" (syndicated); "Hello Girl Says Goodbye Forever and City 'Dials,'" *The Enquirer and Evening News* (Battle Creek, MI), September 4, 1927; "Dials Will Replace 500 Phone Girls," *The Hartford Daily Courant* (Hartford, CT), March 7, 1930; "To what extent do automatic telephones do away with telephone operators?" *The Arizona Republican* (Phoenix, AZ), July 5, 1925; "Phone Girls to Continue Jobs," *The Ogden Standard-Examiner* (Ogden, UT), January 25, 1940; "Cupid Beats Out Old Man Hunger," *The Indianapolis Star* (Indianapolis, IN), May 29, 1925. All articles accessed from Newspapers.com.

Figure A.4: Map of the Bell regional operating companies, from AT&T's 1922 annual report

THE BELL TELEPHONE SYSTEM
SHOWING TERRITORIES OF THE ASSOCIATED OPERATING COMPANIES

—KEY—
⊗ Operating Headquarters
—— Boundaries of Operating Units
···· Corporate Boundaries
--- State Boundaries

Notes: Figure marks the boundaries of the various Bell operating companies' territories across the united states, and the headquarters city for each. Map from AT&T's 1922 annual report.

4

B Data Appendix

B.1 Data on dial cutover location/timing

We collect data on the local adoption of mechanical call switching (dial) from two sources: records at the AT&T archives which report dial penetration in cities with population >50,000 in the 1930s, and local newspaper reports, which cover cities large and small across the country.

To understand the cutover data collection it is useful to first recall the process by which cutovers took place. Although the AT&T corporate office (specifically, AT&T's chief engineer) gave general guidance to the regional operating subsidiaries on the adoption of dial—including information on the performance of dial vs. manual operation in different-sized markets and under different operating conditions—the decision to convert any single telephone exchange from manual to dial was made by the management of the operating companies themselves. This decision would set in motion a multi-year planning and installation process: exchange buildings had to be expanded or built, new switching equipment had to be installed, and new telephone directories and dial telephone sets had to be distributed to subscribers, who in turn had to be taught how to use them when dial service began. Judging from the newspaper reporting which we describe below, the date that telephone service would convert to dial was fixed in advance, but sometimes experienced (usually modest) delays. On the designated day—usually at midnight on a Saturday, when call volumes were lowest—technicians would physically cut the wires out of the manual switchboards, and connect them to the mechanical equipment (hence the term "cutover"). The actual cutting-over took only a few minutes, after which local calls were mechanically operated. In small cities and rural areas with at most a few telephone exchanges, these would typically all be cut over together. In larger cities with many to hundreds of telephone exchanges (New York had hundreds), these conversions effectively took place one exchange/neighborhood at a time, such that in these cities, telephone service was automated in a more piecemeal fashion over years or decades.

Data from AT&T's corporate archives

Because AT&T cutover decisions were decentralized, there is no single source at the AT&T archives documenting the place and time of all cutovers in the Bell system.[1] However, in the course of reviewing documents at the AT&T corporate archives (Warren, NJ), we discovered a three-page document compiled in the late 1930s which lists all cities in the U.S. and Canada with population >50,000, along with the date of that city's first cutover to dial and the percent of subscribers on dial as of December 31, 1937 (Figure B.1).[2] For cities which were less than 100% dial in 1937, we manually search Google and historical newspapers for reports of cutovers between 1937 and 1940, and update the percent dial to 1940 values based on these results.

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[1] According to a call with Sheldon Hochheiser, AT&T corporate historian, on March 1, 2017, the decision and pace of dial adoption was decided by management of the individual regional operating companies, not AT&T corporate.

[2] This document was found in AT&T Archives and History Center box 106-10-02-07.

Figure B.1: AT&T data on the adoption of dial in cities of population >50,000

Notes: Figure shows an extracted table from the source data on dial installation in large cities from the AT&T Archives and History Center (box 106-10-02-07).

We use distinct estimation strategies for studying the effects of dial in small versus large cities, which we cut at a 1920 population of 100,000, with an event study strategy for small cities (which typically had one-shot cutovers) in the paper and a long-differences strategy for larger cities (which were converted to dial in a more graduated, piecemeal fashion) in the appendix. Figures B.2 and B.3 below provide suggestive evidence that this was indeed the case: the figures show that smaller cities in the AT&T data (with population ≤100k) were nearly all 100% dial by 1940, irrespective of the date of their first cutover—suggesting these were one-shot events.

Figure B.2: AT&T city-level data: Fraction dial in 1940 vs. first cutover date
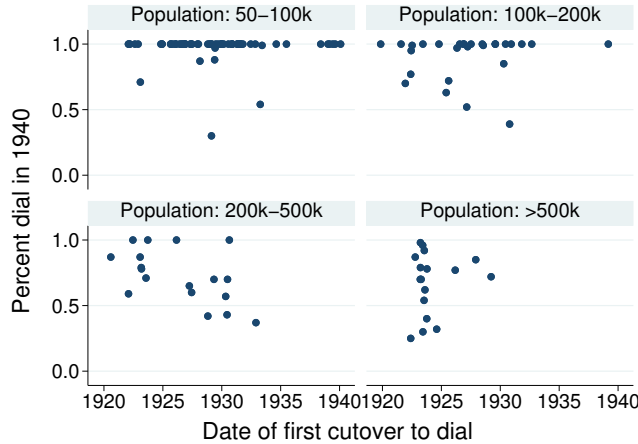
Notes: Figure plots a city's fraction dial in 1940 against the date of the city's first cutover to dial, for cities in the AT&T data, group by their 1920 population. The figure illustrates that smaller large cities (≤100k population) with cutovers were nearly all 100% dial, irrespective of the first cutover date, suggesting that they were single-cutover cities. In larger cities (200-500k), the fraction dial in 1940 varies with how recently cutovers began, and in the largest cities (>500k), which nearly all began cutting over to dial before 1925, they are unrelated.

Figure B.3: AT&T city-level data: Fraction dial in 1940 vs. population
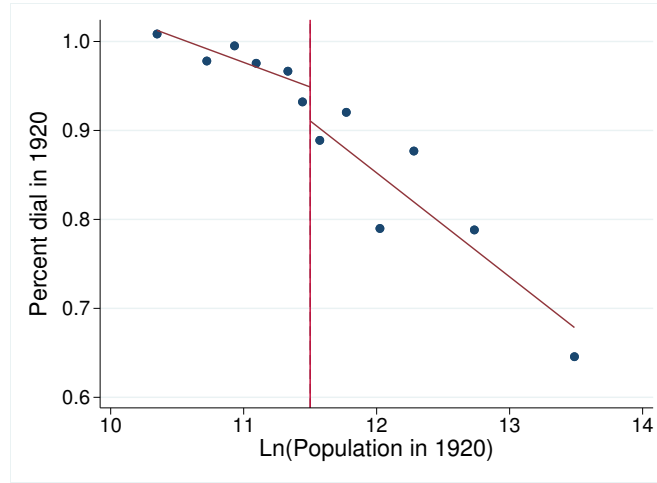
Notes: Figure shows a binned scatterplot of a city's fraction dial in 1940 against its log population, with a trend break at 100k population (the log of which is ≈11.5). The figure indicates that these smaller cities were typically around 100% dial by 1940, irrespective of their size, suggesting that they were single-cutover cities. In larger cities, the fraction dial in 1940 varies inversely with population.

Data from historical newspapers

We supplement the large-city AT&T data with a more comprehensive data collection effort from historical newspapers. Dial cutovers were locally-notable events and often reported on in the days before and after the change, and also sometimes months or even years in advance or later—not only because readers needed to know when to start using their dial telephone sets, but also out of public curiosity or celebration, as well as due to public concern over the fate of soon-to-be disemployed telephone operators, which was itself the focus of many articles.

We searched three online digitized newspaper collections for reports of cutovers and had assistants read through search hits to identify articles which reported cutovers, and for each record the cutover city, date, and number or percent of affected subscribers. Because these data are at the core of the paper, we will describe the data collection in substantial detail.

Round 1: July-August 2017

Data collection efforts began in the summer of 2017 and were initially focused on reviewing articles between 1917 and 1940 at Newspapers.com, which hosts the largest digitized, searchable historical newspaper collection available.[3] After testing several potential Boolean search terms, we settled on two preferred search terms, which we label "ST1" and "ST2" below:

(ST1) telephone ("dial" or "automatic") ("cutover" or "cut over" or
     "changeover" or "manual") ("office" or "exchange")

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[3]The search window was chosen on the grounds that (i) AT&T records indicate that the firm only began dial cutovers in the late 1910s, and (ii) we have outcome data through 1940.

(ST2) telephone ("dial" or "automatic") ("cutover" or "cut over" or "changeover" or "manual") "midnight"

Whereas ST2 is a more targeted search (due to the requirement of the word "midnight") and is designed to minimize false positives, ST1 casts a wider net and is designed to minimize false negatives. Between the two, we believe we can identify nearly all cutovers reported in the Newspapers.com collection. When these searches were conducted in July 2017, ST2 returned 4173 results, and ST1 returned 36072 results, of which 33060 were additional to those of ST2.

We had research assistants read all articles in the ST2 search results and the top 25% of the ST1 results[4] and asked them to determine whether the article does in fact describe a cutover, and if so, to record (i) the cities affected (sometimes several neighboring small towns are cut over at once, or served jointly by a single exchange); (ii) the date, including whether past or future (planned); (iii) the number or percent of subscribers affected, if reported (rarely); and (iv) any additional notes that may be relevant to measurement or interpretation (for example, occasionally an article reports on a cutover at a large firm or other organization that operates its own private, internal switchboard, rather than at the local telephone service provider). Whenever a research assistant flagged an article as describing a cutover or potentially describing a cutover, we manually reviewed their data entry to ensure the accuracy of the entered data.

We find newspaper reporting on both past and future cutovers characterized with varying degrees of specificity: many articles report exact dates, but some—especially articles that reference cutovers in passing, but are focused on other telephone company news—describe only the month and year (e.g., "last month"), season and year ("next fall"), year alone ("towards the end of this year"), or are non-specific (e.g., "nearing completion", proposed but not yet planned, or no timing reported; in the cases where an article describes a cutover without providing any information on its timing, we nevertheless infer whether that timing is past or future based on the verb tense in the article). In many cases, we find multiple reports of the same cutover, and we use these to cross-validate and refine our timing measures where possible. We take these data and aggregate up to the city and month: given that we study census-measured outcomes at decadal frequency, monthly variation in cutover timing should be sufficient for the purposes of this paper.

It is important to attempt to include cutovers even with imprecisely-reported timing: dropping these cutovers would bias our results towards zero, as the control group (of cities not cut over by 1940) would then have treated locations in it. Moreover, with outcomes at only decadal frequency, a bit of measurement error on the precise timing is acceptable in specifications that measure treatment as $\mathbb{1}$(Post-cutover) (but specifications measuring the time since a cutover would be more sensitive to this type of measurement error). When a cutover is reported with an "approximate" date, we

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[4]The search results are listed in order of "relevance", however determined by the website. Reassuringly, the rate of verified cutovers in these search results declines rapidly in the search rank: by the time we get a quarter of a way down the ST1 results list, only around 5 out of every 100 search results is a true description of a cutover, and these are often redundant to earlier reports, or lacking information on timing and unusable.

thus treat it as the true date. If the reported timing is otherwise non-specific, we use the following classification and crosswalk to approximate the month and year:

Category	Timing
ongoing	same month
recent	same month
soon	same month
in_few_weeks	next month
in_few_months	3 months ahead
winter	January of given year
winter_early	December of given year
winter_mid	January of given year
winter_late	February of given year
spring	April of given year
spring_early	March of given year
spring_mid	April of given year
spring_late	May of given year
summer	July of given year
summer_early	June of given year
summer_mid	July of given year
summer_late	August of given year
fall	October of given year
fall_early	September of given year
fall_mid	October of given year
fall_late	November of given year
year_early	March of given year
year_mid	July of given year
year_late	November of given year

When an article provides only the year and no more precise information can be inferred from other reports, we do the following: if the year is in the past or present (relative to the article), we assign the cutover to July of that year (the midpoint). Although this may introduce measurement error, this error will not be material to this paper unless the year is a Census year, and there are only two such cases in the data (one of which is Detroit, a large city, which we exclude from our event study on the grounds of its size anyway). If the year is in the future, the cutover itself is uncertain, let alone the timing, and we treat it as planned but undated.

Figure B.4: Example newspaper headlines reporting dial cutovers and job losses

Round 2: July-August 2019

In the summer of 2019, we undertook a second round of newspaper-based data collection to capture new results from Newspapers.com, whose collection of digitized newspapers had more than doubled, and to expand our data collection effort to the two next-largest digital newspaper repositories (NewspaperArchive.com and GenealogyBank.com), which may cover different cities or time periods. In July 2019, we repeated our ST1 and ST2 searches for the 1917-1940 period on Newspapers.com, and also performed searches on these two additional sites.[5]

In Figure B.5, we map cities with any newspaper issues on Newspapers.com or GenealogyBank.com in this period (1917-1940) as of June 2019 (Panel A), as well as cities with continuous newspaper coverage throughout the period (Panel B). Although some states are over-represented in these data, we are reassured by the fact that every state is covered by multiple newspapers. Moreover, cross-sectional differences will be washed out by city fixed effects in our analysis, which focuses on changes in young women's outcomes over time. Note that cities with newspaper-reported cutovers are not limited to these, as newspapers often reported cutovers in other cities in the same state. But cities with newspapers, especially those with continuous coverage, are most reliably reported—and for this reason, our analysis includes robustness checks on only these latter cities, to ensure that our results are not driven by selection into the newspaper.

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[5]Note that NewspaperArchive.com does not support Boolean search. In this case, we searched each non-Boolean permutation of each search term. For this data source we skipped the following permutations of ST1: "telephone dial manual office" / "telephone dial manual exchange" / "telephone automatic manual office" / "telephone automatic manual exchange", due to the size of the results list and the high rate of false positives. Having omitted these results, we review all other ST1 results from NewspaperArchive.com (rather than just the top 25%). We believe most true positives in these search results will be picked up this way.

Figure B.5: Cities covered by Newspapers.com and GenealogyBank.com

Panel (A): Cities with at least one year of newspaper coverage, 1917-1940
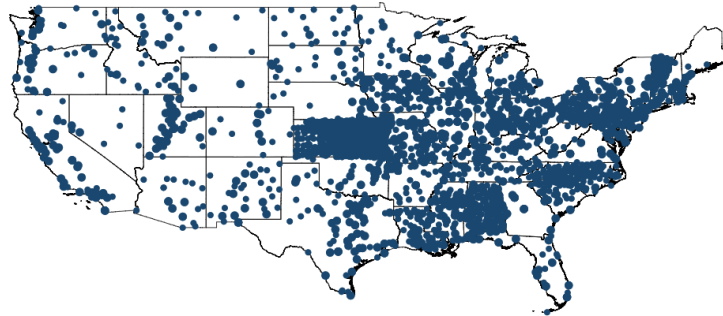
Panel (B): Cities with continuous newspaper coverage, 1917-1940
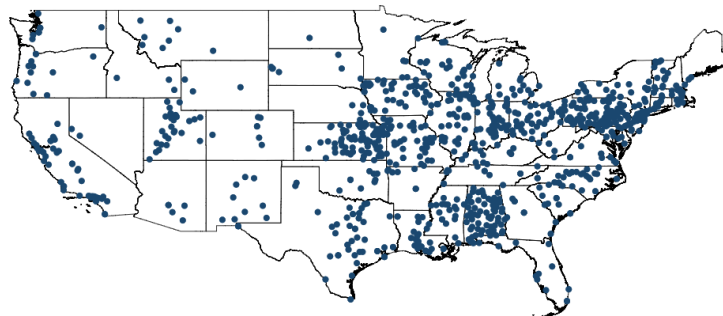
Notes: Figure maps the cities with digitized newspapers on Newspapers.com and GenealogyBank.com at the time of data collection. We often see a given newspaper reporting cutovers in other cities in the same state or county.

When these searches were conducted on Newspapers.com in June 2019, ST2 returned 6666 results, of which 2490 were new since 2017, and 2280 of these unique newspaper issues (in the second round of data collection, we noticed that sometimes the search returns multiple hits from the same newspaper on the same day, and we had assistants read each newspaper issue only once, to reduce duplicated efforts). ST1 returned 55312, of which 39889 were also not already collected in 2017 or covered by ST2, 36502 of these from unique issues, and 3512 in the top 25% of ST1 search results. These results (2280 for ST2, 3512 for ST1) were then manually reviewed by research assistants. Similarly: on GenealogyBank.com, ST2 returned 2609 results, of which 2497 were new since 2017, and 2309 of these unique issues; ST1 returned 21171, of which 18143 were also not already collected in 2017 or covered by ST2, 16304 of these unique issues, and 4021 in the top 25% of ST1 search results. On NewspaperArchive.com, ST2 returned 2100 results, of which 1512 were new since 2017, and 1189 of these unique issues; ST1 returned 1520 (see previous footnote as to why this number is lower than that for ST2), of which 828 were also not already collected in 2017 or covered by ST2, 513 of these unique issues. The table below summarizes this information:

			ST2	ST1 (not in ST2)	Total
Round 1 (2017)	Newspapers.com	All results	4,173	33,060	37,233
		Reviewed	4,173	8,265	12,438
Round 2 (2019)	Newspapers.com	New results	2,490	36,502	38,782
		Reviewed	2,280	3,512	5,792
	NewspaperArchive.com	All results	2,100	1,520	3,620
		Reviewed	1,189	513	1,702
	GenealogyBank.com	All results	2,609	21,171	23,780
		Reviewed	2,309	4,021	6,330
	Total	All results	11,372	92,253	103,415
		Reviewed	9,951	16,311	26,262

Results

In total, we find 3,945 reports of cutovers in the continental U.S., with 3,859 describing non-private branch exchange (PBX) cutovers in 887 distinct cities and towns. With respect to the precision of the timing information, these reports break down as follows:

|		Articles	
Category	Label	Count	Percent
1	Exact date provided	2,171	56.2
2	Date inferred from coarse information + other reports	1,150	29.8
3	Month and year provided or approximated	308	8.0
4	Year provided, past or present	25	0.7
5	Year provided, future	9	0.2
6	No timing information provided	196	5.1
Total		3,859	100

Of the 887 cities with cutovers, 798 have at least one cutover in the newspapers data with exact or approximate timing (categories 1-4 above), whereas 89 *only* have cutovers without reliable timing information. To be conservative, we drop these cities from the analysis in Sections 4 to 6 of the paper, because we cannot know for certain when the shock occurred—or, for reports of future cutovers, if it even occurred at all. For the remaining cities: although a handful (43) have ≥ 1 reports of a cutover that we are unable to date, (i) most of these are large cities excluded from the event study sample, and (ii) we find that the majority (70%) have their earliest known cutover in the 1920s, and the vast majority (98%) by 1933, providing confidence that we can accurately measure cities' earliest cutovers, which is the relevant margin for this paper.

We aggregate these data up to the city x month level, identifying months in which each city was reported to have experienced a dial cutover, and henceforth we call each such city-month a "cutover" (we assume that when there are multiple reported cutovers in a given city in the same month, these are part of the same event—although there are few such cases in the data, as we have previously harmonized cutover dates in the raw data).

There are 1,047 cutovers with known timing across the 798 cities in our final sample (an average of 1.3 per city, with a median of 1, 90th percentile of 2, and max of 15), and 904 that take place

between the 1910 and 1940 Censuses (April 1910 and April 1940), the period studied in this paper. Among these, the average and median cutover took place in 1931.[6] Figure B.6 below maps these cutovers, illustrating their expanding geographic incidence—which is the variation at the heart of this paper. Figure B.7 shows a binned scatterplot of a city's number of cutovers in the Newspapers data against 1920 log population, with a line at 100k population (our threshold for the event study sample). This figure reinforces the evidence that smaller cities typically have only one or at most two cutovers in our data, consistent with these locations being served by only one or a few telephone exchanges, which could be simultaneously converted to dial.

Figure B.6: Newspapers city-level data: Expanding geography of dial, 1915 to 1940
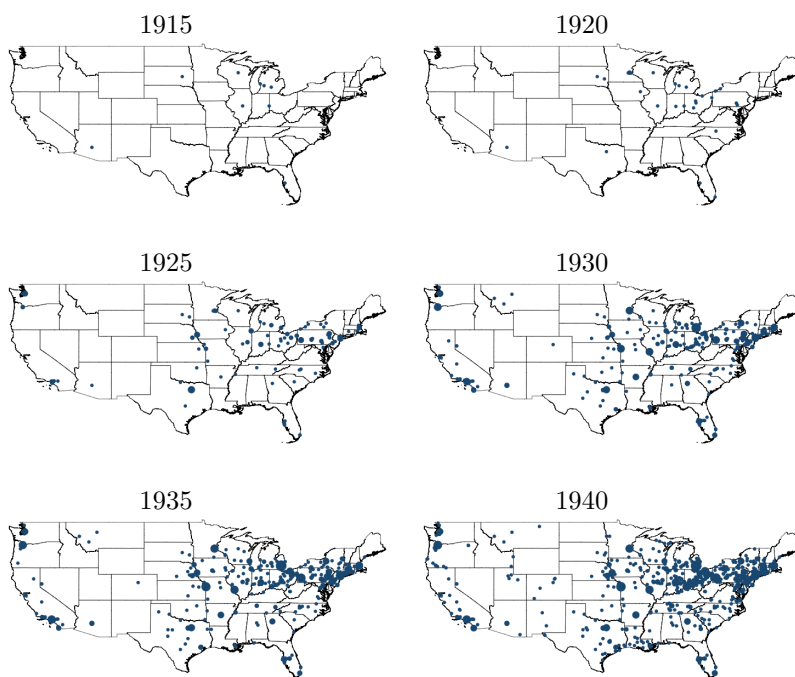
Notes: Figure maps the cities with a dial cutover in the newspaper data through the year shown in each panel. Bubble sizes are proportional to the cumulative # of reported cutovers in the given city through the given year.

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[6]Note that of the 1,047 cutovers with known timing from the Newspapers data, 26 cutovers (2.5%) took place before the first cutover in the AT&T data (in November 1919), ostensibly having been executed by independent (non-AT&T) telephone service providers—which we confirm by manual review. Additional comparisons between AT&T and Newspapers data are provided in the next subsection below.

Figure B.7: Newspapers city-level data: Number of reported cutovers vs. 1920 population
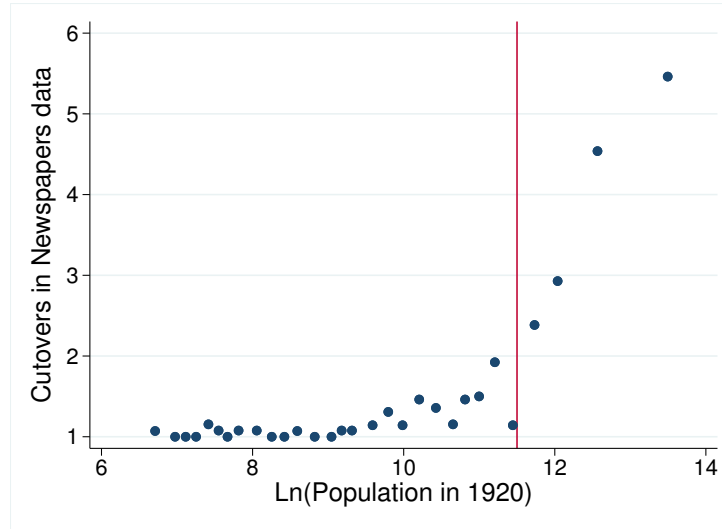
Notes: Figure shows a binned scatterplot of cities' number of reported cutovers, mea-
sured as the number of distinct months between 1919 and 1940 with a cutover reported
in our Newspaper data, against log 1920 population, with a line drawn at 100k popula-
tion (the log of which is ≈11.5). The figure illustrates that smaller cities typically have
only one or at most two cutovers in our data, suggesting that they were single-cutover
cities. Larger cities have several cutovers in our data.

Comparison of AT&T and Newspapers data

We can also cross-validate the AT&T and Newspapers data against each other, by comparing the
timing of the earliest cutover reported in newspapers versus in the AT&T administrative data
for all cities appearing in both sources. Figure B.8 shows this comparison, plotting individual
cities' earliest newspaper-reported cutover (vertical axis) against earliest AT&T-reported cutover
(horizontal axis). Each point represents a city and is labeled with its state's abbreviation, and the
dashed red line is the 45-degree line. Dates coincide across the two sources for the vast majority
of cities, providing reassurance on the quality of the newspaper data. For the handful of cities
where newspapers report a cutover preceding those in the AT&T data by more than a month
(below the 45-degree line), we revisited the reporting articles and determined that either (i) these
were performed by independent (non-AT&T) companies (13 cases), or (ii) these were preliminary
cutovers affecting a very small portion of the population (1 case).

Figure B.8: Timing of cities' first cutover in AT&T data vs. newspaper data
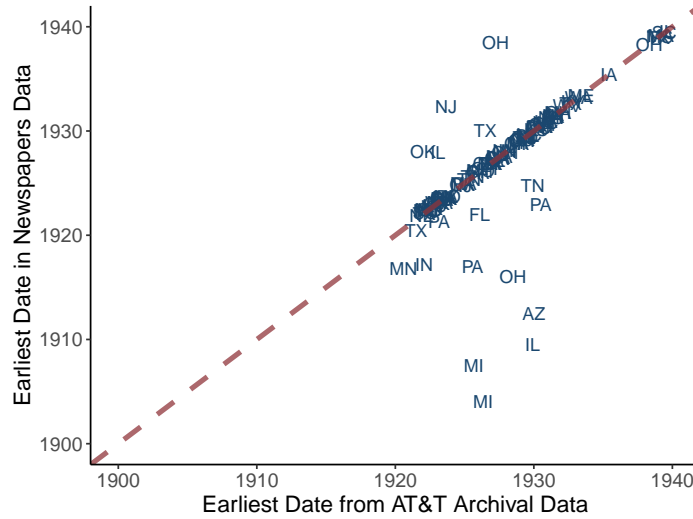
Notes: Figure plots cities' earliest observed cutover date in newspapers data versus AT&T data, for cities in both data sources, and the 45-degree line in red. Each city is labeled with its state abbreviation. Figure is presented to illustrate the degree of agreement between the AT&T and newspapers data. Nearly all cutovers identified in the newspaper data collection that preceded AT&T-reported cutovers were performed by independent (non-AT&T) telephone companies.

B.2 Complete-count Census data

Taken decennially, the US Federal Census enumerates the entire population and contains a wealth of economic, social, and demographic information. We draw on the recently digitized complete count census data from IPUMS (Ruggles et al. 2019) for the censuses in 1900, 1910, 1920, 1930, and 1940.[7] That the data is complete count means simply that all individuals enumerated—the complete count of people in the US in each census year—has been transcribed and coded by IPUMS. This enables us to count not just the number of telephone operators in the telephone industry in each city, but the number who are 17, white, born in Massachusetts, and single, if we wanted that level of granularity. In this appendix subsection, we describe our aggregation procedures— particularly which individuals we include in which samples—as well as other controls variables we build with the complete count data.

Aggregation of complete count individual-level data

Unit of observation

We restrict attention to the adult (16+) non-farm population with non-farm occupations, in the continental U.S. only (lower 48 states plus District of Columbia). Our primary dataset aggregates these individuals in the complete count data up to the level of:

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[7]We stop in 1940 because the census is privacy-restricted for 72 years after it is taken and so 1940 is the most recent census IPUMS has and could transcribe and digitize in full.

15

city (continental U.S. only)
 x American-born (dummy)
 x race and ethnicity (bins)
 x sex (dummy)
 x age (bins)
 x urban (dummy)
 x occupation (1950 encoding)
 x industry (1950 encoding)
 x year (decade)

where these variables are defined as follows:

- American-born: indicates whether an individual was born in a U.S. state or territory
- Race/ethnicity: bins for (i) white/non-Hispanic, (ii) white/Hispanic, (iii) black, (iv) Native American, (v) Asian, (vi) mixed, and (vii) other
- Sex: indicates whether individual is male or female
- Age: 5-year bins for individuals age 16-20 to 56-60, and 61+
- Urban: indicates whether individual's household was urban (vs. rural)
- Occupation: 1950 occupation codes (283 categories)
- Industry: 1950 industry codes (162 categories)

We also prepare derivative datasets that (i) further aggregate up to dummies for telephone operators and the telephone industry (rather than separate bins for each occupation and industry), and (ii) aggregate up all occupations and industries. To study effects of dial on the youngest ages, we also prepare variants of these datasets where age is measured in individual years for ages 16-25, plus a bin for age 46+ (for comparison/control), omitting ages in between.[8]

In addition to these city x demographic bin x year datasets, we prepared separate datasets of (i) all individuals reporting as telephone operators (occ1950=370), and (ii) all individuals reporting as working in the telephone industry (ind1950=578).

Sampled cities

The raw complete count data include each individual's household's state and county, and city where relevant. The IPUMS data includes not only a raw city string (as originally reported on Census manuscripts) but also a standardized city, to account for the fact that city spellings may change or be reported slightly differently for different households or in different years. However, this standardized city was not always provided, or was sometimes provided where the raw city was missing, and we determined that additional harmonization was needed.

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[8]Although not referenced or used in this paper, we also prepare county-level variants of these datasets. Our research interest is in local labor markets, which we believe would be more accurately characterized by cities than counties in a period when commuting over more than short distances was relatively costly. However, results throughout the paper are similar when run with county-level rather than city-level data.

We begin by combining the list of raw city strings and IPUMS-standardized cities from all years 1910-1940 (note that these can vary: some smaller cities are not found in every year of the IPUMS data). Having done so, we then manually examine (i) cities in the same state that start with the same three letters, (ii) cities in the same county that sort adjacently and have a Levenshtein edit distance of ≤ 4, and/or (iii) cities in the same county that sort within 30 positions of each other and have an edit distance ≤ 2, to find spelling variants that appear to be the same city. We use the results of this effort to build a crosswalk from the raw and IPUMS-standardized city names to our manually, fully-harmonized city names. We apply this crosswalk to both the raw city strings and IPUMS-standardized city names, which will also now match when both are provided. We take either of these measures, when available, as an individual's (household's) true city.

From this effort, we produce a list of unique, harmonized cities by year. We then identify all such cities which (i) are observed every year from 1910 to 1940, and (ii) have a population of $\geq 2,000$ in 1920, as measured by aggregating up individuals in the IPUMS data. This yields a balanced panel of 3,027 cities, which comprise the sample for this paper. Within this sample, the median 1920 city population is 4,346; the 95th percentile is 48,414. Of these 3,027 cities, 415 are identified in our cutover data, and 384 with exact or approximate cutover timing.

This characterizes our base city sample, but we later exclude several of these cities from our empirical analysis. Specifically, we remove 14 cities with a population ≤ 500 in 1910, to eliminate those where inference is made difficult by small samples—though this is unlikely to affect our analysis, which is weighted on population. We also winsorize 56 cities where the IPUMS data report large, sharp drop-offs in the fraction of the population (notably, prime-age males) with an occupation. Figure B.9 illustrates some examples, several of which are neighboring cities where these outlier values occur in the same year, suggesting it is due to enumeration anomalies. We identify outlier cities as those where employment growth in any year is below the 1st percentile or above the 99th percentile, or where the ratio of the average employment rate in $t-10$ and $t+10$ to that in year t is above the 99th percentile. To be conservative and err towards precise measurement, we additionally drop 31 cities where newspapers report a cutover but without indicating when it took place, as well as New York City boroughs, since it is often difficult to discern from newspapers articles in which borough a cutover took place, and because there were many more cutovers in the very large New York City system than in other cities. In total, this results in 105 cities being excluded from the original city sample in preparation for analysis.

Figure B.9: Examples of cities with outlier changes in the population with an occupation
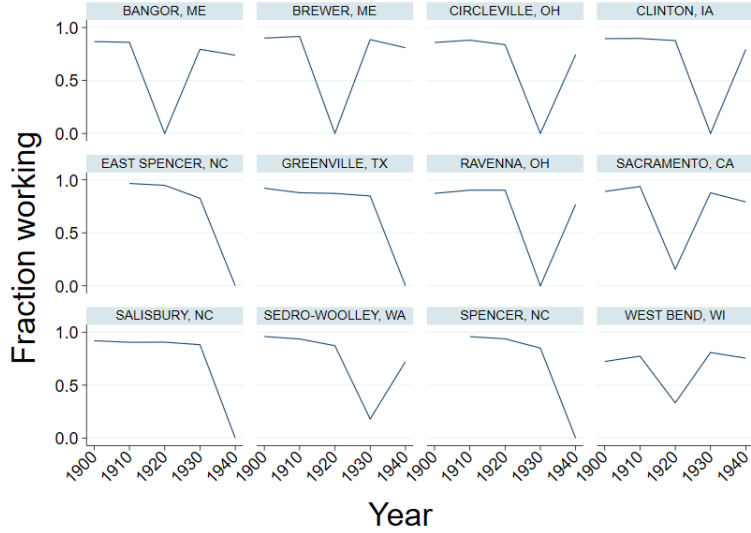
Variables measured

For each city x demographic bin x year, we measure the following variables:

Category	Variable
Population	Population in city-bin-year
Family and Residence	Number married
	Number ever married
	Mean/median age at first marriage
	Number with children
	Mean/median number of children
	Number living with parents
	Number living with other family
	Number who own their dwelling
	Number who rent their dwelling
Migration status (1940 only)	Number who moved within the past 5 years
	Number who moved within state
	Number who moved between states
	Number living in the same incorporated place
	Number living in the same SEA
Income measures (1940 only)	Mean/median income
	Mean/median occupation score
Education status	Number currently in school

For datasets that aggregate up occupations and industries, we also measure the following group-level employment characteristics:

Category	Variable
Employment status	Number working (with an occupation)
	Number who are telephone operators
	Number working in telephone industry

18

In preparing these data, we create a new code that identifies every individual who reported either (i) not being in the labor force, or (ii) having a non-working occupation (occupation codes in the 980-999 range, which includes housewives, students, retirees, disabled persons, and inmates, as well as missing/unknown). We define the working population as all others, i.e. all persons who both (i) report as being in the labor force, and (ii) have a working occupation.

Other remarks

We also collect 1920 to 1940 city populations from Census publications for validation and independent use. From the 1930 Census, we retrieve 1920 and 1930 populations for all cities with >1,000 people in 1930, and from the 1940 Census, 1930 and 1940 population for all cities with >1,000 people in 1940. We then merge these data to build the panel (retaining 1930 values from the 1940 Census, rather than the 1930 Census, where conflicts arise).

B.3   Creating our Linked Sample of Female Telephone Operators

In addition to studying future cohorts, we study the effects of automating telephone operation on the telephone operators themselves. To do this, we have to follow operators over time, tracing their careers and lives from when they were employed as operators in the telephone industry to after the cutover shocks. If the operators had been men, this task would be relatively straightforward and we could rely on one of the commonly used census linking strategies in the economic history literature.[9] However, as we showed in Table 2, the vast majority of operators in 1920 and 1930 were young women. While the demographic profile of operators makes them an interesting set of workers to study, it also makes them impossible to link across censuses in traditional ways. Census linking is commonly performed on "stable" features enumerated in the census, most importantly names. However, young female operators may marry and then change their names upon marriage between the census we see them as an operator and the next census where we would like to find them. Because census records do not attach unmarried names to married records, such name changes would make linking impossible without additional data or information.

To create our individual-level longitudinal data on telephone operators, we turn to an alternative source of linked data, the FamilySearch public family history tree (Price et al. 2019). In this appendix, we describe in detail our precise procedure to link telephone operators via the FamilySearch tree and how we weight our final sample to ensure our results are internally valid to all telephone operators in this period and not just those more likely to be on the tree. In the penultimate subsection of the appendix, highlighting the novelty of using the FamilySearch tree to created linked data for empirical analysis in this paper and future research, we ask and answer the question of which individual features make someone more or less likely to be on the tree.

When we analyze the effects of cutovers on telephone operators, we compare the operators to control women. To serve as a control woman for a given operator, the control must match the operator

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[9]For an overview of automated census linking, see Abramitzky et al. (2020).

in the initial census (1920 or 1930, the census in which the operators were employed as telephone operators in the telephone industry) on sex, race, Hispanic ethnicity, marital status, whether or not she has any children, whether or not she is US-born, whether or not her mother is US-born, whether or not her father is US-born, and is within 5 years younger or older of the operator. In addition, we match controls on precise geography, zooming in on the enumeration district. In the 1920 and 1930 census, enumeration districts are census-constructed geographies that contain about 1000 people all living in close proximity, akin to a neighborhood in most cities. The final key matching variable is employment status: we only compare telephone operators to other women who were also employed in the initial census. As we note in the paper, we identified 21 controls for each operator. We then followed the same genealogical linking strategy for the control women as for the operators, locating them on FamilySearch and, if possible, tracing them forward to the following census. Ultimately, we linked nearly 5 controls per operator. However, to reduce data collection time, we attempted to link control women forward only after we had linked the operators and we *only* attempted to link the control women for operators we linked ahead to the following census. This reduced the number of control women we had to attempt to link dramatically, from more than 4.2 million to about 460 thousand, and with no empirical downside because the control observations for unmatched operators would not be included in our analysis. Because this procedure determined entry into the sample, we focus our description of our linking and our analysis of who is on the tree on the operators. In the final subsection of the appendix, however, we describe the control sample in more detail.

B.3.1   The Procedure to Link Telephone Operators

To link the women in our sample, we develop and implement a novel linking procedure, making use of a popular genealogy platform and the "work" of many expert family historians (both professional genealogists and hobbyists) linking the women in their family trees across censuses, marriage events, migration events, and more. In this subsection, we describe our linking pipeline and the matched data, echoing information from the main paper with additional details. In Figure B.10 we illustrate our procedure as it compares to traditional linking methods, and in Figure B.11 we document the specifics of our approach at each of the steps described below.

We begin by reproducing our description of the linking procedure from Section 2. We link in the following four steps. First, we identify all women working as telephone operators in the telephone industry in the 1920 and 1930 complete count census data (Ruggles 2002). After limiting to women in our focal cities, we have 96,183 women in 1920 and 61,110 women in 1930.[10] Second, we look for each telephone operator on FamilySearch, a public genealogy platform with an open wiki-style

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[10]Note that this sample omits a small number of male operators from our analysis as well as a small number of operators younger than 16 or older than 60. Only operators in cities with cutovers after 1920 are included. We further limit to operators in cities with population ≤100,000 in 1920, where cutovers were typically one-shot events, matching the sample we use when we study the next generation of potential operators (see Section 4). For the 1930 sample, we further restrict the sample by filtering out cities with cutovers before 1930, as these women are selected on being operators *after* their city was cut over to dial service.

family tree (Price et al. 2019), where users create pages for deceased individuals—usually their own ancestors but not always—and attach links to historical records, including entries from Federal Censuses, marriage records, and birth certificates. We search FamilySearch in our base years by name, age, sex, location, and state of birth. Because the 1920 and 1930 census transcriptions in the IPUMS complete count data are based on the same original manuscripts as those on FamilySearch and we can use names, addresses, and other characteristics to link them, matching is straightforward. However, not all telephone operators have a page on FamilySearch. We are able to find 34.6% of operators in 1920 and 37.0% in 1930 with a FamilySearch page.[11]

Third, we query the FamilySearch tree for links to the next census. That is, we begin with the set of operators who were attached to the tree in year $t \in \{1920, 1930\}$, the census in which they were an operator. We check whether or not each operator's profile on FamilySearch has been linked to a record from the census in $t+10$. Conditional on being on the tree, 49.3% of records in our sample from 1920 are linked ahead to the 1930 census and 50.1% of 1930 records are linked to 1940.

Finally, for the set of operators with FamilySearch records attached to censuses in t and $t+10$, we use census record metadata—reel, page, and line number—to make links back to the complete count, restricted use IPUMS data. This process yields a sample of 16,253 operators linked from 1920 to 1930 and another 11,220 linked from 1930 to 1940. For all of these operators, we observe the full set of census covariates in t and $t+10$, allowing us to study what happens to operators a decade later, including their occupation, industry, marital status, and fertility.

An example can clarify why linking women is difficult, and why the FamilySearch data can help. Suppose we start with a telephone operator in 1920 in New York named Daisy Fay. We see in the 1920 census that Daisy was born in 1902 in Kentucky. With traditional census linking methods like Abramitzky et al. (2020) or Ferrie (1996), we would search for records in the 1930 Census with the name Daisy Fay, born in 1902 in Kentucky, likely with some tolerance for transcription errors or enumeration errors in these fields. However, if Daisy marries Tom Buchanan in 1922, we would have no way of knowing that Daisy Fay is likely known as Daisy Buchanan in 1930. Worse, if another woman named Daisy born in Kentucky around 1902 marries and takes a surname of Fay, we could falsely match two women who are not the same person. With our FamilySearch-based approach, we instead search for Daisy Fay on FamilySearch in 1920. If her 1920 record is attached to a page, we consider her on the tree. We then look to see if a FamilySearch user has also attached her to the census in 1930, possibly triangulating with knowledge of her name after marriage or her marriage date, either from personal knowledge or an attached marriage or birth certificate (or in Daisy's case, a prominent work of American literature). If she is linked to both the 1920 and 1930

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[11]Whether or not an operator—or anyone else—is attached to the FamilySearch tree is inevitably nonrandom. Pages are built, and records attached, by people working on family history today, and the FamilySearch platform is affiliated with the Church of Jesus Christ of Latter-day Saints. As long as the bias in who is likely or not likely to be on the tree is uncorrelated with the timing of cutovers, our event study strategy—comparing operators across cities and before and after cutovers—should produce an unbiased estimate of the cutover treatment effect. Later in this appendix, we describe in more detail what predicts whether or not an operator is on the tree and shows that match rates are not a function of our treatment.

censuses, she will make our sample.

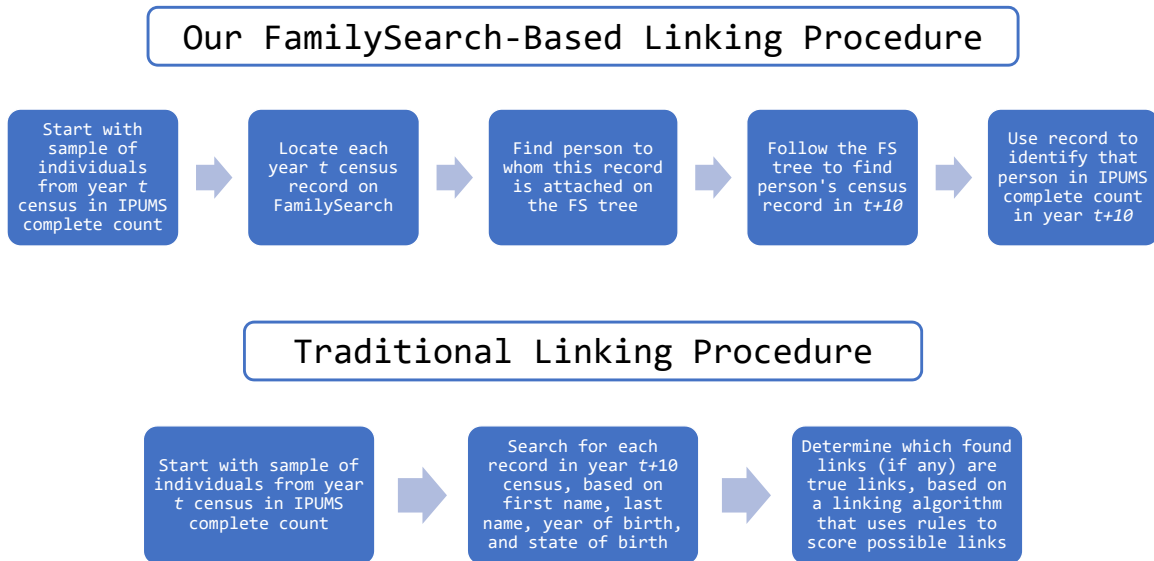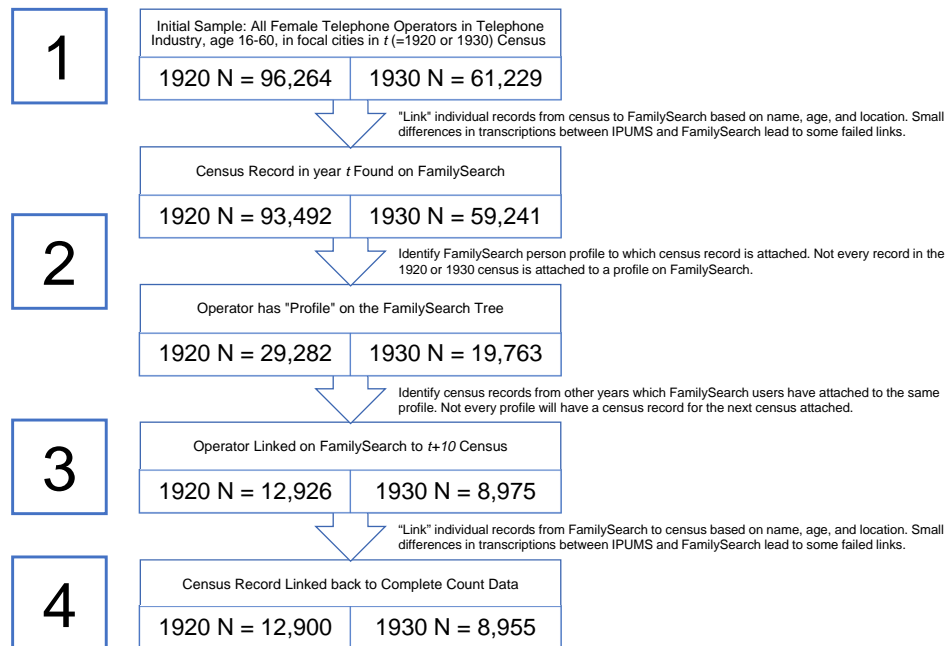Figure B.10: FamilySearch-based versus Traditional Linking Procedures

Our FamilySearch-Based Linking Procedure

Start with sample of individuals from year *t* census in IPUMS complete count → Locate each year *t* census record on FamilySearch → Find person to whom this record is attached on the FS tree → Follow the FS tree to find person's census record in *t+10* → Use record to identify that person in IPUMS complete count in year *t+10*

Traditional Linking Procedure

Start with sample of individuals from year *t* census in IPUMS complete count → Search for each record in year *t+10* census, based on first name, last name, year of birth, and state of birth → Determine which found links (if any) are true links, based on a linking algorithm that uses rules to score possible links

Figure B.11: Sampling at Each Stage of the FamilySearch-based Procedure

1

Initial Sample: All Female Telephone Operators in Telephone Industry, age 16-60, in focal cities in *t* (=1920 or 1930) Census	
1920 N = 96,264	1930 N = 61,229

"Link" individual records from census to FamilySearch based on name, age, and location. Small differences in transcriptions between IPUMS and FamilySearch lead to some failed links.

Census Record in year *t* Found on FamilySearch	
1920 N = 93,492	1930 N = 59,241

2

Identify FamilySearch person profile to which census record is attached. Not every record in the 1920 or 1930 census is attached to a profile on FamilySearch.

Operator has "Profile" on the FamilySearch Tree	
1920 N = 29,282	1930 N = 19,763

Identify census records from other years which FamilySearch users have attached to the same profile. Not every profile will have a census record for the next census attached.

3

Operator Linked on FamilySearch to *t+10* Census	
1920 N = 12,926	1930 N = 8,975

"Link" individual records from FamilySearch to census based on name, age, and location. Small differences in transcriptions between IPUMS and FamilySearch lead to some failed links.

4

Census Record Linked back to Complete Count Data	
1920 N = 12,900	1930 N = 8,955

B.3.2 Inverse Propensity Weights

The set of operators that we can link using the FamilySearch data from 1920 to 1930 (or 1930 to 1940) is inevitably not random. To account for any potential unrepresentativeness of our final sample, we follow Bailey et al. (2017) and use inverse propensity weights (IPW) to adjust for observable differences between telephone operators in our initial sample and those we are able to match to the following census via the FamilySearch tree.[12]

Bailey et al. (2017) construct these weights in two steps, per footnote 33 of that paper:

- <u>Step 1</u>: Run a probit regression of link status (whether an individual is matched) on the following variables: an indicator for those with a middle name; the length of first, middle, and last names; polynomials in the day of birth and age; an index for how common the first and last names are; whether or not one has siblings and the number of siblings; and the length of the names of one's parents.

- <u>Step 2</u>: Inverse propensity scores are then computed as $\frac{1-p}{p}\frac{m}{1-m}$, where p is the predicted likelihood of being matched based on the estimated probit coefficients and m is the actual match rate.

Starting with the census attributes we observe about the telephone operators in our initial sample, we follow a similar procedure to predict which women are linked and which are not. As we emphasized in the previous section, there are three reasons an operator might not reach our final sample.[13] The IPWs account for any differential propensity to stay in or leave the sample being driven at any stage in the linking process.

We adapt the reweighting procedure to match our setting as follows:

1. We control for age and age-squared. However, we cannot include polynomials for the day of birth, as day of birth is not recorded in either the 1920 or 1930 censuses.

2. We include two indicators for middle names/initials, one for the presence of a middle name and one for the presence of a middle initial.[14]

3. We control for first and last name length, separately. We do not control for parents' name length as we rarely observe our operators living with their parents, unlike the Bailey et al. (2017) example which linked children to their adult-selves.

4. We control for both the commonness of first and last name, measured as the log of the number of people in the 1900, 1910, and 1920 censuses with that first or last name.

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[12]Concerns about whether or not the final analysis sample is representative of the underlying population is not unique to our setting, nor to genealogy-based linking (versus hand or automated linking). All historical samples built with census linking could potentially be subject to concerns about unrepresentativeness, as linking is a function of names, ages, and other individual characteristics—features which might correlate both with being link-able and with other empirically-relevant observed or unobserved individual attributes.

[13]The reasons why an operator may not reach our final sample are: 1. The operator does not have a page on the FamilySearch tree to begin with; 2. The operator's record in the following census is not attached to her page on the tree; 3. The metadata in the tree or in the IPUMS complete count census indicates that the same person is not referenced in the same reel x page x line of each source (errors), although this last issue is very rare.

[14]Approximately a dozen operators in each linked sample are edge cases with enumerated names like "Ruby Anna M" or "Anna Bell F". In these cases, we declare them to have both middle names and middle initials and include the interaction of the two indicators in our probit model.

5. We control for whether or not the operator is married or single, whether or not the operator has children, and household size, all measured in the initial census. These are similar, in spirit, to the Bailey et al. (2017) controls for siblings.

6. We include a full set of indicators for the operator's role within the household (head of household, spouse, daughter, boarder, etc).

7. We include a full set of indicators for birthplace (state of birth for the American-born, and country of birth for the few foreign-born).

8. We include a full set of indicators for current state and size of place, where size of place refers to the city or town of residence in the census year.

9. We control for race and Hispanic status.

In Table B.1, we present the coefficients from the probit regression for our linked samples, with the weight-generating function for the link for the full sample in the first column, from 1920 to 1930 in the second column, and from 1930 to 1940 in the third column. We omit the many fixed effects—for state of birth, state of residence, household role, and size of place—and report coefficients directly from the probit model. We see that operators with more common first names are less likely to be linked, so we will use the IPWs to up-weight the women with common names whom we do link so that our final analysis sample is comparable to our initial sample. On the other hand, we see that operators with children or in larger households are *more* likely to be linked, perhaps because they have larger families with more descendants researching genealogy today. We see matching is less likely for non-white and Hispanic women, a common result in the linking literature that may be exacerbated by the demographics of the FamilySearch userbase.

Table B.1: Probit Model of Operator Linking to Build Inverse Propensity Weights

	Telephone Operators		
	Linked	Linked 1920-1930	Linked 1930-1940
	(1)	(2)	(3)
Age	-0.003	-0.017***	0.022***
	(0.007)	(0.006)	(0.005)
Age-Squared/100	-0.000	0.018*	-0.037***
	(0.012)	(0.010)	(0.009)
Middle Name	0.114*	0.131**	0.107
	(0.061)	(0.056)	(0.072)
Middle Initial	0.176***	0.197***	0.147***
	(0.010)	(0.016)	(0.017)
Middle Name and Initial	-0.536**	-1.317**	-0.226
	(0.221)	(0.576)	(0.309)
First Name Length	-0.004***	-0.004	-0.003
	(0.001)	(0.004)	(0.004)
Last Name Length	-0.003**	-0.005	-0.000
	(0.001)	(0.004)	(0.003)
First Name Commonness	-0.013***	-0.013***	-0.013***
	(0.002)	(0.002)	(0.003)
Last Name Commonness	-0.004**	-0.004***	-0.004**
	(0.002)	(0.001)	(0.002)
Single	0.032	0.038	0.023
	(0.033)	(0.028)	(0.024)
Has Children	0.171**	0.171***	0.167***
	(0.069)	(0.028)	(0.025)
Family Size	0.056***	0.056***	0.055***
	(0.002)	(0.003)	(0.004)
Nonwhite	-0.237***	-0.356*	-0.196
	(0.057)	(0.200)	(0.219)
Hispanic	-0.174***	-0.170*	-0.189
	(0.066)	(0.100)	(0.131)
Relate FE	Yes	Yes	Yes
Birthplace FE	Yes	Yes	Yes
Size of Place FE	Yes	Yes	Yes
State of Residence FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	157,293	96,183	61,110

B.3.3 Who is on the FamilySearch Tree?

While the weighting scheme described in the previous section addresses the potential unrepresentativeness of our sample built with genealogical links, because this paper is among the first to use

the FamilySearch tree links for empirical analysis[15] we also want to explore in more detail the representativeness of the tree. We do so in this section, tracing out which operators in our initial sample are more or less likely to be on the tree. We find that being on the tree is far from random on many demographic features—especially state of residence, place of birth, and family size—but unrelated to the treatment in this paper, telephone exchange cutovers.

Of the 157,293 telephone operators we went looking for on the FamilySearch tree, we found 55,928 (35.6%) on the tree. As Table B.2 shows, the share-on-the-tree is very similar for operators in 1920 and in 1930. Why are some operators on the tree and others are not? One simple reason is that the operators on the tree were put there by someone, likely a descendant. However, these descendants need not be direct descendants; much of the genealogical work on the tree is done by relatively distant relatives expanding their trees in many directions. An operator could be a grandmother or great grandmother of a FamilySearch user today, but she could also be a distant great aunt or the spouse of a third cousin. We lack specific (private, protected) data from FamilySearch on who is adding each person to the tree and how they are related. But we can investigate which demographic features or other census-enumerated covariates predict which operators or are not on the FamilySearch tree, which is more germane to our analysis.

Table B.2: Share of Operators on the Family Search Tree, 1920-1930

Year	Telephone Operators	On the Tree	Share (%)	Linked to Next Census	Share (%)
Pooled	157293	55928	35.6	27473	17.5
1920	96183	33304	34.6	16253	16.9
1930	61110	22624	37.0	11220	18.4

The FamilySearch tree has much more coverage of people who were likely ancestors of today's FamilySearch users. One particularly large user group is members of the Church of Jesus Christ of Latter-day Saints (LDS), as FamilySearch is operated in partnership with the LDS church. We can see this in the maps in Figure B.12 and Figure B.13, where Utah and other Mountain West states stand out in their rates of tree coverage. Tree rates are lowest in Mid-Atlantic states like New York and New Jersey, which have relatively fewer LDS members.

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[15]We are not the first to propose using the FamilySearch data. Price et al. (2019) introduce the FamilySearch tree data to the economics literature, but their focus is on building a new method to link census data using the FamilySearch tree as training data rather than using the links themselves. In addition, Price et al. (2020) use linked data trained with the FamilySearch tree and Michelman et al. (2020) use a focused set of FamilySearch links, supplemented by additional hand-linking, to trace elite college graduates in the 1920s and 1930s. Lleras-Muney et al. (2020) study the correlation between longevity and education using FamilySearch links as well.

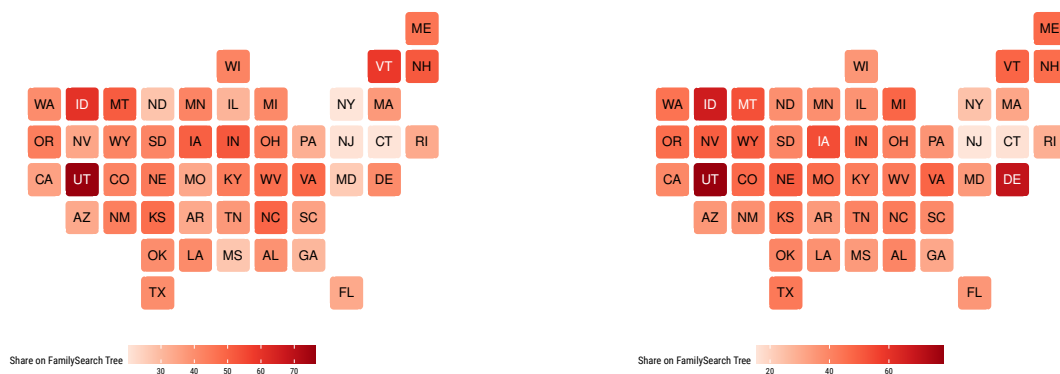Figure B.12: Share of Telephone Operators on the FamilySearch Tree by State of Birth
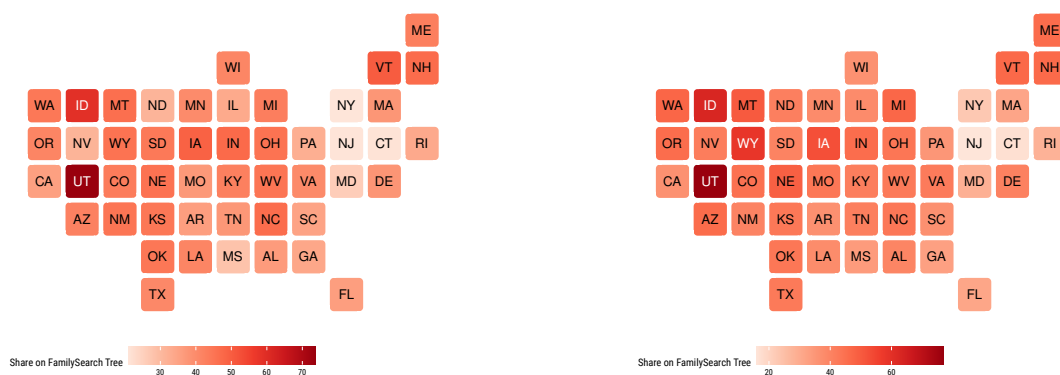
(a) 1920

(b) 1930

Note: We map the share of telephone operators who were found on the tree in the same census we identified them as telephone operators in the telephone industry, plotted according to their states of birth in the census.

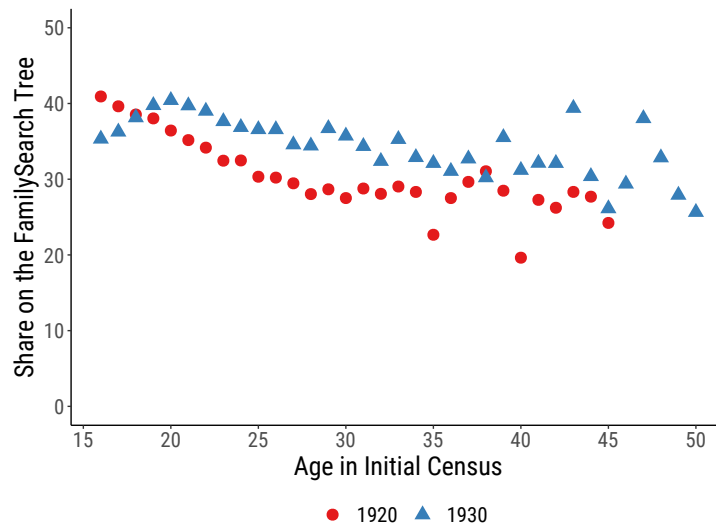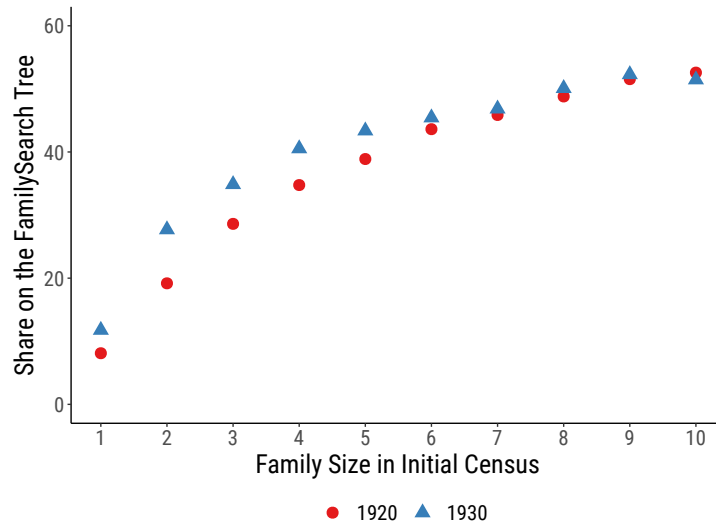Figure B.13: Share of Telephone Operators on the FamilySearch Tree by State of Residence

(a) 1920

(b) 1930

Note: We map the share of telephone operators who were found on the tree in the same census we identified them as telephone operators in the telephone industry, plotted according to their states of residence in the census.

Operators who were younger in the initial census are more likely to be found on the tree, though this relationship is stronger in 1920 than in 1930. We plot the share on the tree by age in Figure B.14. More than 35% of the youngest operators in 1920 are on the FamilySearch tree, falling to around 27% around age 30 where the rate plateaus. In 1930, by contrast, tree rates fluctuate around 35% for most ages, falling a bit for the few operators older than 35.

Figure B.14: Share of Telephone Operators on the FamilySearch Tree by Age

Note: We plot the share of telephone operators who were found on the tree in the same census we identified them as telephone operators in the telephone industry according to their age in the initial census.

As most people are added to the FamilySearch tree by descendants—and the FamilySearch hints are often based on own name and names of other people in the household—those in larger families are more likely to be on the tree. We plot this relationship in Figure B.15. In both 1920 and 1930, the share of women operators on the tree who were in households alone is quite low, less than 10% in 1920 and about 12% in 1930. This rises monotonically with family size in both censuses.[16]

Name commonness is a pervasive challenge for census linking, whether by hand or by automated methods (Abramitzky et al. 2020, Bailey et al. 2017). When there are multiple people with the same name, humans and algorithms can struggle to determine which is the correct person and which is the doppelganger. This challenge affects our tree linking as well. Women with the most common first names are least likely to be on the tree, as we show in Figure B.16a, where we split women into deciles by name commonness. However, the differences are relatively small: women with the least common first names are on the tree in 1920 and 1930 about 37% of the time, compared to 27% for women with the most common first names. The pattern among last names is a bit different. Women with incredibly rare last names are very unlikely to be on the tree (25.8% in 1920 and 27.9% in 1930), possibly because such women have been enumerated or transcribed with error. That is, these rare last names may not actually be rare, but rather noise and error, and a name transcribed incorrectly might make it hard to connect it to other records for that person. Otherwise, the relationship between last name commonness and being on the tree is relatively flat, fluctuating between 30 and 40% in both 1920 and 1930.

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[16]We measure family size in the census with the `famsize` variable from IPUMS, defined as "the number of own family members residing with each individual, including the person her/himself. Persons not living with others related to them by blood, marriage/cohabitating partnership, or adoption are coded 1."

Figure B.15: Share of Telephone Operators on the FamilySearch Tree by Family Size

Note: We plot the share of telephone operators who were found on the tree in the same census we identified them as telephone operators in the telephone industry according to the size of their family in their household in the initial census. Specifically, we use the IPUMS variable `famsize`.

Figure B.16: Share of Telephone Operators on the FamilySearch Tree by Name Commonness
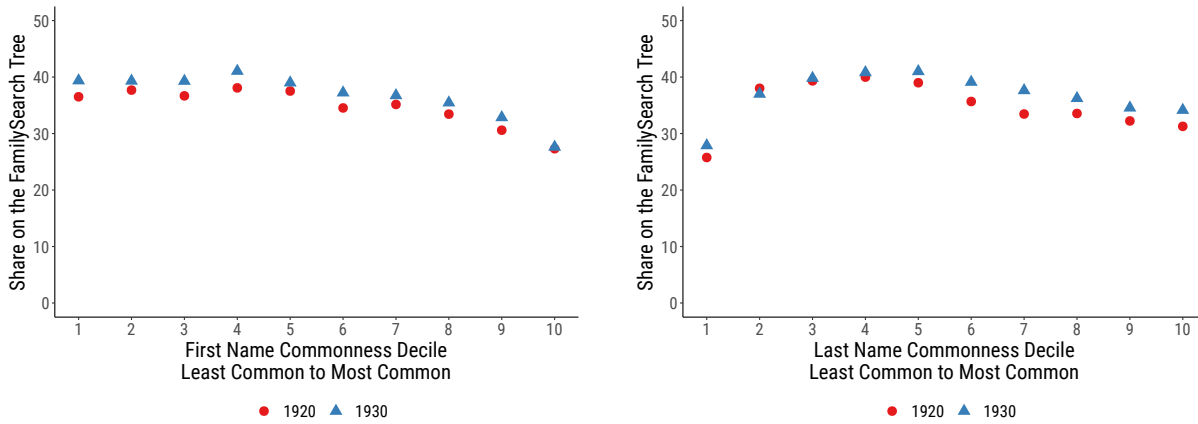
(a) Commonness of First Name

(b) Commonness of Last Name

Note: We plot the share of telephone operators who were found on the tree in the same census we identified them as telephone operators in the telephone industry according to the commonness of their first or last names. Name commonness is calculated using the complete count censuses in 1900, 1910, 1920, and 1930.

Similar to how rare first or last names may help census linkers or genealogists distinguish one person from another, middle names or initials could play a similar role. 28% of the operators enumerated in the census have a middle initial reported, while only about 1% have a full middle name. In both cases, these people are more likely to be on the FamilySearch tree, ranging from about 43% with a middle initial or name down to 32% without either listed.

Figure B.17: Share of Telephone Operators on the FamilySearch Tree by Middle Name Status
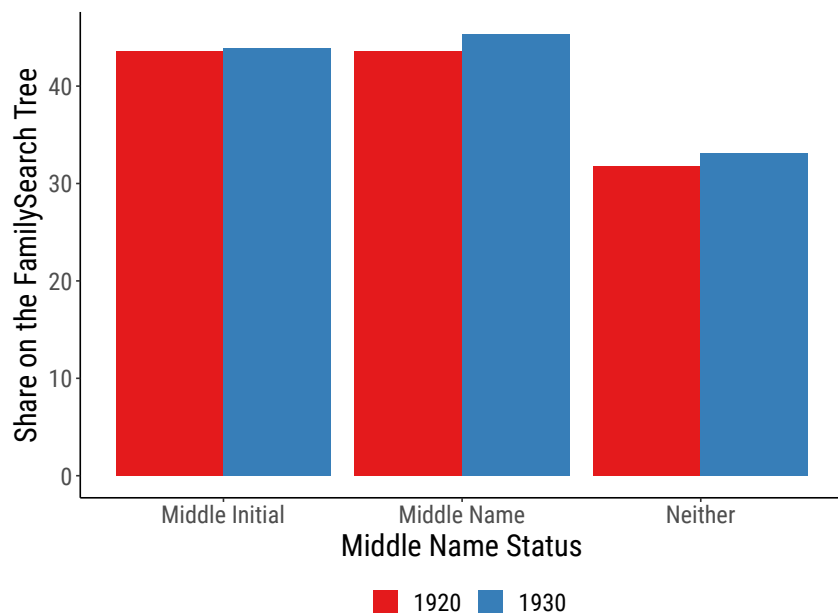
Ultimately, our goal is to estimate the effect of automation on incumbent operators. If operators in cities with cutovers over the following decade are differentially likely—either more or less—to be found on the tree, that could cause issues with inference, at a minimum by shifting sample weight across cities which were or were not treated. Is this the case? Can *future* cutovers predict whether women are on the tree in the census before the cutover? As we show in Table B.3, the answer is no. Having a cutover over the next decade—that is, being in the treated group—does not significantly predict whether we were able to find an operator on the FamilySearch tree (column 1). Moreover, the relative magnitudes of these effects are small: a cutover in the next decade increases the probability a woman is found on the tree by about 0.3 p.p. from a base tree rate of 35.6%.

We also see that having a cutover in the next decade does not predict whether we were ultimately able to link an operator to a record in the next census, as we show in column 2 of Table B.3. Here the relative magnitudes are also small. Conditional on being on the FamilySearch tree, about half of women are found in the next census. A cutover reduces this rate by a statistically insignificant 0.8 p.p.

Table B.3: Telephone Cutovers Do Not Predict Operator
FamilySearch Tree Status

	On the FamilySearch Tree	Linked to Next Census
	(1)	(2)
Cutover Previous Decade	0.003	0.008
	(0.007)	(0.007)
City Controls	Yes	Yes
Individual Controls	Yes	Yes
Year FEs	Yes	Yes
State FEs	Yes	Yes
Observations	157,293	55,928

Note: We analyze whether or not an operator is on the FamilySearch tree (column 1) or whether or not an operator is successfully linked to the following census conditional on being on the FamilySearch tree (column 2). We cannot replicate the empirical strategy from our the individual-level analysis in the paper which includes city by year fixed effects (enabled because we compare operators with control women who were not operators). Instead, we include a suite of city-level and individual-level controls. All regressions are unweighted. Individual controls include fixed effects for age, birthplace, race, marital status, family size, middle names or initials, all measured in the base year, as well as controls for first and last name commonness. City controls include population, the share of women with occupations, the black and foreign-born shares of the population, and the share of people in the city who were operators in the telephone industry, all measured in the base year. SEs clustered by city in parentheses.

B.3.4 Linking Control Women

When we analyze the individual-level linked data, we use a differences in differences specification: we compare the outcomes in the following census of telephone operators living in cities with and without cutovers over the following decade, and—to account for any secular trends in the local labor market—we compare operators to other working women living in their neighborhoods. In this section, we detail how we construct this control sample.

For every telephone operator in our sample, we identify a set of women who are "similar" in the complete count census and can serve as a control for her. The size of the complete count census allows us to match on a number of dimensions. Specifically, we focus on women of the same race and Hispanic status and within five years of age. We also match on marital status and fertility; if the operator is married, her controls are as well, and if the operator has children, her controls must have children as well. We also use the operator's birthplace and her mother and father's birthplaces to link on nativity; if the operator is US-born, her controls must be US-born, and the same for her mother's US-born status and her father's US-born status. Finally, we match on precise geography, using census enumeration districts. In 1920 and 1930, enumeration districts contained approximately 1000 residents (usually 200 to 300 households) and were geographically compact units that we think of as neighborhoods. Finally, we require control women to have an occupation

in the initial census so that we compare telephone operators to other working women.

As we document in Table B.4, we locate approximately 21 control women for each telephone operator in 1920 and 1930. We then follow the linking procedure outlined in Appendix Subsection B.3.1 to locate these control women on FamilySearch and find them in the following census. Of 459,786 matched control women, 191,761 are on the tree (41% versus 36% for operators).[17] 97,922 of the control women are attached to records in the following census. These women form our final control set, providing nearly five control observations for every operator observation.

Table B.4: Share of Control Women on the Family Search Tree, 1920-1930

| Year | Control Women | | On the Tree | | Linked to Next Census | |
	Unique	per Operator	Unique	per Operator	Unique	per Operator
Pooled	459786	21.4	191761	9.2	97922	4.7
1920	273024	21.4	113990	9.2	58710	4.8
1930	186762	21.4	77771	9.2	39212	4.6

While we see 4.7 control women per operator on average, there is wide variation in the actual number of control women for each operator in the sample (Figure B.18). In both 1920 and 1930, about three-quarters of our operators have at least one linked control woman. In both years, about 14% of operators have only one linked control woman and another 10% have two. At the other end of the range, 16% of operators are matched to more than ten linked control women.

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[17]Note that compared to the operator sample, a higher share of control women are on the tree. However, this is not surprising, as we have limited our search to the control women of operators whom we already successfully linked. These control women thus come from the same neighborhoods (and age, race, and family nativity, etc.) of linked operators, not random operators, and are selected on being more "linkable".

Figure B.18: Control Women Linked to Next Census per Telephone Operator.
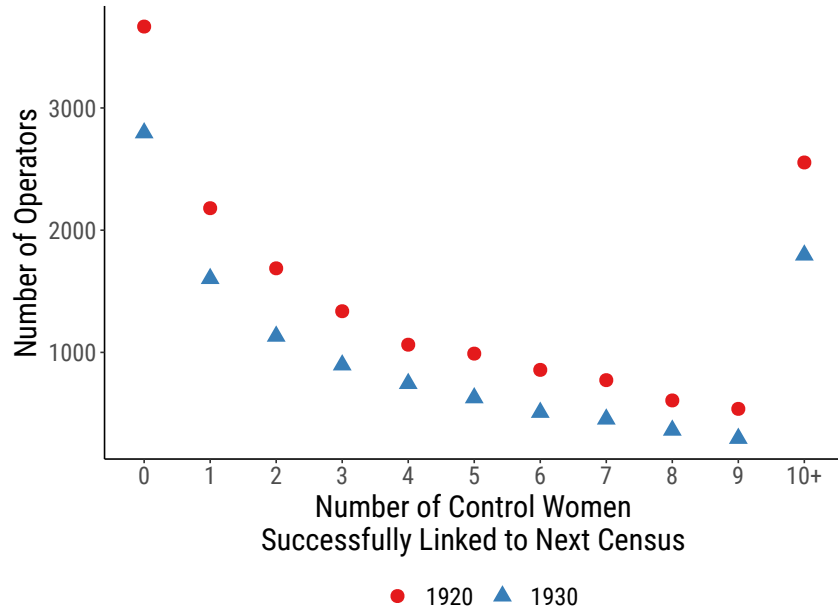
We do not enforce uniqueness among controls for operators. That is, one control woman could serve as a control for multiple operators in our data, if the operators (and the control) live in the same enumeration district and match on all the required criteria. More than 75% of control women in 1920 and 1930 are matched to only one operator. Of the controls matched to multiple operators, 17% are matched to only two operators and only a handful are matched to more than 6 operators (50 in 1920 and 35 in 1930; none matched to more than 8 operators).

B.4   Additional (supplementary) data

We also collect data from additional sources which have ancillary uses in this paper.

AT&T data on dial diffusion, 1913-1972

Archival documents at the AT&T corporate archives include a two-page report providing the annual time series of the total number of Bell system telephones from 1913 to 1972, and a breakdown by the type of central office, manual versus dial (see "Bell System Distributions of Company Telephones," AT&T Archives and History Center, box 085-04-03-02). We use these data to chart aggregate dial diffusion within the Bell system (shown in Figure 1).

AT&T subscribers in large cities, 1915-1940

In addition to the AT&T dial diffusion data, we also collect data on annual local *telephone* adoption U.S. cities with over 50,000 population. The AT&T publication "Bell Telephones in Principal

Cities" (AT&T 1915) was published annually, and made available to us by the AT&T Archives and History Center for years 1915 to 1940. Each volume of this publication reports the number of Bell system telephone stations in each city, as well as an estimate of the service area population (for measuring telephone penetration), and a breakdown of the percent of telephones which are business subscribers (vs. residence). In Feigenbaum and Gross (2020), we use these data to study how cutovers were related to AT&T network growth.

NHGIS shapefiles, GNIS place names, and other geographic data

We use the USGS Geographic Names Information System (GNIS) database of U.S. populated places to link cities to their counties (manually disambiguating cities of the same name in the same state, as needed) and retrieve their geographic coordinates. We use these data with 1940 state boundary shapefiles from NHGIS (Ruggles et al. 2019) to map cutovers (as in Figure 2), but these data are not otherwise used in the analysis.

Measures of local severity of the Great Depression

We use data from Fishback et al. (2005) on county per-capita retail sales growth from 1929 to 1933, 1935, and 1939 to measure local severity of the Great Depression. See the appendix to Fishback et al. (2005) for information on data sources. To each city in our city sample, we assign retail sales growth of the surrounding county. For cities that span county boundaries, we compute weighted averages, weighting by the city population in each associated county.

National Industrial Conference Board 1926 clerical salary survey

We use historical survey data on salaries of clerical workers to identify clerical occupations with a similar earnings profiles to telephone operators, which might therefore be natural substitutes. In the 1920s, the National Industrial Conference Board (NICB) surveyed employers in large U.S. cities for the job titles and salaries of clerical workers, with survey returns covering 18 cities, 416 firms, and 25,879 employees. The published results (NICB 1926) report salary distributions for 20 different job titles—separately for males and females, and often separately for junior and senior employees—with one of the worker categories being "Switchboard operators".[18]

We identified three surveyed occupations with similar female earnings profiles to telephone operators: "Office labor-saving machine operators", "Junior stenographers", and "Experienced typists", which had median and average earnings of around \$21-22 per week. Table B.5 shows the earnings profile for women in each occupation, and an imputed median (see table notes). These occupations map directly to 1950 occupation codes 341 ("Office machine operators") and 350 ("Stenographers, typists, and secretaries"), which are thus two of the occupations we study in Section 5 when we examine the post-cutover occupations of young women.

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[18]We thank Claudia Goldin for pointing us to and sharing these records.

Table B.5: NICB (1926) Salary Distributions for Select Clerical Occupations

Switchboard operators				Office Machine Operators		
Range	Count	Cum. pct.		Range	Count	Cum. pct.
≤16.50	30	5%		≤15.00	235	10%
16.51 - 18.00	77	16%		15.01 - 17.00	190	18%
18.01 - 19.50	51	24%		17.01 - 19.00	235	28%
19.51 - 21.00	155	47%		19.01 - 21.00	363	44%
21.01 - 22.50	57	56%		21.01 - 23.50	461	63%
22.51 - 25.00	111	73%		23.51 - 26.00	376	79%
25.01 - 27.50	66	83%		26.01 - 30.00	373	95%
≥27.51	116	100%		≥30.01	116	100%

Imputed median: 22.00　　　　　　*Imputed median: 22.71*

Junior Stenographers				Experienced Typists		
Range	Count	Cum. pct.		Range	Count	Cum. pct.
≤17.50	191	20%		≤17.50	263	23%
17.51 - 19.00	104	31%		17.51 - 19.00	105	33%
19.01 - 20.50	97	41%		19.01 - 20.50	196	50%
20.51 - 22.00	123	54%		20.51 - 22.00	149	63%
22.01 - 23.50	151	69%		22.01 - 23.50	153	77%
23.51 - 25.00	78	77%		23.51 - 25.00	89	84%
25.01 - 26.50	97	88%		25.01 - 27.50	96	93%
≥26.51	120	100%		≥27.51	81	100%

Imputed median: 20.97　　　　　　*Imputed median: 20.50*

Notes: Table reports salary distribution for female switchboard operators, office machine operators, junior stenographers, and experienced typists from NICB (1926). Medians imputed assuming a uniform distribution in each bin in which the median salary occurs (i.e., as a weighted average of the lower and upper bounds of the bin in which the true median falls).

C Supplemental Descriptive Figures/Tables

Here we provide additional descriptive statistics for young, white, American-born women over the sample period. Table C.1 lists the most common occupations for this demographic in 1920, and their rank in other years. Table C.2 does the same for occupation-industry pairings, where telephone operator is at the top of the list. Tables C.3 and C.4 provide a more detailed view of the characteristics of telephone operators by region and city size.

Table C.1: Top occupations for white, American-born women age 16 to 20, 1910-1940

| | Rank | | | |
Occupation	1910	1920	1930	1940
Operative and kindred workers	1	1	1	1
Stenographers, typists, and secretaries	4	2	2	3
Salesmen and sales clerks	2	3	3	4
Clerical and kindred workers	10	4	5	5
Telephone operators	5	5	6	13
Private household workers	3	6	4	2
Bookkeepers	8	7	7	8
Laborers	11	8	10	9
Teachers	9	9	11	16
Waiters and waitresses	14	10	8	6

Notes: Table ranks the top 10 occupations employing white, American-born women age 16-20 in the U.S. in 1920, and shows their ranks in other years.

Table C.2: Top occupation-industries for white, American-born women age 16 to 20, 1910-1940

| | | Rank | | | |
Occupation	Industry	1910	1920	1930	1940
Telephone operators	Telephone	4	1	2	14
Salesmen and sales clerks	General merchandise	2	2	3	5
Private household workers	Private households	1	3	1	1
Stenographers, typists, and secretaries	Misc business services	7	4	8	60
Operative and kindred workers	Yarn, thread, and fabric	5	5	6	9
Operative and kindred workers	Apparel and accessories	3	6	4	2
Teachers	Educational services	8	7	11	30
Salesmen and sales clerks	Food stores, except dairy	14	8	10	6
Operative and kindred workers	Knitting mills	10	9	12	18
Operative and kindred workers	Footwear, except rubber	12	10	14	19

Notes: Table ranks the top 10 occupation-industry pairs employing white, American-born women age 16-20 in the U.S. in 1920, and shows their ranks in other years. Note that the decline in "Stenographers, typists, and secretaries" in the "Misc business services" industry from 1930 to 1940 is due to a change in industry classification (the occupation overall was growing).

Table C.3: Characteristics of telephone operators, 1910-1940, by census region

	Telephone industry				Other industries			
	1910	1920	1930	1940	1910	1920	1930	1940
Northeast								
Population (1000s)	23.51	48.76	68.74	50.44	0.80	2.33	9.36	14.43
Percent female (%)	90.6	95.2	96.4	90.1	29.6	70.6	86.6	85.9
& native-born	86.2	91.0	91.6	85.5	27.2	65.8	81.4	81.1
& white/non-Hisp	85.9	90.7	91.4	85.0	26.8	64.6	80.7	80.4
& young (16-25)	72.4	70.0	61.3	23.5	19.0	42.4	42.0	20.8
Midwest								
Population (1000s)	30.86	49.34	63.24	50.61	0.95	2.00	7.91	13.66
Percent female (%)	91.8	95.0	96.2	93.4	20.5	63.9	87.1	89.1
& native-born	88.2	90.8	92.2	90.6	19.4	60.1	82.7	85.8
& white/non-Hisp	88.1	90.7	92.1	90.3	19.4	60.0	82.2	85.3
& young (16-25)	72.6	66.8	58.5	29.7	14.2	39.3	41.9	24.6
South								
Population (1000s)	11.59	23.05	31.70	32.06	0.39	0.87	3.23	7.82
Percent female (%)	83.9	91.8	95.8	90.7	10.7	60.0	82.6	85.2
& native-born	83.4	90.9	95.2	90.2	10.7	58.8	81.7	84.2
& white/non-Hisp	82.9	90.4	94.7	89.4	10.7	57.0	80.2	82.9
& young (16-25)	68.0	70.2	62.3	29.5	6.4	36.0	40.2	21.0
West								
Population (1000s)	7.06	13.48	18.37	19.59	0.26	0.54	2.34	5.25
Percent female (%)	91.5	95.8	96.0	94.6	24.2	70.2	88.7	90.6
& native-born	86.6	90.0	90.3	90.2	22.6	65.3	83.1	86.2
& white/non-Hisp	85.6	89.2	89.5	89.2	22.3	64.5	82.5	84.9
& young (16-25)	72.6	66.3	48.9	31.4	12.8	36.2	35.1	21.8

Notes: Table reports characteristics of telephone operators in the telephone industry and in other industries, by census region and year. Data for the pooled population can be seen in Table 2 of the paper.

Table C.4: Characteristics of telephone operators, 1910-1940, by city size

|	Telephone industry				Other industries			
	1910	1920	1930	1940	1910	1920	1930	1940
Cities w/ pop. 2-5k								
Population (1000s)	6.25	9.23	12.13	8.05	0.21	0.29	0.83	2.22
Percent female (%)	90.2	94.9	97.7	92.5	13.2	48.1	79.3	87.2
& native-born	88.5	93.1	95.6	89.8	12.3	47.7	76.9	83.9
& white/non-Hisp	88.2	92.9	95.4	89.3	12.3	47.7	76.8	82.8
& young (16-25)	72.1	67.6	56.3	28.5	8.5	31.7	40.3	21.2
Cities w/ pop. 5-10k								
Population (1000s)	4.64	7.42	9.52	6.78	0.13	0.22	0.70	1.75
Percent female (%)	93.0	95.7	97.2	92.3	10.8	45.4	81.3	87.4
& native-born	91.1	93.6	95.0	89.3	10.8	43.6	79.3	84.3
& white/non-Hisp	90.7	93.3	94.7	88.7	10.8	43.1	78.7	83.6
& young (16-25)	75.3	71.4	60.3	28.6	7.7	25.7	40.1	22.8
Cities w/ pop. 10-20k								
Population (1000s)	4.56	7.55	10.71	7.03	0.12	0.27	1.06	1.85
Percent female (%)	92.3	95.9	97.0	91.3	20.5	60.3	87.1	86.0
& native-born	89.7	93.7	94.8	88.6	20.5	57.4	83.9	83.3
& white/non-Hisp	89.5	93.4	94.6	87.9	20.5	57.4	83.6	82.6
& young (16-25)	75.6	73.3	64.1	30.1	18.0	40.4	46.5	24.5
Cities w/ pop. 20-50k								
Population (1000s)	5.82	10.48	15.63	10.69	0.14	0.48	1.90	2.78
Percent female (%)	92.1	95.9	96.4	92.4	20.4	67.5	87.6	86.5
& native-born	88.6	92.4	93.1	89.1	19.7	63.6	84.0	83.0
& white/non-Hisp	88.2	92.0	92.7	88.7	19.7	63.4	83.5	82.3
& young (16-25)	74.9	73.2	65.7	28.9	13.9	44.3	45.4	23.7
Cities w/ pop. 50-100k								
Population (1000s)	4.04	8.78	11.66	8.72	0.09	0.40	1.70	2.19
Percent female (%)	90.8	95.2	95.9	92.3	31.6	75.9	87.9	87.3
& native-born	87.2	91.6	92.5	90.0	28.4	71.1	83.9	84.2
& white/non-Hisp	87.0	91.3	92.2	89.4	28.4	70.6	83.4	83.4
& young (16-25)	75.5	73.2	63.8	27.6	24.2	47.3	41.7	23.7
Cities w/ pop. 100-200k								
Population (1000s)	4.44	9.78	12.20	8.18	0.10	0.47	1.64	2.25
Percent female (%)	90.6	95.1	96.0	92.9	20.6	79.2	89.1	87.2
& native-born	86.9	92.0	93.2	90.6	15.5	73.7	85.7	84.6
& white/non-Hisp	86.5	91.8	92.9	90.1	15.5	72.8	84.7	83.5
& young (16-25)	74.9	74.5	65.3	27.2	10.3	50.1	39.0	19.2
Cities w/ pop. >200k								
Population (1000s)	25.89	52.63	70.75	39.37	0.54	2.25	11.16	11.51
Percent female (%)	91.6	95.5	96.2	92.2	45.5	81.5	88.3	89.4
& native-born	86.3	89.9	90.5	87.9	42.2	76.0	83.0	84.6
& white/non-Hisp	85.9	89.4	90.1	87.1	41.4	74.2	82.0	83.7
& young (16-25)	75.2	70.1	61.3	25.7	30.0	47.6	41.7	21.3

Notes: Table reports characteristics of telephone operators in the telephone industry and in other industries, by city size and year. Breakdowns by city size are for the 3,027 cities in our primary sample (see Appendix B). Data for the pooled population can be seen in Table 2 of the paper.

D First Stage, Balance Tests and Event Study Pre-Trends

D.1 Differential effects of cutovers across subpopulations

Figure D.1 presents event study estimates of the effects of dial cutovers on the fraction of different population groups who are telephone operators in the telephone industry, estimated with 10-year intervals. Consistent with the evidence from Table 2 that the majority of operators were young, white, American-born women, we see that the employment share reduction is largest for this group, further motivating our focus on young women throughout the paper.

Figure D.1: Effect of dial cutovers on the fraction of different demographic groups who are telephone operators in the telephone industry (event study, 10-yr intervals)
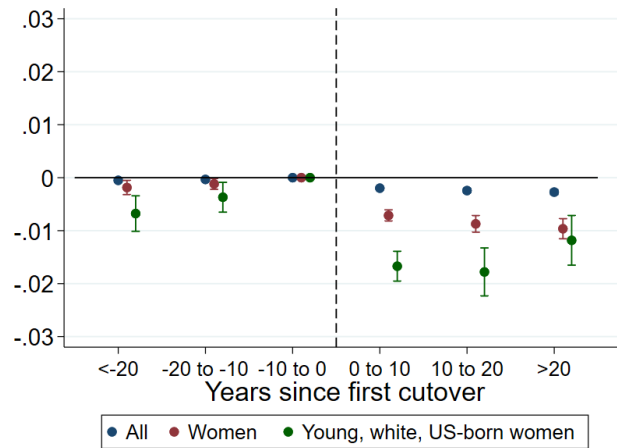
Notes: Figure shows event study estimates of the effects of local dial adoption on the fraction of increasingly narrow adult populations (from all adults to young, white, American-born women) in successive cohorts who are telephone operators in the telephone industry, for the small city sample (population ≤100k in 1920). Error bars represent 95% confidence intervals, computed from SEs clustered at the city level.

D.2 Pre-treatment balance tests

Figure D.2: Pre-treatment run-up balance tests for the outcomes studied throughout the paper
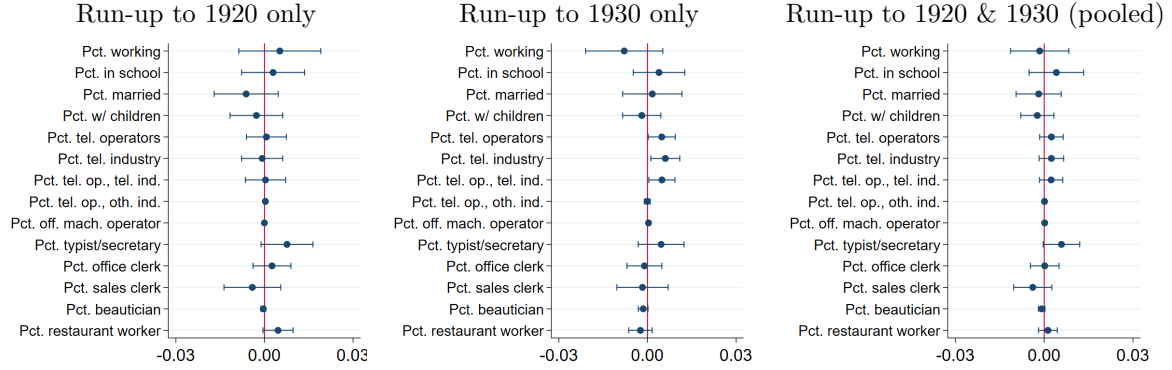
Notes: Figures estimate differences in the prior 10-year *changes* in each of the variables shown across cities with versus without an imminent cutover. For example, the left panel examines cities which in 1920 had not yet had a cutover in our data, calculates the 1910 to 1920 change in a given outcome (ΔY), and estimates the conditional difference in means of ΔY between cities which did versus did not have a cutover between 1920 and 1930, controlling for city size. The middle panel repeats for 1930, and the right panel pools 1920 and 1930 together. All outcomes are measured for young, white, American-born women only. The evidence indicates that most outcomes were not changing differentially (over the prior 10 years) in cities with an impending cutover, with the exception of employment in telephone operation, which was growing prior to cutovers, and possibly employment in secretarial and restaurant work, which we show in the paper absorbed many of the women who might have otherwise been operators. In all figures, sample restricted to cities with population ≤100k in 1920. Error bars represent 95% confidence intervals, computed from SEs clustered at the city level.

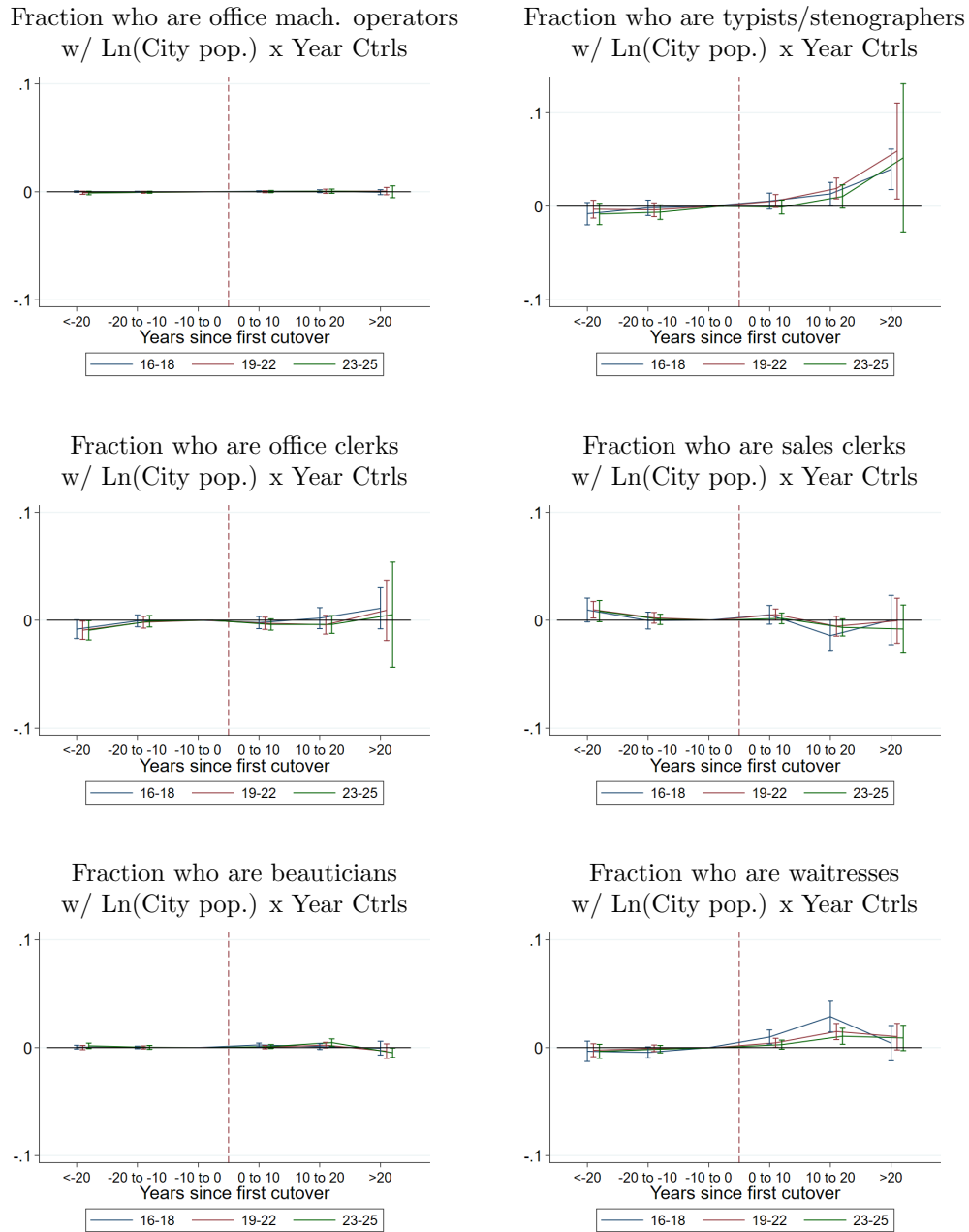## D.3   Event studies for labor market outcomes, by age

In Figure D.3 and D.4, we show the event study estimates for labor market outcomes of successive cohorts of young, white, American-born women around cutovers, splitting the population into the same age groups as in Tables 4 and 5: 16 to 18, 19 to 22, and 23 to 25.

Figure D.3: Changes in work, education, marriage, and fertility patterns for young, white, American-born women around cutovers (event study, by age)
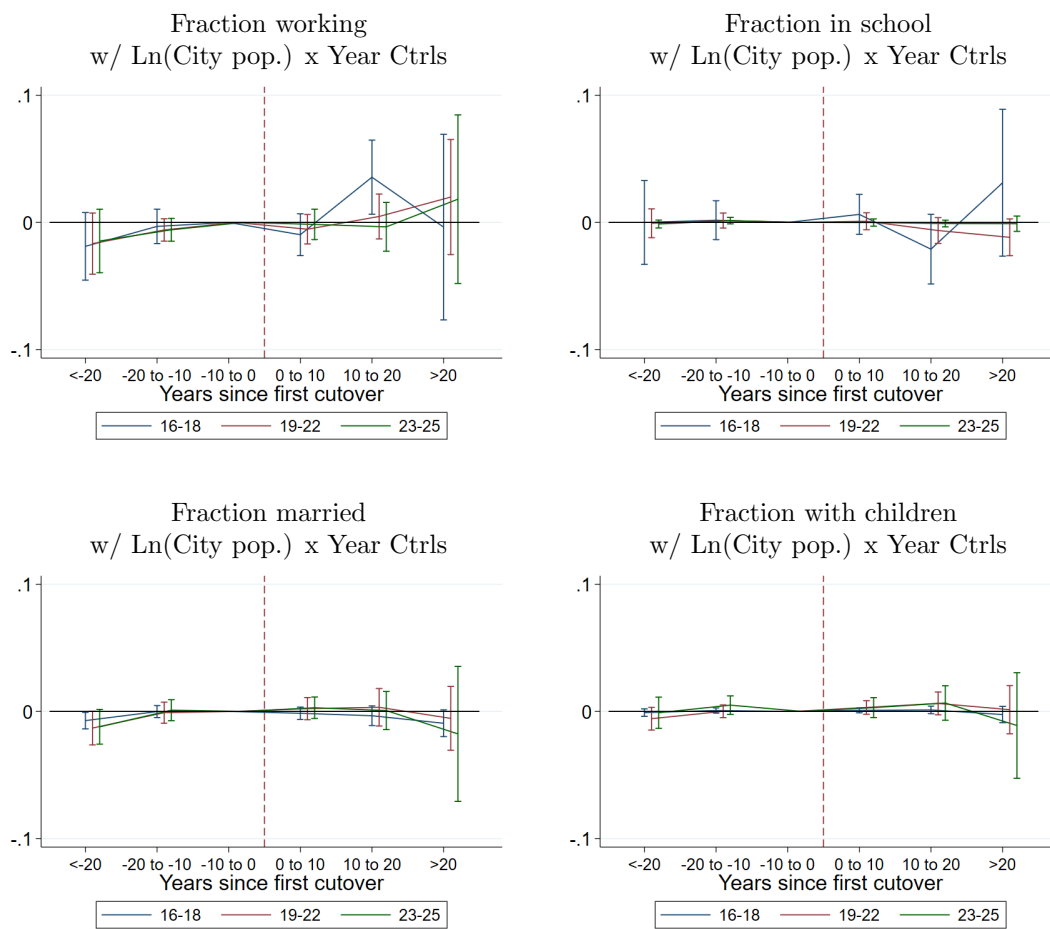
Notes: Figure shows event study estimates, by age, of the effects of local dial adoption on the fraction of young, white, American-born women in successive cohorts who are in the labor force, in school, married, and have children, for cities with population ≤100k in 1920. Error bars represent 95% confidence intervals, computed from SEs clustered at the city level.

Figure D.4: Changes in employment shares in select occupations for young, white, American-born women around cutovers
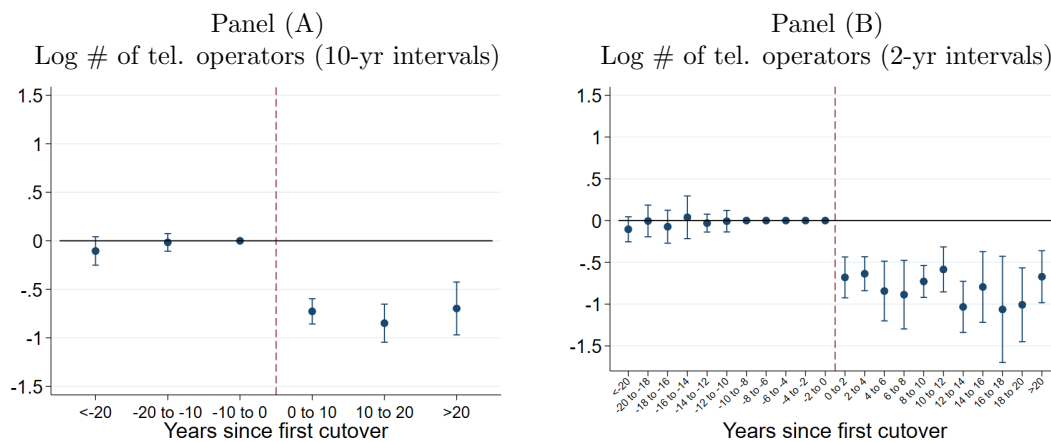

Fraction who are office mach. operators
w/ Ln(City pop.) x Year Ctrls

Fraction who are typists/stenographers
w/ Ln(City pop.) x Year Ctrls

Fraction who are office clerks
w/ Ln(City pop.) x Year Ctrls

Fraction who are sales clerks
w/ Ln(City pop.) x Year Ctrls

Fraction who are beauticians
w/ Ln(City pop.) x Year Ctrls

Fraction who are waitresses
w/ Ln(City pop.) x Year Ctrls

Notes: Figure shows event study estimates, by age, of the effects of local dial adoption on young, white, American-born women's employment shares in select occupations, across successive cohorts, for cities with population ≤100k in 1920. Error bars represent 95% confidence intervals, computed from SEs clustered at the city level.

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E   Robustness Checks and Heterogeneity

E.1   Robust event study estimation methods

Appendix Figure E.1 presents a robustness check on the event study results in Figures 3 and 4 of the paper, evaluating their robustness to alternative event study estimation methods, as suggested by recent developments in applied econometrics. See Section 4.2 for discussion.

Figure E.1: Effect of dial cutovers on the log number of young, white, American-born women and fraction of the same who are telephone operators in the telephone industry (robust event study)
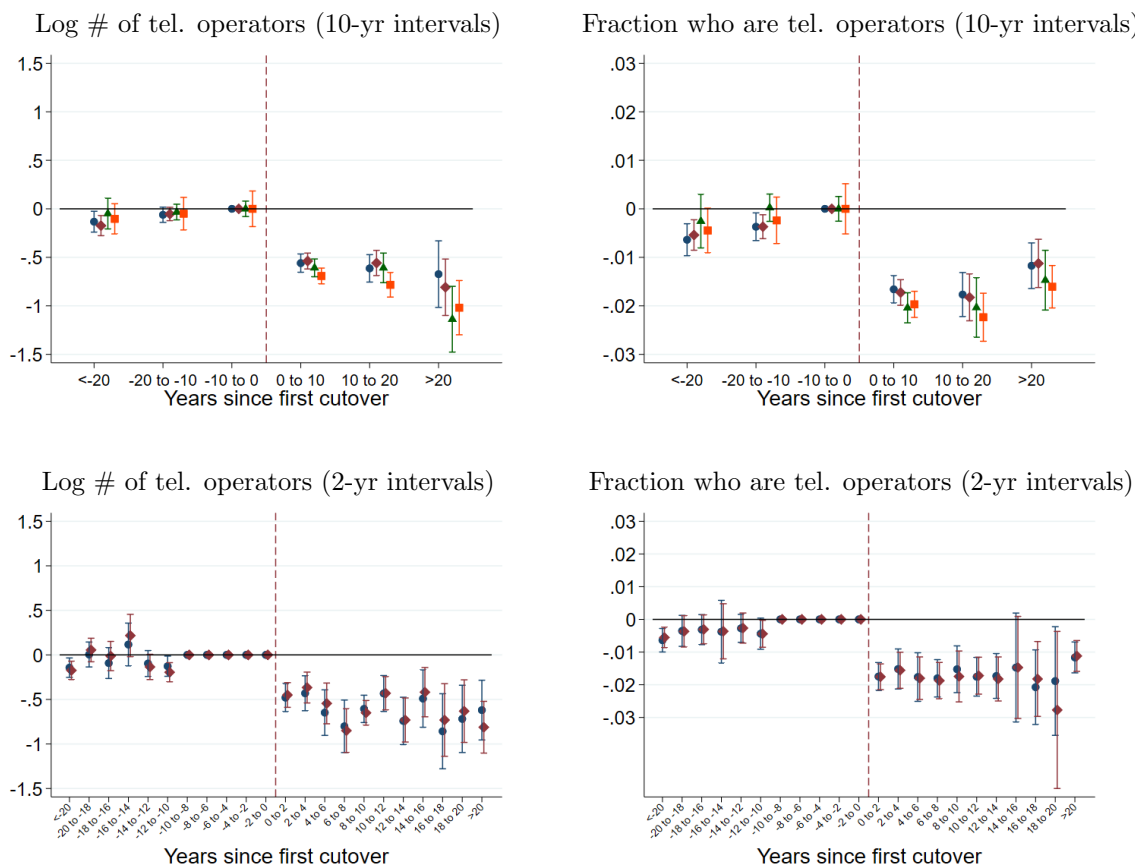
Notes: Left panels show event study estimates of the effects of local dial adoption on the (log) number of young, white, American-born women in successive cohorts who are telephone operators in the telephone industry, for the small city sample (population ≤100k in 1920), with 10- and 2-year event windows. When event windows are narrower than the 10-year frequency at which outcomes are measured, each bin contains different cities (every fifth bin represents the same set of cities). Right panels show event study estimates for the fraction of working young, white, American-born women who are the same. The figure provides results by four estimation methods: (i) vanilla OLS (blue circle), (ii) Sun and Abraham (2020) (red diamond), (iii) Callaway and Sant'Anna (2020) (green triangle), and (iv) Borusyak et al. (2021) (orange square). With 2-year intervals, only the first two methods are estimated. Error bars represent 95% confidence intervals, computed from SEs clustered at the city level.

E.2 Restricting to cities with continuous newspaper coverage

Here we present robustness checks restricting the estimation sample to cities with continuous newspaper coverage in our data sources from 1917 to 1940. Figures E.2 to E.4, and Tables E.1 and E.2, are counterparts to Figures 3 to 5, and Tables 4 and 5, in the body of the paper.
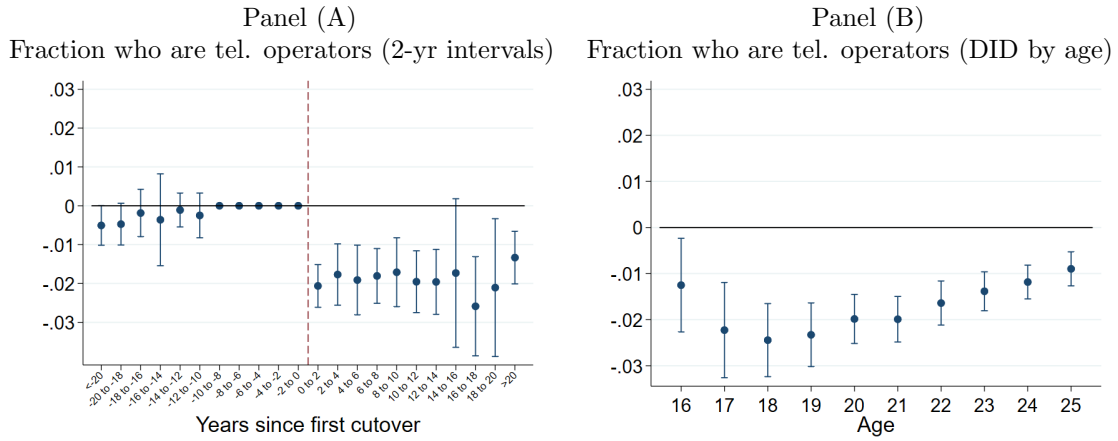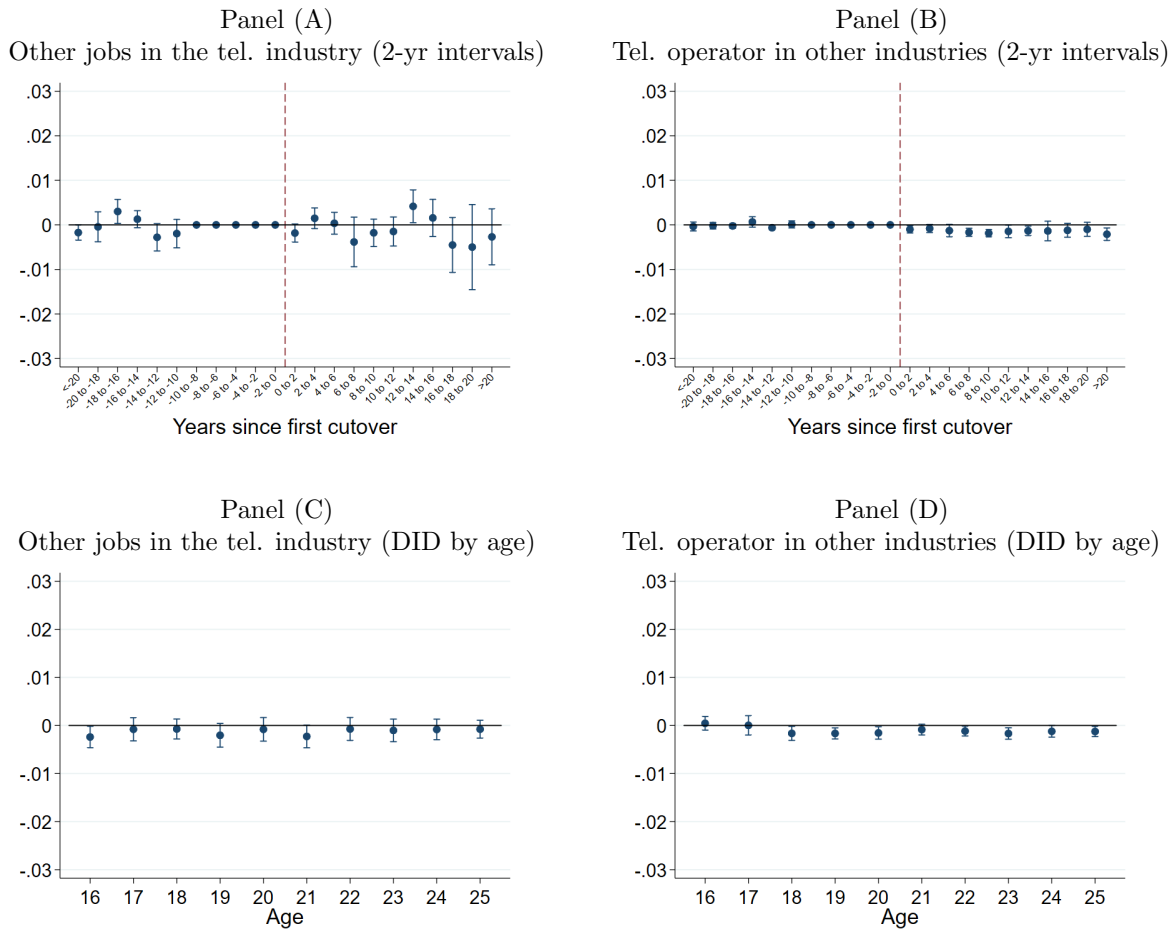
Effects on Operator Employment

Figure E.2: Effect of dial cutovers on the log number of young, white, American-born women who are telephone operators in the telephone industry (event study)

Notes: Figure shows event study estimates of the effects of local dial adoption on the (log) number of young, white, American-born women in successive cohorts who are telephone operators in the telephone industry, for the small city sample (population ≤100k in 1920), with 10- and 2-year event windows. When event windows are narrower than the 10-year frequency at which outcomes are measured, each bin contains different cities (every fifth bin represents the same set of cities). Sample restricted to cities with continuous newspaper coverage from 1917 to 1940. Error bars represent 95% confidence intervals, computed from SEs clustered at the city level.

Figure E.3: Effect of dial cutovers on the fraction of working young, white, American-born women who are telephone operators in the telephone industry (event study and DID by age)

Panel (A)
Fraction who are tel. operators (2-yr intervals)

Panel (B)
Fraction who are tel. operators (DID by age)

Notes: Figure shows event study and difference-in-difference estimates (by age) of the effects of local dial adoption on the fraction of working young, white, American-born women in successive cohorts who are telephone operators in the telephone industry, for the small city sample (population ≤100k in 1920). Sample restricted to cities with continuous newspaper coverage from 1917 to 1940. Error bars represent 95% confidence intervals, computed from SEs clustered at the city level.

Figure E.4: Effect of dial cutovers on the fraction of working young, white, American-born women with other jobs in the telephone industry or who are telephone operators in other industries

Notes: Upper panels show event study estimates of the effects of local dial adoption on the fraction of working young, white, American-born women in successive cohorts who have other jobs in the telephone industry (left) and who are telephone operators in other industries (right), for the small city sample (population ≤100k in 1920). Sample restricted to cities with continuous newspaper coverage from 1917 to 1940. Because event windows are narrower than the 10-year frequency at which outcomes are measured, each bin contains different cities (every fifth bin represents the same set of cities). Lower panels show the associated difference-in-differences estimates, by age. We plot the estimates on the same scale (-0.03 to 0.03) as the previous figures to ease comparison. Error bars represent 95% confidence intervals, computed from SEs clustered at the city level.

Effects on the Labor Market for Young Women

Table E.1: Changes in work, education, marriage, and fertility patterns around cutovers
sample restricted to cities with continuous newspaper coverage from 1917 to 1940

Panel A: White, American-born women ages 16 to 18

	Tel. oper.	Working	In school	Married	Has children
		Fraction of the group that is:			
Post-cutover	-0.007***	-0.005	-0.005	-0.002	0.000
	(0.001)	(0.011)	(0.012)	(0.003)	(0.001)
N	2088	2088	2088	2088	2088
R^2	0.64	0.86	0.79	0.86	0.77
Cities	522	522	522	522	522
Cut over	137	137	137	137	137
Y Mean	0.01	0.31	0.52	0.09	0.03

Panel B: White, American-born women ages 19 to 22

	Tel. oper.	Working	In school	Married	Has children
		Fraction of the group that is:			
Post-cutover	-0.011***	-0.009	-0.005	0.003	0.002
	(0.001)	(0.009)	(0.004)	(0.005)	(0.003)
N	2088	2088	2088	2088	2088
R^2	0.61	0.84	0.83	0.87	0.83
Cities	522	522	522	522	522
Cut over	137	137	137	137	137
Y Mean	0.01	0.49	0.11	0.37	0.19

Panel C: White, American-born women ages 23 to 25

	Tel. oper.	Working	In school	Married	Has children
		Fraction of the group that is:			
Post-cutover	-0.006***	-0.005	-0.001	0.002	-0.002
	(0.001)	(0.009)	(0.002)	(0.005)	(0.005)
N	2088	2088	2088	2088	2088
R^2	0.53	0.81	0.75	0.85	0.81
Cities	522	522	522	522	522
Cut over	137	137	137	137	137
Y Mean	0.01	0.42	0.02	0.60	0.37

Notes: Tables present difference-in-difference estimates, by age, of the effects of local dial adoption on the fraction of young, white, American-born women in successive cohorts who are in the labor force, in school, married, and have children, for cities with population ≤100k in 1920. Sample restricted to cities with continuous newspaper coverage from 1917 to 1940. The left-most column provides the effect of cutovers on the fraction of these women who were telephone operators in the telephone industry, as a reference point. All regressions include city and year fixed effects, and log city size x year controls. *, **, *** represent significance at the 0.1, 0.05, and 0.01 levels, respectively. SEs clustered by city in parentheses.

Table E.2: Changes in employment shares in select occupations around cutovers
sample restricted to cities with continuous newspaper coverage from 1917 to 1940

Panel A: White, American-born women ages 16 to 18

Conditional on working, fraction employed as or in

	Tel. oper.	Off. mach.	Typist/secr.	Office clerk	Sales clerk	Beautician	Restaurant
Post-cutover	-0.024***	0.000	0.008	-0.003	0.010	0.002**	0.012***
	(0.004)	(0.000)	(0.005)	(0.003)	(0.006)	(0.001)	(0.004)
N	2084	2084	2084	2084	2084	2084	2084
R^2	0.56	0.50	0.64	0.68	0.59	0.43	0.67
Cities	522	522	522	522	522	522	522
Cut over	137	137	137	137	137	137	137
Y Mean	0.03	0.00	0.09	0.04	0.12	0.01	0.05

Panel B: White, American-born women ages 19 to 22

Conditional on working, fraction employed as or in

	Tel. oper.	Off. mach.	Typist/secr.	Office clerk	Sales clerk	Beautician	Restaurant
Post-cutover	-0.020***	0.000	0.007	-0.001	0.010***	-0.000	0.004
	(0.002)	(0.001)	(0.004)	(0.004)	(0.004)	(0.001)	(0.003)
N	2087	2087	2087	2087	2087	2087	2087
R^2	0.61	0.61	0.81	0.77	0.64	0.77	0.81
Cities	522	522	522	522	522	522	522
Cut over	137	137	137	137	137	137	137
Y Mean	0.03	0.00	0.15	0.05	0.10	0.01	0.04

Panel C: White, American-born women ages 23 to 25

Conditional on working, fraction employed as or in

	Tel. oper.	Off. mach.	Typist/secr.	Office clerk	Sales clerk	Beautician	Restaurant
Post-cutover	-0.012***	-0.000	0.000	-0.003	0.005	0.002	0.003
	(0.001)	(0.001)	(0.004)	(0.004)	(0.004)	(0.001)	(0.003)
N	2086	2086	2086	2086	2086	2086	2086
R^2	0.50	0.60	0.79	0.74	0.57	0.75	0.79
Cities	522	522	522	522	522	522	522
Cut over	137	137	137	137	137	137	137
Y Mean	0.02	0.00	0.14	0.05	0.09	0.01	0.04

Notes: Tables present difference-in-difference estimates, by age, of the effects of local dial adoption on young, white, American-born women's employment shares in select occupations, across successive cohorts, for cities with population ≤100k in 1920. Sample restricted to cities with continuous newspaper coverage from 1917 to 1940. The left-most column provides the effect of cutovers on the fraction of these women who were telephone operators in the telephone industry, as a reference point. The other occupations across columns are: (i) office machine operators, (ii) typists, stenographers, and secretaries, (iii) other office clerks, (iv) sales clerks, (v) beauty parlor workers, and (vi) restaurant workers. All regressions include city and year fixed effects, and log city size x year controls. *, **, *** represent significance at the 0.1, 0.05, and 0.01 levels, respectively. SEs clustered by city in parentheses.

E.3   Heterogeneity by region, city size, and exposure

We also explore heterogeneity across cities in the difference-in-difference estimates of the effects of cutovers, which we present in the tables below. Table E.3 estimates effects on employment of our focal demographic group (young, white, American-born women) in telephone operation, in the telephone industry, and overall, by geographic region, and Table E.4 does so for the fraction who are working in select other occupations. Tables E.5 and E.6 repeat by city size. Tables E.7 and E.8 repeat by local "exposure" to automation, which we define as the fraction of young women in 1910 who were telephone operators in the telephone industry.

Heterogeneity by region

Table E.3: Effects of dial on fraction of young, white, American-born women who are telephone operators, in telephone industry, or employed at all

| | | Fraction who are tel. operators | | Fraction non-operators | Fraction with |
	All	In tel. industry	In oth. industry	In tel. industry	occupation
			Sample: Northeast census region		
Post-cutover	-0.010***	-0.009***	-0.001***	-0.001	0.008
	(0.002)	(0.002)	(0.000)	(0.001)	(0.010)
N	2962	2962	2962	2962	2964
R^2	0.63	0.62	0.40	0.55	0.84
Cities	741	741	741	741	741
Cut over	86	86	86	86	86
Y Mean	0.02	0.02	0.00	0.00	0.47
			Sample: Midwest census region		
Post-cutover	-0.019***	-0.018***	-0.001***	-0.002**	0.006
	(0.002)	(0.002)	(0.000)	(0.001)	(0.007)
N	4209	4209	4209	4209	4212
R^2	0.56	0.57	0.43	0.44	0.76
Cities	1053	1053	1053	1053	1053
Cut over	89	89	89	89	89
Y Mean	0.03	0.03	0.00	0.00	0.40
			Sample: South census region		
Post-cutover	-0.020***	-0.019***	-0.002***	-0.002	-0.010
	(0.003)	(0.003)	(0.000)	(0.002)	(0.009)
N	3238	3238	3238	3238	3244
R^2	0.59	0.59	0.48	0.49	0.79
Cities	811	811	811	811	811
Cut over	60	60	60	60	60
Y Mean	0.04	0.04	0.00	0.01	0.34
			Sample: West census region		
Post-cutover	-0.027***	-0.025***	-0.002**	-0.002	-0.036
	(0.005)	(0.005)	(0.001)	(0.001)	(0.023)
N	1236	1236	1236	1236	1236
R^2	0.52	0.54	0.50	0.39	0.61
Cities	309	309	309	309	309
Cut over	26	26	26	26	26
Y Mean	0.04	0.04	0.00	0.01	0.33

Notes: Tables present difference-in-difference estimates of the effects of local dial adoption on the fraction of young, white, American-born women in successive cohorts who are (i) telephone operators, (ii) other occupations in telephone industry, and (iii) working. Sample restricted to cities in the given region. All regressions include city and year fixed effects, and log city size x year controls. *, **, *** represent significance at the 0.1, 0.05, and 0.01 levels, respectively. SEs clustered by city in parentheses.

Table E.4: Effects of dial on fraction of young, white, American-born women in select occupations

	Tel. oper.	Off. mach.	Typist/secr.	Office clerk	Sales clerk	Beautician	Restaurant
				Sample: Northeast census region			
			Conditional on working, fraction employed as or in				
Post-cutover	-0.009***	0.000	0.012**	-0.008*	0.000	0.001	0.003
	(0.002)	(0.001)	(0.005)	(0.004)	(0.004)	(0.001)	(0.003)
N	2962	2962	2962	2962	2962	2962	2962
R^2	0.62	0.53	0.85	0.80	0.78	0.74	0.77
Cities	741	741	741	741	741	741	741
Cut over	86	86	86	86	86	86	86
Y Mean	0.02	0.00	0.12	0.06	0.08	0.01	0.02
				Sample: Midwest census region			
			Conditional on working, fraction employed as or in				
Post-cutover	-0.018***	0.001	0.006	0.004	-0.002	0.001	0.009***
	(0.002)	(0.001)	(0.004)	(0.004)	(0.003)	(0.001)	(0.002)
N	4209	4209	4209	4209	4209	4209	4209
R^2	0.57	0.67	0.80	0.80	0.60	0.78	0.78
Cities	1053	1053	1053	1053	1053	1053	1053
Cut over	89	89	89	89	89	89	89
Y Mean	0.03	0.00	0.12	0.05	0.10	0.01	0.04
				Sample: South census region			
			Conditional on working, fraction employed as or in				
Post-cutover	-0.019***	0.001	0.003	0.005	0.011**	0.002	0.005
	(0.003)	(0.001)	(0.006)	(0.004)	(0.005)	(0.002)	(0.005)
N	3238	3238	3238	3238	3238	3238	3238
R^2	0.59	0.55	0.80	0.70	0.59	0.76	0.80
Cities	811	811	811	811	811	811	811
Cut over	60	60	60	60	60	60	60
Y Mean	0.04	0.00	0.13	0.04	0.11	0.01	0.05
				Sample: West census region			
			Conditional on working, fraction employed as or in				
Post-cutover	-0.025***	-0.001	0.012*	-0.009	-0.000	0.001	0.001
	(0.005)	(0.002)	(0.007)	(0.007)	(0.008)	(0.003)	(0.007)
N	1236	1236	1236	1236	1236	1236	1236
R^2	0.54	0.61	0.80	0.75	0.51	0.79	0.71
Cities	309	309	309	309	309	309	309
Cut over	26	26	26	26	26	26	26
Y Mean	0.04	0.00	0.15	0.04	0.11	0.02	0.07

Notes: Tables present difference-in-difference estimates of the effects of local dial adoption on the fraction of young, white, American-born women in successive cohorts who are in select occupations. Sample restricted to cities in the given region. The left-most column provides the effect of cutovers on the fraction of these women who were telephone operators in the telephone industry, as a reference point. The other occupations across columns are: (i) office machine operators, (ii) typists, stenographers, and secretaries, (iii) other office clerks, (iv) sales clerks, (v) beauty parlor workers, and (vi) restaurant workers. All regressions include city and year fixed effects, and log city size x year controls. *, **, *** represent significance at the 0.1, 0.05, and 0.01 levels, respectively. SEs clustered by city in parentheses.

Table E.5: Effects of dial on fraction of young, white, American-born women who are telephone operators, in telephone industry, or employed at all

| | | Fraction who are tel. operators | | Fraction non-operators | Fraction with |
	All	In tel. industry	In oth. industry	In tel. industry	occupation
			Sample: Cities ≤100k population in 1920		
Post-cutover	-0.017***	-0.016***	-0.001***	-0.002***	0.000
	(0.001)	(0.001)	(0.000)	(0.001)	(0.006)
N	11645	11645	11645	11645	11656
R^2	0.60	0.60	0.44	0.47	0.83
Cities	2914	2914	2914	2914	2914
Cut over	261	261	261	261	261
Y Mean	0.03	0.03	0.00	0.00	0.40

| | | Fraction who are tel. operators | | Fraction non-operators | Fraction with |
	All	In tel. industry	In oth. industry	In tel. industry	occupation
			Sample: Cities ≤50k population in 1920		
Post-cutover	-0.015***	-0.014***	-0.001***	-0.002**	0.010*
	(0.001)	(0.001)	(0.000)	(0.001)	(0.005)
N	11357	11357	11357	11357	11368
R^2	0.57	0.58	0.42	0.44	0.81
Cities	2842	2842	2842	2842	2842
Cut over	212	212	212	212	212
Y Mean	0.03	0.03	0.00	0.00	0.39

| | | Fraction who are tel. operators | | Fraction non-operators | Fraction with |
	All	In tel. industry	In oth. industry	In tel. industry	occupation
			Sample: Cities ≤20k population in 1920		
Post-cutover	-0.012***	-0.011***	-0.001***	-0.001*	0.003
	(0.002)	(0.002)	(0.000)	(0.001)	(0.007)
N	10563	10563	10563	10563	10572
R^2	0.55	0.55	0.37	0.38	0.78
Cities	2643	2643	2643	2643	2643
Cut over	154	154	154	154	154
Y Mean	0.03	0.03	0.00	0.00	0.37

| | | Fraction who are tel. operators | | Fraction non-operators | Fraction with |
	All	In tel. industry	In oth. industry	In tel. industry	occupation
			Sample: Cities ≤10k population in 1920		
Post-cutover	-0.008***	-0.007***	-0.001***	-0.000	-0.005
	(0.002)	(0.002)	(0.000)	(0.001)	(0.010)
N	9233	9233	9233	9233	9240
R^2	0.51	0.52	0.34	0.36	0.74
Cities	2310	2310	2310	2310	2310
Cut over	110	110	110	110	110
Y Mean	0.04	0.03	0.00	0.00	0.36

Notes: Tables present difference-in-difference estimates of the effects of local dial adoption on the fraction of young, white, American-born women in successive cohorts who are (i) telephone operators, (ii) other occupations in telephone industry, and (iii) working. Sample restricted to cities of the given size. All regressions include city and year fixed effects, and log city size x year controls. *, **, *** represent significance at the 0.1, 0.05, and 0.01 levels, respectively. SEs clustered by city in parentheses.

Table E.6: Effects of dial on fraction of young, white, American-born women in select occupations

	Tel. oper.	Off. mach.	Typist/secr.	Office clerk	Sales clerk	Beautician	Restaurant
Sample: Cities ≤100k population in 1920							
Conditional on working, fraction employed as or in							
Post-cutover	-0.016***	0.000	0.007**	-0.003	0.001	0.001*	0.007***
	(0.001)	(0.000)	(0.003)	(0.002)	(0.002)	(0.001)	(0.002)
N	11645	11645	11645	11645	11645	11645	11645
R^2	0.60	0.59	0.81	0.78	0.66	0.75	0.77
Cities	2914	2914	2914	2914	2914	2914	2914
Cut over	261	261	261	261	261	261	261
Y Mean	0.03	0.00	0.13	0.05	0.10	0.01	0.04
Sample: Cities ≤50k population in 1920							
Conditional on working, fraction employed as or in							
Post-cutover	-0.014***	0.001	0.006*	0.003	-0.001	-0.000	0.005*
	(0.001)	(0.000)	(0.003)	(0.002)	(0.003)	(0.001)	(0.002)
N	11357	11357	11357	11357	11357	11357	11357
R^2	0.58	0.55	0.80	0.77	0.64	0.74	0.75
Cities	2842	2842	2842	2842	2842	2842	2842
Cut over	212	212	212	212	212	212	212
Y Mean	0.03	0.00	0.12	0.04	0.10	0.01	0.04
Sample: Cities ≤20k population in 1920							
Conditional on working, fraction employed as or in							
Post-cutover	-0.011***	0.000	0.003	-0.002	-0.004	-0.000	0.001
	(0.002)	(0.001)	(0.005)	(0.003)	(0.003)	(0.001)	(0.003)
N	10563	10563	10563	10563	10563	10563	10563
R^2	0.55	0.51	0.76	0.72	0.61	0.72	0.73
Cities	2643	2643	2643	2643	2643	2643	2643
Cut over	154	154	154	154	154	154	154
Y Mean	0.03	0.00	0.11	0.04	0.10	0.01	0.04
Sample: Cities ≤10k population in 1920							
Conditional on working, fraction employed as or in							
Post-cutover	-0.007***	0.001	0.005	-0.003	0.000	-0.000	-0.001
	(0.002)	(0.001)	(0.005)	(0.003)	(0.004)	(0.002)	(0.004)
N	9233	9233	9233	9233	9233	9233	9233
R^2	0.52	0.51	0.74	0.69	0.58	0.70	0.71
Cities	2310	2310	2310	2310	2310	2310	2310
Cut over	110	110	110	110	110	110	110
Y Mean	0.03	0.00	0.11	0.04	0.10	0.01	0.04

Notes: Tables present difference-in-difference estimates of the effects of local dial adoption on the fraction of young, white, American-born women in successive cohorts who are in select occupations. Sample restricted to cities of the given size. The left-most column provides the effect of cutovers on the fraction of these women who were telephone operators in the telephone industry, as a reference point. The other occupations across columns are: (i) office machine operators, (ii) typists, stenographers, and secretaries, (iii) other office clerks, (iv) sales clerks, (v) beauty parlor workers, and (vi) restaurant workers. All regressions include city and year fixed effects, and log city size x year controls. *, **, *** represent significance at the 0.1, 0.05, and 0.01 levels, respectively. SEs clustered by city in parentheses.

Heterogeneity by exposure

Table E.7: Effects of dial on fraction of young, white, American-born women who are telephone operators, in telephone industry, or employed at all

		Sample: Cities in top 25% of exposure			
		Fraction who are tel. operators		Fraction non-operators	Fraction with
	All	In tel. industry	In oth. industry	In tel. industry	occupation
Post-cutover	-0.017***	-0.016**	-0.001*	-0.002	-0.011
	(0.006)	(0.006)	(0.001)	(0.001)	(0.015)
N	2941	2941	2941	2941	2944
R^2	0.59	0.61	0.38	0.36	0.70
Cities	736	736	736	736	736
Cut over	33	33	33	33	33
Y Mean	0.05	0.05	0.00	0.00	0.33

		Sample: Cities in bottom 25% of exposure			
		Fraction who are tel. operators		Fraction non-operators	Fraction with
	All	In tel. industry	In oth. industry	In tel. industry	occupation
Post-cutover	-0.010***	-0.009***	-0.001***	-0.001	0.014
	(0.002)	(0.002)	(0.000)	(0.001)	(0.012)
N	2931	2931	2931	2931	2932
R^2	0.59	0.58	0.38	0.43	0.86
Cities	733	733	733	733	733
Cut over	58	58	58	58	58
Y Mean	0.02	0.02	0.00	0.00	0.45

Notes: Tables present difference-in-difference estimates of the effects of local dial adoption on the fraction of young, white, American-born women in successive cohorts who are (i) telephone operators, (ii) other occupations in telephone industry, and (iii) working. Sample restricted to cities by quartile of "exposure" to automation, which we define as a city's fraction of young, white, American-born working women in 1910 who were operators. All regressions include city and year fixed effects, and log city size x year controls. *, **, *** represent significance at the 0.1, 0.05, and 0.01 levels, respectively. SEs clustered by city in parentheses.

Table E.8: Effects of dial on fraction of young, white, American-born women in select occupations

	Sample: Cities in top 25% of exposure						
	Conditional on working, fraction employed as or in						
	Tel. oper.	Off. mach.	Typist/secr.	Office clerk	Sales clerk	Beautician	Restaurant
Post-cutover	-0.016**	-0.001	0.015*	-0.004	0.012	0.003	0.003
	(0.006)	(0.001)	(0.008)	(0.005)	(0.008)	(0.003)	(0.009)
N	2941	2941	2941	2941	2941	2941	2941
R^2	0.61	0.51	0.71	0.68	0.53	0.73	0.72
Cities	736	736	736	736	736	736	736
Cut over	33	33	33	33	33	33	33
Y Mean	0.05	0.00	0.12	0.04	0.11	0.02	0.05

	Sample: Cities in bottom 25% of exposure						
	Conditional on working, fraction employed as or in						
	Tel. oper.	Off. mach.	Typist/secr.	Office clerk	Sales clerk	Beautician	Restaurant
Post-cutover	-0.009***	0.000	0.006	-0.001	-0.005	0.001	0.003
	(0.002)	(0.001)	(0.006)	(0.005)	(0.005)	(0.001)	(0.003)
N	2931	2931	2931	2931	2931	2931	2931
R^2	0.58	0.52	0.81	0.79	0.68	0.73	0.75
Cities	733	733	733	733	733	733	733
Cut over	58	58	58	58	58	58	58
Y Mean	0.02	0.00	0.10	0.05	0.08	0.01	0.03

Notes: Tables present difference-in-difference estimates of the effects of local dial adoption on the fraction of young, white, American-born women in successive cohorts who are in select occupations. Sample restricted to cities by quartile of "exposure" to automation, which we define as a city's fraction of young, white, American-born working women in 1910 who were operators. The left-most column provides the effect of cutovers on the fraction of these women who were telephone operators in the telephone industry, as a reference point. The other occupations across columns are: (i) office machine operators, (ii) typists, stenographers, and secretaries, (iii) other office clerks, (iv) sales clerks, (v) beauty parlor workers, and (vi) restaurant workers. All regressions include city and year fixed effects, and log city size x year controls. *, **, *** represent significance at the 0.1, 0.05, and 0.01 levels, respectively. SEs clustered by city in parentheses.

E.4 Long-differences strategy with "Large City" sample

In the body of the paper, we focus our attention on "small" cities (those with population ≤100,000 in 1920), where cutovers were typically one-time events. As a robustness check, we also examine the 160 "large" cities in the AT&T sample which merge to our focal city sample (=164 U.S. cities in AT&T data, less four New York City boroughs)—where automation typically took place gradually, as individual exchanges were converted to mechanical operation one or two at a time. Although this sample is not suitable for event studies, we can use AT&T's data on the *intensity* of local dial adoption to estimate its effects on young women, and the results are statistically and quantitatively similar to those for smaller cities in the body of the paper.

Specifically, we relate the AT&T data on the fraction of a city's subscribers with dial service as of 1940 to 1920-to-1940 long-differenced outcomes, as follows:

$$\Delta_{1920,1940} Y_i = \beta \cdot \text{Fraction dial in } 1940_i + X_i \phi + \varepsilon_i$$

where i indexes cities. Only one city in the AT&T data had its first cutover before the 1920 census (Norfolk, VA), which we therefore omit from the estimation.

Results from these regressions are presented below, in a format analogous to the by-age difference-in-difference results in the paper. Figures E.5 and E.6 estimate the effects on the fraction of young, white, American-born women who are telephone operators in the telephone industry, have other jobs in the telephone industry, or are telephone operators in other industries (for comparison to Figures 4 and 5 in the body of the paper). Tables E.9 and E.10 estimate effects on the fraction of women who are working, in school, married, have children, and are in assorted occupations (for comparison to Tables 4 and 5 in the body of the paper).
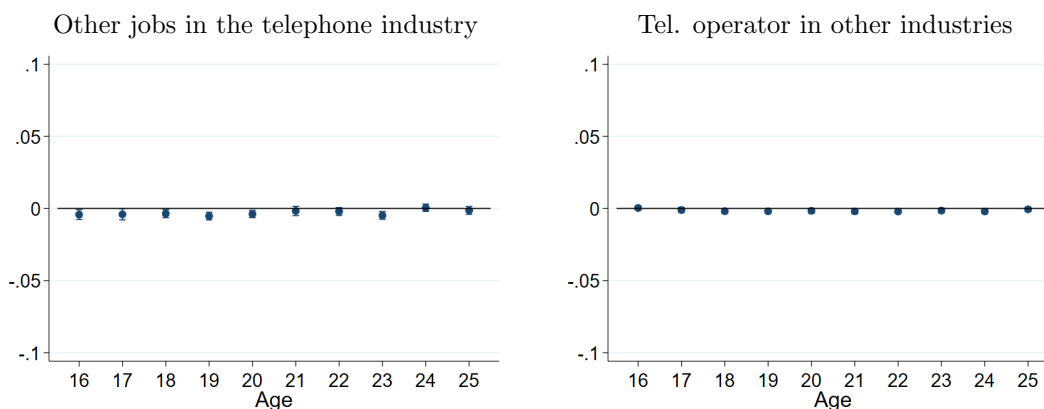
Effects on Operator Employment

Figure E.5: Effect of dial cutovers on the fraction of working young, white, American-born women in a given city who are telephone operators in the telephone industry (long differences, by age)

Fraction who are tel. operators (LD by age)
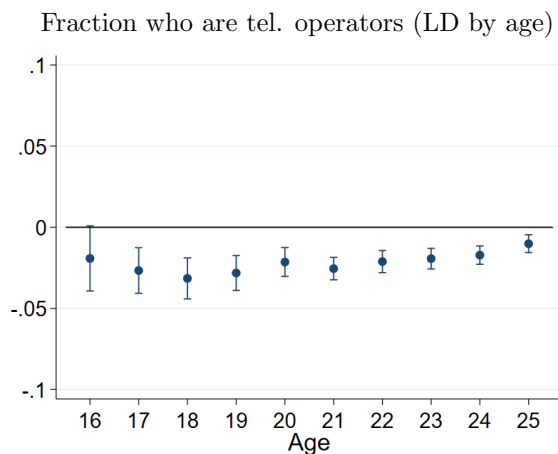
Notes: Figure shows 1920 to 1940 long difference estimates, by age, of the effects of local dial adoption (measured as the fraction of a city on dial as of the 1940 Census [April 1940]) on the fraction of working young, white, American-born women who are telephone operators in the telephone industry. Sample restricted to cities in the AT&T data whose first cutover occurred after the 1920 Census (January 1920). Error bars represent 95% confidence intervals, computed from robust SEs.

Figure E.6: Effect of dial cutovers on the fraction of working young, white, American-born women with other jobs in the telephone industry or who are telephone operators in other industries

Other jobs in the telephone industry   Tel. operator in other industries

Notes: Figure shows 1920 to 1940 long differences estimates, by age, of the effects of local dial adoption (measured as the fraction of a city on dial as of the 1940 Census [April 1940]) on the fraction of working young, white, American-born women in successive cohorts who have other jobs in the telephone industry (left) and who are telephone operators in other industries (right). Sample restricted to cities in the AT&T data whose first cutover occurred after the 1920 Census (January 1920). We plot the estimates on the same scale (-0.1 to 0.1) as the previous figures to ease comparison. Error bars represent 95% confidence intervals, computed from robust SEs.

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Effects on the Labor Market for Young Women

Table E.9: Changes in work, education, marriage, and fertility patterns around cutovers

Panel A: White, American-born women ages 16 to 18

| | | Fraction of the group that is: | | | |
	Tel. oper.	Working	In school	Married	Has children
Fraction dial in 1940	-0.007***	0.035	-0.050**	-0.008	-0.000
	(0.003)	(0.026)	(0.019)	(0.006)	(0.002)
N	159	159	159	159	159
R^2	0.10	0.21	0.18	0.01	0.02
Y Mean	-0.03	-0.34	0.27	-0.02	-0.00

Panel B: White, American-born women ages 19 to 22

| | | Fraction of the group that is: | | | |
	Tel. oper.	Working	In school	Married	Has children
Fraction dial in 1940	-0.011***	-0.021	-0.008	-0.021	-0.002
	(0.002)	(0.016)	(0.006)	(0.016)	(0.009)
N	159	159	159	159	159
R^2	0.24	0.01	0.01	0.02	0.00
Y Mean	-0.02	-0.02	0.04	-0.02	-0.02

Panel C: White, American-born women ages 23 to 25

| | | Fraction of the group that is: | | | |
	Tel. oper.	Working	In school	Married	Has children
Fraction dial in 1940	-0.007***	-0.016	-0.002	-0.020	0.004
	(0.001)	(0.015)	(0.001)	(0.013)	(0.011)
N	159	159	159	159	159
R^2	0.25	0.02	0.03	0.04	0.01
Y Mean	-0.01	0.06	0.01	0.03	-0.01

Notes: Tables present long differences estimates, by age, of the effects of local dial adoption (measured as the fraction of a city on dial as of the 1940 Census [April 1940]) on the fraction of young, white, American-born women in successive cohorts who are in the labor force, in school, married, and have children. Sample restricted to cities in the AT&T data whose first cutover occurred after the 1920 Census (January 1920). The left-most column provides the effect of cutovers on the fraction of these women who were telephone operators in the telephone industry, as a reference point. All regressions include log city size controls. *, **, *** represent significance at the 0.1, 0.05, and 0.01 levels, respectively. Robust SEs in parentheses.

Table E.10: Changes in employment shares in select occupations around cutovers

Panel A: White, American-born women ages 16 to 18

Conditional on working, fraction employed as or in

	Tel. oper.	Off. mach.	Typist/secr.	Office clerk	Sales clerk	Beautician	Restaurant
Fraction dial in 1940	-0.030***	0.001	0.033**	0.004	-0.004	0.002	0.022**
	(0.007)	(0.001)	(0.014)	(0.007)	(0.010)	(0.001)	(0.009)
N	159	159	159	159	159	159	159
R^2	0.10	0.03	0.13	0.05	0.20	0.13	0.06
Y Mean	-0.05	0.00	-0.02	0.02	0.01	0.01	0.06

Panel B: White, American-born women ages 19 to 22

Conditional on working, fraction employed as or in

	Tel. oper.	Off. mach.	Typist/secr.	Office clerk	Sales clerk	Beautician	Restaurant
Fraction dial in 1940	-0.024***	-0.000	0.016	-0.004	0.006	0.003	0.020***
	(0.004)	(0.001)	(0.013)	(0.008)	(0.007)	(0.002)	(0.006)
N	159	159	159	159	159	159	159
R^2	0.22	0.05	0.02	0.00	0.13	0.04	0.11
Y Mean	-0.03	0.01	-0.01	0.02	0.02	0.02	0.05

Panel C: White, American-born women ages 23 to 25

Conditional on working, fraction employed as or in

	Tel. oper.	Off. mach.	Typist/secr.	Office clerk	Sales clerk	Beautician	Restaurant
Fraction dial in 1940	-0.016***	0.000	0.005	-0.003	0.002	0.004**	0.014***
	(0.002)	(0.002)	(0.012)	(0.009)	(0.005)	(0.002)	(0.005)
N	159	159	159	159	159	159	159
R^2	0.29	0.08	0.01	0.00	0.02	0.04	0.07
Y Mean	-0.02	0.01	0.02	0.02	0.01	0.02	0.05

Notes: Tables present long differences estimates, by age, of the effects of local dial adoption (measured as the fraction of a city on dial as of the 1940 Census [April 1940]) on young, white, American-born women's employment shares in select occupations. Sample restricted to cities in the AT&T data whose first cutover occurred after the 1920 Census (January 1920). The left-most column provides the effect of cutovers on the fraction of these women who were telephone operators in the telephone industry, as a reference point. The other occupations across columns are: (i) office machine operators, (ii) typists, stenographers, and secretaries, (iii) other office clerks, (iv) sales clerks, (v) beauty parlor workers, and (vi) restaurant workers. All regressions include log city size controls. *, **, *** represent significance at the 0.1, 0.05, and 0.01 levels, respectively. Robust SEs in parentheses.

E.5 Instrumental variables strategy

Throughout the paper, we estimate the effects of cutovers on labor market outcomes directly in the reduced form. The underlying logic is that cutovers reshuffled the broader labor market for young women by nearly eliminating one of its largest occupations. In this section, we operationalize this logic with a 2SLS design, where the first stage uses cutovers to instrument for the share of working young women who were telephone operators in the telephone industry, and the second stage then uses this to explain other labor market outcomes. Concretely, we estimate:

$$W_{it} = \beta \cdot \mathbb{1}(\text{Post-Cutover})_{it} + \alpha_i + \delta_t + X_{it}\phi + \varepsilon_{it}$$

$$Y_{it} = \beta \cdot \hat{W}_{it} + \alpha_i + \delta_t + X_{it}\phi + \varepsilon_{it}$$

We restrict our focus to young, white, American-born women. As before, it indexes city-years, α_i and δ_t are city/year fixed effects, and X_{it} are time-varying controls. W_{it} and Y_{it} represent our first- and second-stage outcomes, respectively. To improve readability, we rescale W_{it} to make each unit represent a 10 p.p. decline in the fraction of working young women who were telephone operators. Table E.11 uses this specification to estimate the effect of a decline in telephone operators jobs on the fraction of young women in subsequent cohorts who were (i) working, and (ii) conditional on working, the fraction working in focal occupations. The predictive power of cutovers for telephone operator employment rates is unsurprisingly very large (with a partial F-statistic of nearly 200), and the results are statistically and quantitatively similar to those in the paper. We see no relationship between the decline of telephone operator jobs and overall employment rates, but we see countervailing employment growth in secretarial work, restaurant work, and perhaps in beauty work. The magnitudes are also consistent with those in the paper: with cutovers on average eliminating around 2% of jobs for this demographic, these estimates can be multiplied by one-fifth to get roughly the same magnitudes we find in our main results.

Table E.11: Effects of decline of telephone operation on the employment and occupations of future cohorts of young, white, American-born women: 2SLS design

| | Pct. working | Conditional on working, fraction employed as or in | | | | | |
		Off. mach.	Typist/secr.	Office clerk	Sales clerk	Beautician	Restaurant
Pct. tel. oper./tel. ind. ↓10%	0.003	0.002	0.044***	-0.018	0.009	0.008**	0.043***
	(0.031)	(0.002)	(0.016)	(0.013)	(0.013)	(0.003)	(0.011)
N	11645	11645	11645	11645	11645	11645	11645
F-stat	198.58	198.58	198.58	198.58	198.58	198.58	198.58
Cities	2914	2914	2914	2914	2914	2914	2914
Cut over	261	261	261	261	261	261	261
Y Mean	0.40	0.00	0.13	0.05	0.10	0.01	0.04

Notes: Tables present estimates of the effects of a 10% decline in telephone operator employment on the fraction of young, white, American-born women in successive cohorts who are working, and of those working, the fraction in select occupations. Observations are at the city x year level. The RHS variable measures the fraction of this demographic working as telephone operators in the telephone industry, and we instrument for this variable with an indicator for whether the given city has experienced a dial cutover. All estimates are scaled to correspond to a 10 p.p. decline in the fraction of the population working as operators. All regressions include city and year fixed effects, and log city size x year controls. *, **, *** represent significance at the 0.1, 0.05, and 0.01 levels, respectively. SEs clustered by city in parentheses.

Tables E.12 and E.13 estimate the 2SLS versions of Tables 4 and 5 from the body of the paper. The patterns, including the magnitudes (once appropriately adjusted, i.e. multiplied by one-fifth, as above), are again similar to those which we find in the reduced form.

Table E.12: Changes in work, education, marriage, and fertility patterns around cutovers

Panel A: White, American-born women ages 16 to 18

| | Fraction of the group that is: | | | |
	Working	In school	Married	Has children
Pct. tel. op. tel. ind. ↓10%	-0.004	0.005	-0.008	0.003
	(0.035)	(0.034)	(0.009)	(0.004)
N	11613	11613	11613	11613
F-stat	71.41	71.41	71.41	71.41
Cities	2913	2913	2913	2913
Cut over	261	261	261	261
Y Mean	0.30	0.54	0.09	0.03

Panel B: White, American-born women ages 19 to 22

| | Fraction of the group that is: | | | |
	Working	In school	Married	Has children
Pct. tel. op. tel. ind. ↓10%	-0.014	-0.004	0.016	0.022
	(0.031)	(0.017)	(0.023)	(0.015)
N	11642	11642	11642	11642
F-stat	157.59	157.59	157.59	157.59
Cities	2914	2914	2914	2914
Cut over	261	261	261	261
Y Mean	0.48	0.11	0.36	0.19

Panel C: White, American-born women ages 23 to 25

| | Fraction of the group that is: | | | |
	Working	In school	Married	Has children
Pct. tel. op. tel. ind. ↓10%	-0.006	-0.007	0.023	0.022
	(0.055)	(0.011)	(0.039)	(0.036)
N	11633	11633	11633	11633
F-stat	110.55	110.55	110.55	110.55
Cities	2914	2914	2914	2914
Cut over	261	261	261	261
Y Mean	0.41	0.02	0.60	0.38

Notes: Tables present difference-in-difference estimates, by age, of the effects of local dial adoption on the fraction of young, white, American-born women in successive cohorts who are in the labor force, in school, married, and have children, for cities with population ≤100k in 1920. The RHS variable measures the fraction of this demographic working as telephone operators in the telephone industry, and we instrument for this variable with an indicator for whether the given city has experienced a dial cutover. All estimates are scaled to correspond to a 10 p.p. decline in the fraction of the population working as operators. All regressions include city and year fixed effects, and log city size x year controls. *, **, *** represent significance at the 0.1, 0.05, and 0.01 levels, respectively. SEs clustered by city in parentheses.

Table E.13: Changes in employment shares in select occupations around cutovers

Panel A: White, American-born women ages 16 to 18

| | Conditional on working, fraction employed as or in | | | | | |
	Off. mach.	Typist/secr.	Office clerk	Sales clerk	Beautician	Restaurant
Pct. tel. op. tel. ind. ↓10%	0.002	0.033**	-0.006	0.008	0.010***	0.066***
	(0.001)	(0.016)	(0.011)	(0.017)	(0.003)	(0.013)
N	11613	11613	11613	11613	11613	11613
F-stat	71.41	71.41	71.41	71.41	71.41	71.41
Cities	2913	2913	2913	2913	2913	2913
Cut over	261	261	261	261	261	261
Y Mean	0.00	0.08	0.04	0.11	0.01	0.05

Panel B: White, American-born women ages 19 to 22

| | Conditional on working, fraction employed as or in | | | | | |
	Off. mach.	Typist/secr.	Office clerk	Sales clerk	Beautician	Restaurant
Pct. tel. op. tel. ind. ↓10%	0.002	0.053***	-0.016	0.017	0.004	0.038***
	(0.002)	(0.018)	(0.015)	(0.014)	(0.004)	(0.011)
N	11642	11642	11642	11642	11642	11642
F-stat	157.59	157.59	157.59	157.59	157.59	157.59
Cities	2914	2914	2914	2914	2914	2914
Cut over	261	261	261	261	261	261
Y Mean	0.00	0.14	0.05	0.10	0.01	0.04

Panel C: White, American-born women ages 23 to 25

| | Conditional on working, fraction employed as or in | | | | | |
	Off. mach.	Typist/secr.	Office clerk	Sales clerk	Beautician	Restaurant
Pct. tel. op. tel. ind. ↓10%	0.004	0.026	-0.040*	0.002	0.017*	0.045**
	(0.004)	(0.032)	(0.023)	(0.022)	(0.009)	(0.019)
N	11633	11633	11633	11633	11633	11633
F-stat	110.55	110.55	110.55	110.55	110.55	110.55
Cities	2914	2914	2914	2914	2914	2914
Cut over	261	261	261	261	261	261
Y Mean	0.00	0.13	0.05	0.09	0.01	0.04

Notes: Tables present difference-in-difference estimates, by age, of the effects of local dial adoption on young, white, American-born women's employment shares in select occupations, across successive cohorts, for cities with population ≤100k in 1920. The RHS variable measures the fraction of this demographic working as telephone operators in the telephone industry, and we instrument for this variable with an indicator for whether the given city has experienced a dial cutover. All estimates are scaled to correspond to a 10 p.p. decline in the fraction of the population working as operators. All regressions include city and year fixed effects, and log city size x year controls. *, **, *** represent significance at the 0.1, 0.05, and 0.01 levels, respectively. SEs clustered by city in parentheses.

F Supplementary Results

F.1 Structural changes taking place in treated cities around cutovers

Recall that the period we study was one of significant changes in young women's education, labor force participation, and occupational composition. These trends were thus taking place around the time that telephone service was mechanized, both in the aggregate and at the level of individual cities. In the paper we include city size x year controls to account for differential trends in larger and smaller cities which may have incidentally coincided with cutovers (recall that automation on average proceeded from larger to smaller cities). As an empirical matter, these controls eliminate differential pre-trends in the outcomes we study, but they also wash out variation that can provide a deeper view of the underlying structural change. In this appendix section, we present results with year fixed effects only, where underlying trends are more visible.

Table F.1 provides the counterpart to Table 4 in the body of the paper, estimating changes in the fraction of young, white, American-born women working, in school, married, or with children around the time of a city's first cutover. The most important trend taking place at this time was increasing high school completion, with a rapidly growing fraction of 16, 17, and 18 year-olds staying in school rather than entering the labor force—and the magnitudes of these differences are substantially larger than what could be attributed to cutovers alone.

Table F.2 then provides a counterpart to Table 5, estimating changes in employment of successive cohorts in our focal occupations. Though not easily discerned given the scaling of the figure (which is fixed to be consistent across all charts), there is an underlying trend of growing employment of office machine operators—a rapidly-growing office job—as well as growth in waitressing, consistent with the evidence of Cobble (1992), who documents the feminization of the restaurant industry in the first half of the 20th century, observing that as the restaurant industry grew (more than tripled between 1900 and 1930, with over a million people working at restaurants by 1940), "waiting jobs opened up to women"— especially "American-born, white, lower-middle-class women," the same demographic from which AT&T hired its telephone operators.

Depending on how the question is framed, these underlying trends can also be viewed as part of the story of this paper, and any additional controls unnecessary—and perhaps even a detriment, insofar as they obscure these trends. In studying how future cohorts of young women adjusted to automation, we do not necessarily need to know whether growth in employment in other occupations was causal: we are primarily interested in understanding whether and where young women found work after telephone operation was automated, which is ultimately a descriptive question. In the body of the paper, we nevertheless control for these underlying trends so as to isolate changes in employment that are more closely related to cutovers themselves.

Table F.1: Changes in work, education, marriage, and fertility patterns around cutovers

Panel A: White, American-born women ages 16 to 18

| | Fraction of the group that is: | | | | |
	Tel. oper.	Working	In school	Married	Has children
Post-cutover	-0.006***	-0.043***	0.029***	-0.003	-0.000
	(0.001)	(0.009)	(0.008)	(0.002)	(0.001)
N	11652	11652	11652	11652	11652
R^2	0.54	0.80	0.74	0.79	0.68
Cities	2913	2913	2913	2913	2913
Cut over	261	261	261	261	261
Y Mean	0.01	0.30	0.53	0.09	0.03

Panel B: White, American-born women ages 19 to 22

| | Fraction of the group that is: | | | | |
	Tel. oper.	Working	In school	Married	Has children
Post-cutover	-0.006***	-0.004	0.005**	-0.003	0.002
	(0.001)	(0.006)	(0.003)	(0.005)	(0.003)
N	11656	11656	11656	11656	11656
R^2	0.50	0.81	0.79	0.83	0.79
Cities	2914	2914	2914	2914	2914
Cut over	261	261	261	261	261
Y Mean	0.01	0.48	0.11	0.36	0.19

Panel C: White, American-born women ages 23 to 25

| | Fraction of the group that is: | | | | |
	Tel. oper.	Working	In school	Married	Has children
Post-cutover	-0.003***	0.002	0.001	0.002	0.004
	(0.000)	(0.005)	(0.001)	(0.005)	(0.004)
N	11656	11656	11656	11656	11656
R^2	0.42	0.77	0.66	0.79	0.77
Cities	2914	2914	2914	2914	2914
Cut over	261	261	261	261	261
Y Mean	0.01	0.41	0.02	0.60	0.38

Notes: Tables present difference-in-difference estimates, by age, of the effects of local dial adoption on the fraction of young, white, American-born women in successive cohorts who are in the labor force, in school, married, and have children, for cities with population ≤100k in 1920. The left-most column provides the effect of cutovers on the fraction of these women who were telephone operators in the telephone industry, as a reference point. All regressions include city and year fixed effects only. *, **, *** represent significance at the 0.1, 0.05, and 0.01 levels, respectively. SEs clustered by city in parentheses.

Table F.2: Changes in employment shares in select occupations around cutovers

Panel A: White, American-born women ages 16 to 18

| | Conditional on working, fraction employed as or in | | | | | | |
	Tel. oper.	Off. mach.	Typist/secr.	Office clerk	Sales clerk	Beautician	Restaurant
Post-cutover	-0.013***	0.001***	-0.006	0.001	0.004	0.001	0.015***
	(0.003)	(0.000)	(0.004)	(0.003)	(0.004)	(0.001)	(0.004)
N	11613	11613	11613	11613	11613	11613	11613
R^2	0.45	0.36	0.57	0.62	0.50	0.39	0.56
Cities	2913	2913	2913	2913	2913	2913	2913
Cut over	261	261	261	261	261	261	261
Y Mean	0.03	0.00	0.08	0.04	0.11	0.01	0.05

Panel B: White, American-born women ages 19 to 22

| | Conditional on working, fraction employed as or in | | | | | | |
	Tel. oper.	Off. mach.	Typist/secr.	Office clerk	Sales clerk	Beautician	Restaurant
Post-cutover	-0.011***	0.002***	-0.002	0.002	0.005**	-0.002***	0.003
	(0.001)	(0.000)	(0.003)	(0.002)	(0.002)	(0.001)	(0.002)
N	11642	11642	11642	11642	11642	11642	11642
R^2	0.49	0.53	0.74	0.73	0.57	0.65	0.72
Cities	2914	2914	2914	2914	2914	2914	2914
Cut over	261	261	261	261	261	261	261
Y Mean	0.03	0.00	0.14	0.05	0.10	0.01	0.04

Panel C: White, American-born women ages 23 to 25

| | Conditional on working, fraction employed as or in | | | | | | |
	Tel. oper.	Off. mach.	Typist/secr.	Office clerk	Sales clerk	Beautician	Restaurant
Post-cutover	-0.006***	0.002***	0.003	0.003	0.006***	-0.002	0.004*
	(0.001)	(0.000)	(0.003)	(0.002)	(0.002)	(0.001)	(0.002)
N	11633	11633	11633	11633	11633	11633	11633
R^2	0.39	0.51	0.71	0.66	0.49	0.64	0.69
Cities	2914	2914	2914	2914	2914	2914	2914
Cut over	261	261	261	261	261	261	261
Y Mean	0.02	0.00	0.13	0.05	0.09	0.01	0.04

Notes: Tables present difference-in-difference estimates, by age, of the effects of local dial adoption on young, white, American-born women's employment shares in select occupations, across successive cohorts, for cities with population ≤100k in 1920. The left-most column provides the effect of cutovers on the fraction of these women who were telephone operators in the telephone industry, as a reference point. The other occupations across columns are: (i) office machine operators, (ii) typists, stenographers, and secretaries, (iii) other office clerks, (iv) sales clerks, (v) beauty parlor workers, and (vi) restaurant workers. All regressions include city and year fixed effects only. *, **, *** represent significance at the 0.1, 0.05, and 0.01 levels, respectively. SEs clustered by city in parentheses.

F.2 Potential mechanisms to explain adjustment

To better understand the mechanisms behind our results, we explore additional cuts of the data on differential Great Depression severity and by measures of local economic dynamism.
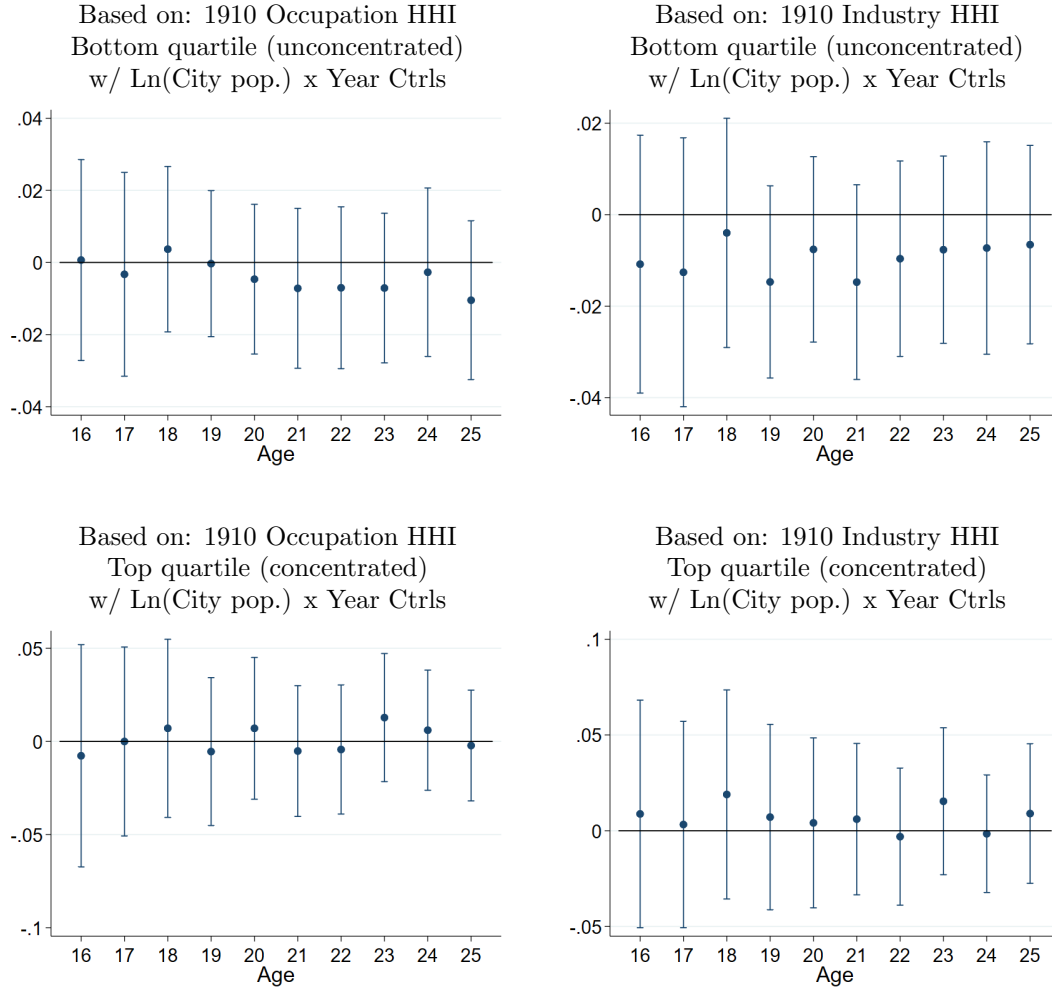
Local economic dynamism

Our second focus is economic dynamism. "Business dynamism" is typically defined in terms of firm entry and worker flows (e.g., the fraction of employment at given point in time from firms founded, or the fraction of workers who have changed jobs, in the last t years), motivated by the idea that a dynamic economy will generate a steady churn of workers and firms. Although no such data are available for the period we study, we can consider a number of alternatives.

Since our focus is cities, we begin with measures of local industrial diversity, inspired by the Jacobs (1970) thesis which has since been incorporated into the urban economics and economic growth literature (Lucas Jr. 1988, Glaeser et al. 1992). Specifically, we compute, for each city and year, a Herfindahl index of employment concentration across (i) occupations and (ii) industries (based on coded occupations/industries in the IPUMS data). We do so for all adult workers—although in unreported results we have also done so specifically for women and found similar patterns. We then bin cities into quartiles of the distribution of occupation or employment concentration in 1910 (the pre-period), and interpret unconcentrated and concentrated cities as more and less economically dynamic, respectively. Figure F.1 presents estimated effects of cutovers on the fraction of young, white, American-born women who are working, for cities below and above the median of pre-period employment concentration. We do not find any discernable difference between these categories of dynamism in the effect of cutovers on employment.
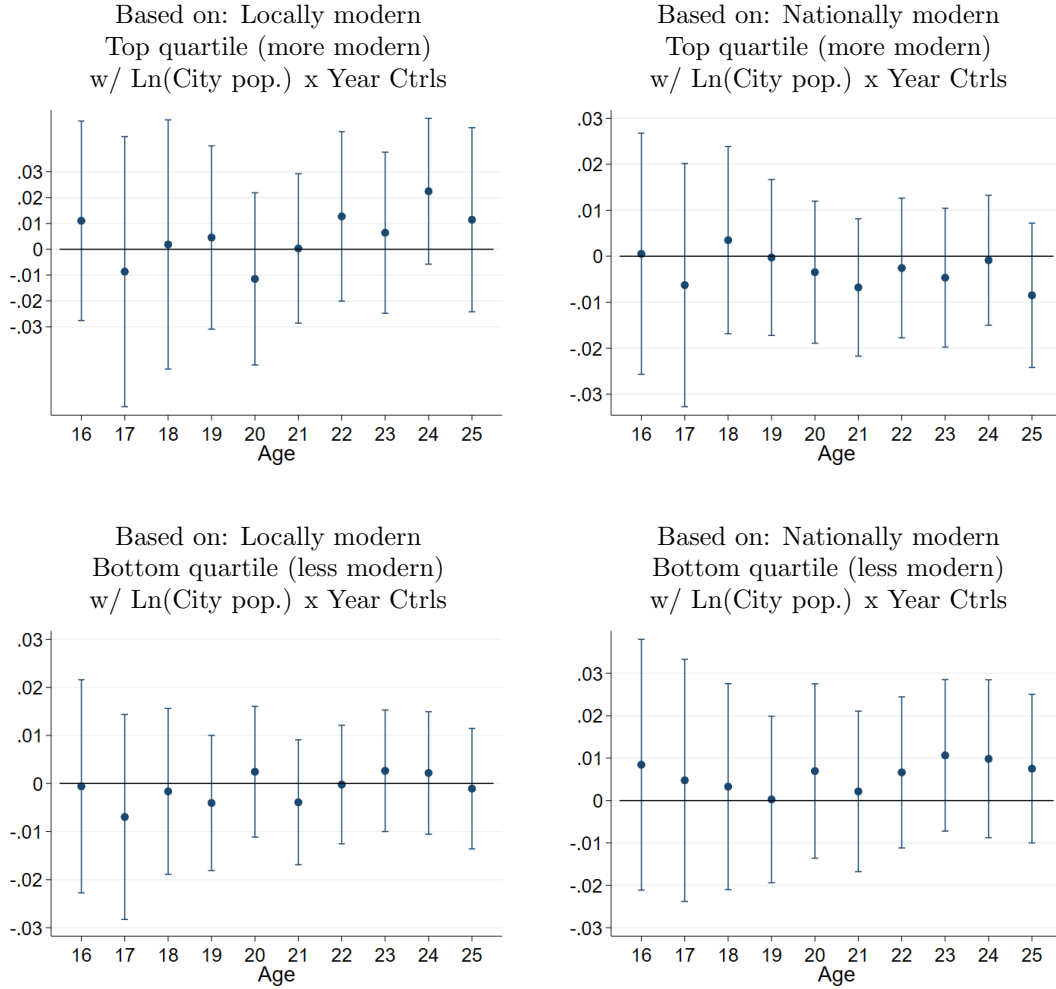
As a second measure, we calculate the fraction of employment in each city-year that is in a "new" (i.e., modern for its time) occupation, which we define as occupations which were in the bottom ten percent of all occupations in their share of 1910 employment. This measure can be defined nationally and locally, and we do so both ways. Examples of occupations which were rare in 1910 but employed a meaningful number of people in the 1920s and 1930s range from auto mechanics to movie projectionists. We bin city-years into quartiles of this measure and interpret them vis-à-vis "employment modernity". Figure F.2 presents the estimates for cities above and below the median of these cuts of the sample. We find no discernable differences.

Figure F.1: Changes in employment of young, white, American-born women around cutovers (diff-in-diff, by age, 16-25), by measures of local dynamism

Based on: 1910 Occupation HHI
Bottom quartile (unconcentrated)
w/ Ln(City pop.) x Year Ctrls

Based on: 1910 Industry HHI
Bottom quartile (unconcentrated)
w/ Ln(City pop.) x Year Ctrls

Based on: 1910 Occupation HHI
Top quartile (concentrated)
w/ Ln(City pop.) x Year Ctrls

Based on: 1910 Industry HHI
Top quartile (concentrated)
w/ Ln(City pop.) x Year Ctrls

Notes: Figure shows difference-in-differences estimates, by age, of the effects of local dial adoption on the fraction of young, white, American-born women in successive cohorts who are working, for cities with population ≤100k in 1920. Sample restricted to cities in the given quartile of the given dynamism measure (occupational or industrial employment concentration in 1910, in the left and right panels, respectively). Error bars represent 95% confidence intervals, computed from SEs clustered at the city level.

Figure F.2: Changes in employment of young, white, American-born women around cutovers (diff-in-diff, by age, 16-25), by measures of local dynamism

Based on: Locally modern
Top quartile (more modern)
w/ Ln(City pop.) x Year Ctrls

Based on: Nationally modern
Top quartile (more modern)
w/ Ln(City pop.) x Year Ctrls

Based on: Locally modern
Bottom quartile (less modern)
w/ Ln(City pop.) x Year Ctrls

Based on: Nationally modern
Bottom quartile (less modern)
w/ Ln(City pop.) x Year Ctrls

Notes: Figure shows difference-in-differences estimates, by age, of the effects of local dial adoption on the fraction of young, white, American-born women in successive cohorts who are working, for cities with population ≤100k in 1920. Sample restricted to cities in the given quartile of the given dynamism measure (fraction of local employment in occupations that are locally or nationally new (rare before 1910), in the left and right panels, respectively). Error bars represent 95% confidence intervals, computed from SEs clustered at the city level.

Labor market tightness, vis-à-vis Great Depression severity

We investigate heterogeneity across cities which were more or less severely exposed to the Great Depression in the 1930s. Although a natural first instinct might be to take advantage of the time dimension of our cutover panel variation, and explore heterogeneity across cities which were cut over to dial in 1929-1933 versus in other years, or in the 1920s versus in the 1930s, these comparisons present several challenges: (i) cutover timing correlates strongly with city size, potentially confounding the Great Depression interaction; (ii) the comparison abstracts away from considerable variation in depression severity across the country; (iii) even as is, our tests are underpowered (especially when we want to compare cities automated in the 1929-1933 period against others); and (iv) it is unclear whether to treat the 1930 census data as being pre- or mid-depression, which began in late 1929 but did not reach peak intensity until 1932-1933.

Instead of temporal variation, we exploit variation across space in Great Depression severity, using the Fishback et al. (2005) data on the county per-capita retail sales contractions from 1929 to 1933, assigning county-level values to each city in a given county (and averaging across counties where necessary). We partition our sampled cities into two groups—above- versus below-median depression severity—and estimate the effects of cutovers on young women's employment for each. These effects will thus represent average effects in cities with weaker versus stronger labor demand in the 1930s, independent of precisely when each city experienced its first cutover. The results are presented by age in Figure F.3 and pooled across ages in Table F.3.

This evidence suggests a potential interaction of automation with recessions: in counties with more severe contractions in retail spending, cutovers were followed by a detectable decline in employment of young, white, American-born women, particularly those above school age, while in less affected counties, cutovers instead coincided with net employment growth. Without more refined analysis relating cutovers to not only the location but also the timing of aggregate demand shocks (for which our data are not well suited), we cannot assert confidently that the effects of automation depend on aggregate demand. But this is a natural next question for research on the labor market effects of automation, which we would encourage others to pursue.

Figure F.3: Changes in employment of young, white, American-born women around cutovers (diff-in-diff, by age, 16-25), by Great Depression severity
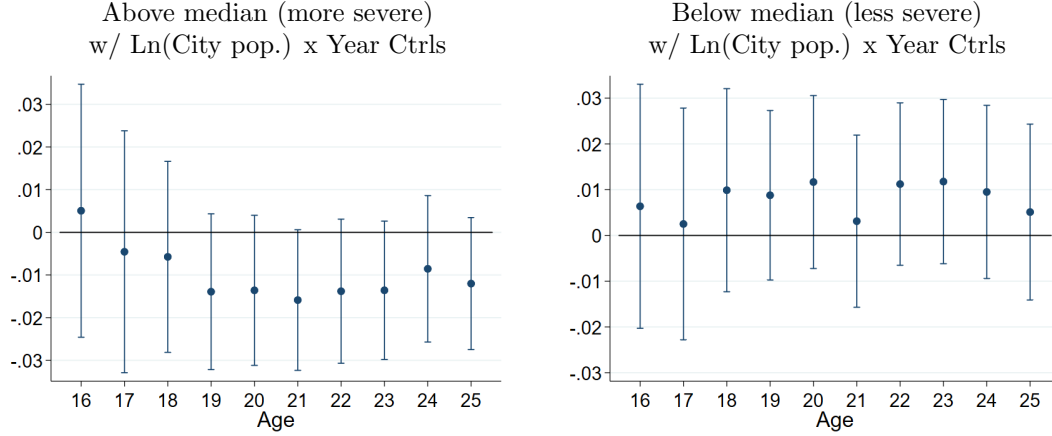
Notes: Figure shows difference-in-differences estimates, by age, of the effects of local dial adoption on the fraction of young, white, American-born women in successive cohorts who are working, for cities with population ≤100k in 1920. Great Depression (GD) severity is measured as the county change in retail sales per capita from 1929 to 1933 (peak to trough), using data from Fishback et al. (2005). Sample restricted to cities with above- versus below-median GD exposure. Error bars represent 95% confidence intervals, computed from SEs clustered at the city level.

Table F.3: Effects on employment, by age and local severity of Great Depression

| | 16-18 | | 19-22 | | 23-25 | |
	More	Less	More	Less	More	Less
Post-cutover	-0.001	0.007	-0.014***	0.009	-0.011**	0.009
	(0.009)	(0.009)	(0.005)	(0.005)	(0.005)	(0.006)
N	17640	17284	23512	23052	17636	17288
R^2	0.80	0.83	0.68	0.73	0.65	0.72
Cities	4410	4321	5878	5763	4409	4322
Cut over	332	450	442	600	332	449
Y Mean	0.27	0.32	0.45	0.50	0.38	0.43

Notes: Table presents results from a DID regression estimating the changes around dial cutovers in the fraction of young, white, American-born women in successive cohorts who are working. Great Depression (GD) severity is measured as the county change in retail sales per capita from 1929 to 1933 (peak to trough), using data from Fishback et al. (2005). The table splits cities in our sample into above- and below-median GD exposure. All regressions include city and year fixed effects, and log city size x year controls. *, **, *** represent significance at the 0.1, 0.05, and 0.01 levels, respectively. SEs clustered by city in parentheses.

F.3 General equilibrium effects

Although most of the paper focuses on the effects of cutovers on young, white, American-born women, it is also possible that the effects spilled over onto other populations, especially via labor market competition. In the following tables we explore changes in local employment of other demographic groups, specifically focusing on the two occupations which appear to have absorbed future cohorts of young, white, American-born women: (i) typists, stenographers, and secretaries (Table F.4), and (ii) waitressing (Table F.5). The table explores changes in the number of adults (of all ages) working in a given occupation around cutovers, and the fraction of employment each occupation represents for different populations (by sex, race, and birthplace). We do not generally find widespread evidence of spillovers, though Table F.5 includes modest evidence that the growth in the fraction of white, American-born women in restaurant work may have come at the expense of employment of black women (final column, significant at the 10% level).

Table F.4: Total employment + fraction of assorted subpopulations who are typists/stenographers

	Log # of all adults (16+)	Pct. of white, American Men	Women	Pct. of white, foreign Men	Women	Pct. of non-white, American Men	Women
Post-cutover	0.047*	-0.000	0.002	0.000	0.001	-0.000	0.001
	(0.027)	(0.000)	(0.002)	(0.000)	(0.002)	(0.000)	(0.001)
Constant	-5.307***	0.001	-0.098***	-0.003***	-0.129***	-0.000	0.015*
	(0.330)	(0.001)	(0.022)	(0.001)	(0.019)	(0.001)	(0.008)
N	11660	11650	11646	11514	10551	9233	8637
R^2	0.94	0.65	0.88	0.37	0.66	0.36	0.60
Cities	2915	2915	2914	2884	2805	2326	2299
Cut over	261	261	261	260	256	232	233

Notes: Table presents results from a DID regression estimating the changes around dial cutovers in the (log) number of adults in a city who are typists/stenographers/secretaries, and the fraction of different subpopulations who are the same (binning the population by race, place of birth, and sex). All regressions include city and year fixed effects, and log city size x year controls. *, **, *** represent significance at the 0.1, 0.05, and 0.01 levels, respectively. SEs clustered by city in parentheses.

Table F.5: Total employment + fraction of assorted subpopulations who are waiters/waitresses

	Log # of all adults (16+)	Pct. of white, American Men	Women	Pct. of white, foreign Men	Women	Pct. of non-white, American Men	Women
Post-cutover	0.010	-0.000	0.003**	-0.000	0.001	-0.002	-0.004*
	(0.038)	(0.000)	(0.001)	(0.000)	(0.001)	(0.004)	(0.002)
Constant	-5.039***	-0.001	-0.037***	-0.007	-0.014	-0.020	0.019
	(0.346)	(0.003)	(0.012)	(0.005)	(0.015)	(0.040)	(0.019)
N	11660	11650	11646	11514	10551	9233	8637
R^2	0.89	0.63	0.83	0.66	0.50	0.74	0.61
Cities	2915	2915	2914	2884	2805	2326	2299
Cut over	261	261	261	260	256	232	233

Notes: Table presents results from a DID regression estimating the changes around dial cutovers in the (log) number of adults in a city who are restaurant or food service workers, and the fraction of different subpopulations who are the same (binning the population by race, place of birth, and sex). All regressions include city and year fixed effects, and log city size x year controls. *, **, *** represent significance at the 0.1, 0.05, and 0.01 levels, respectively. SEs clustered by city in parentheses.