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ANSWERING THE CALL OF AUTOMATION: HOW THE LABOR MARKET ADJUSTED TO MECHANIZING TELEPHONE OPERATION

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ABSTRACT

In the early 1900s, telephone operation was among the most common jobs for American women, and telephone operators were ubiquitous. Between 1920 and 1940, AT&T undertook one of the largest automation investments in modern history, replacing operators with mechanical switching technology in over half of the U.S. telephone network. Using variation across U.S. cities in the timing of adoption, we study how this wave of automation affected the labor market for young women. Although automation eliminated most of these jobs, it did not reduce future cohorts' overall employment: the decline in operators was counteracted by employment growth in middle-skill clerical jobs and lower- skill service jobs, including in new categories of work. Using a new genealogy-based census-linking method, we show that incumbent telephone operators were most impacted, and a decade later more likely to be in lower-paying occupations or no longer working.

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Automation anxiety has recently surged in the U.S. and other developed economies (Autor 2015), fueled by warnings of a sweeping wave of automation (e.g., Brynjolfsson and McAfee 2014). Yet the degree to which automation reduces employment, and for whom, is increasingly seen as ambiguous: automation can in theory be offset by countervailing forces, from productivity growth to the emergence of new work in which labor has comparative advantage (Acemoglu and Restrepo 2018, 2019a,b). Basic questions include whether, where, and how quickly labor demand will recover from large automation events, and who, if anybody, suffers its consequences.

In this paper, we study the impacts of one of the largest automation shocks in history: the automation of telephone operation. In 1920, *telephone operator* was one of the most common jobs for American women, and operators were a staple of everyday life across the country. Between 1920 and 1940, telephone exchanges serving over half of the U.S. were mechanized, replacing most local operators, one city at a time. The fraction of female employment exposed to this shock is similar to the fraction of the current U.S. workforce employed as cashiers or customer service workers—jobs which are increasingly being automated today (BLS 2019a).

We document the effects of mechanizing telephone operation on both incumbent workers and future generations. To do so, we construct a dataset measuring the local adoption of mechanical call switching and combine it with census data on the complete U.S. population and a longitudinally-linked sample of women. Our exercise comprises two distinct but closely related analyses, on two samples, answering two 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? As a first step, we show 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 and permanently fell by 50 to 80%. These jobs comprised around 2% of employment for this group, and even more for those under age 20—and given turnover rates, this shock may have foreclosed entry-level job opportunities for as much as 10 to 15% of peak cohorts.

The effect of this shock on incumbent operators was to dispossess many of their jobs and careers: telephone operators in cities with cutovers were less likely to be in the same job the next decade we observe them, less likely to be working at all, and conditional on working were more likely to be in lower-paying occupations. In contrast, however, automation *did not* reduce employment rates in subsequent cohorts of young women, who found work in other sectors—including in jobs with similar demographics and wages (such as typists and secretaries), and some with lower wages (such as food service workers). This job growth is not attributable to mechanical switching's effects on productivity (which were low) or q-complementarity (which was specific to the telephone sector). Though wage data for this era are more limited, using available data we also do not find evidence that local labor markets re-equilibrated at significantly lower wages. The stability of both employment rates and wages is consistent with demand growing for these categories of workers in other sectors of the economy—and, in turn, with the predictions of Acemoglu and Restrepo (2018), who suggest firms will endogenously develop new uses for labor when automation makes it abundant. Buttressing this interpretation, our evidence indicates some occupations expanded to new sectors of local economies after cutovers—i.e., the emergence of new work (Autor et al. 2023). Taken together, these results suggest that although existing workers may be exposed to job loss, local economies can adjust to large automation shocks over medium horizons.

To understand this paper, it is useful to first describe AT&T (the principal U.S. telephone service provider for most of the twentieth century, through its regional subsidiaries), its operating force, and its mechanization. From AT&T's founding in the mid-1870s to the late 1910s, telephone calls were manually connected by operators, who by the early 1900s were almost entirely young, white, American-born women. By 1920, AT&T was the largest U.S. employer, accounting for over one percent of the non-farm U.S. workforce, and by far the largest employer of women. The growing network, however, strained the limits of manual technology, whose rapidly growing cost led AT&T to begin advising its operating companies to adopt mechanical switching, which diffused gradually across the U.S. telephone network over time (Feigenbaum and Gross 2023). Under this 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. Its effect was to nearly eliminate an entire major category of work, one city or exchange at a time.

Our analysis combines three sources of data. First, we measure cutovers to dial service across the continental U.S. using AT&T archival records and data collected from thousands of local newspaper articles. Of the nearly 3,000 cities in our sample, 332 have their first cutover by 1940. For most of the paper, we will focus on the 2,846 cities with $\leq$100,000 population in 1920 (261 with cutovers by 1940), where subscribers were typically converted to dial all at once. Second, to study successive cohorts of young women, we aggregate individual-level complete count census data from 1910 to 1940 to a city panel. This panel allows us to measure, for example, employment rates for specific ages and demographic groups in each city, or local populations in specific occupation-industry cells. Third, to study incumbent operators, we need to link these women across censuses. Because traditional census record-linking techniques are not capable of following young women over time (due to name changes prompted by marriage), we develop a new, generalizable approach to census linking: we match public genealogical data from the genealogy platform FamilySearch to complete count census records to track individuals over time—including through name changes—and reweight to account for the representativeness of FamilySearch data and our linking method. This approach to census record linking produces a broader, more representative linked sample of women than existing methods, and is among the contributions of this paper.[1]

To evaluate the effects of automation on incumbent workers, 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. Relative to non-operator women in the same city and telephone operators in untreated cities, we find that treated operators

[1] Prior approaches to building linked samples of women restrict to women whose marital status does not change across censuses (Marchingiglio and Poyker 2020; Price et al. 2021), or use marriage records to identify maiden and married names (Craig, Eriksson and Niemesh 2019; Withrow 2020). In our case, linking always-single or already-married telephone operators to their census record ten years later would condition the sample on an endogenous outcome or restrict our analysis to a small, non-modal population of operators, and the uneven coverage of marriage certificates across states would preclude a nationally representative sample.

were significantly less likely to be working as operators ten years later. While some found other jobs in the telephone industry, others (especially older workers) left the workforce, and those who remained were more likely to be in lower-paying occupations. The magnitudes of these effects are tempered by the fact that many women exited the workforce as they aged, but because telephone operation 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 chosen to keep them.

To estimate the effects on future generations of young women, we use an event study design to compare outcomes for successive cohorts before versus after a city's first cutover to dial. We show that employment, marriage, fertility, and school enrollment rates were trending similarly in the decades before automation across similar-sized cities with and without cutovers. 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 it did not reduce employment rates: the negative shock to telephone operator demand was counteracted by growth in other occupations, especially secretarial work and food service work, which absorbed the young, working women who might have otherwise been telephone operators.

Finally, we examine why telephone automation did not reduce employment rates of future generations of young women entering the labor market. To do so, we first use wage and employment data to probe whether (i) wages declined in these substitute occupations or for young women overall—which could indicate that the labor market re-equilibrated at lower wages after telephone operation was automated—or (ii) young, white, American-born women displaced other groups in these substitute occupations. We then consider mechanisms that could restore labor demand, including (iii) whether dial switching directly increased labor demand in complementary occupations or industries, (iv) whether the cost or efficiency of dial telephones raised productivity, and in turn labor demand, (v) whether concurrent technological changes might have driven structural change, and (vi) whether demand endogenously emerged that harnessed this newly-abundant population. Our evidence is most consistent with the latter. In addition to evidence inconsistent with other mechanisms, we show that most of the growth in secretarial employment took place in industries which had not previously employed these kinds of workers, which we interpret as evidence of the emergence of new work (Autor et al. 2023). Further reinforcing this interpretation, we find that displacement effects appear to dominate in environments which are less conducive to reinstating demand growth for our population (young women), such as in manufacturing-intensive cities or those with slack aggregate demand due to the Great Depression.

Our paper adds to a burgeoning empirical literature studying the effects of automation on workers and labor markets.[2] This literature often finds that automation displaces some workers (e.g., Bessen et al. 2019), but varies for which workers and with what net impacts on employment (Chiacchio, Petropoulos and Pichler 2018; Dauth et al. 2018; Graetz and Michaels 2018; Acemoglu and Restrepo 2020; Adachi, Kawaguchi and Saito 2020). The empirical literature studying the forces

[2]In addition to the automation literature, our results add to research on skill-biased technical change (Acemoglu 1998; Autor, Katz and Krueger 1998; Autor, Levy and Murnane 2003), including historical scholarship (Goldin and Katz 2008; Gray 2013) and studies of white-collar jobs that primarily employ women (e.g., Dillender and Forsythe 2019).

which blunt this displacement is less developed. Recent research has studied employment growth in firms or industries where automation is adopted, showing that automation can increase intra-firm labor demand through q-complementarity or scale effects (e.g., Aghion et al. 2021; Humlum 2021; Koch, Manuylov and Smolka 2021). Though Acemoglu and Restrepo (2018) hypothesize that technological and organizational innovation across the economy may endogenously create new uses for labor, this is more difficult to directly demonstrate (though it has been suggested by evidence in, e.g., Acemoglu and Restrepo 2019b; Autor et al. 2023). As a result, questions such as whether, when, and how new work will materialize to offset the jobs lost to automation are not fully resolved. This paper in part seeks to bring new evidence to this discourse.

Telephone operation is in many ways an opportune setting for studying these issues. One reason is its scale: as a large, geographically-dispersed, and entry-level job, automating telephone operation could have aggregate effects on both incumbent workers and future cohorts. A second is precision: telephone operation is a well-defined, well-measured occupation whose automation was discrete and can be precisely measured. In contrast to studies where automation is measured as industrial robot adoption or as a general category of capital investment, the specificity of mechanical call switching allows us to isolate what technology was adopted, which jobs it displaced, and which workers (or categories of workers) were implicated. In short, we can relate the technology to the specific task it performed and the workers who would have otherwise performed it.[3]

The historical setting of our paper, and the specificity of the job and industry, may raise questions of external validity.[4] In this case, our concerns are relatively low, for two reasons. First, we believe the insights we draw from our historical evidence are general, especially when seen through the lens of task-based theories of automation. Second, related work often produces valuable insights from similarly-specific jobs, industries, technologies, or settings with their own institutional complexity. History also presents opportunities: only over long periods can we observe (rare) moments when technology abruptly displaces a major occupation, and long panels allow us to examine how these shocks affect both existing workers *and* future cohorts—a population difficult to study in other settings. Similar to Humlum (2021), our results suggest these effects exist on a continuum: incumbent workers with the least time to adjust (and most invested in the occupation) suffer the largest consequences, whereas future generations are better able to adapt.

Even so, compared to modern technologies that are thought to have high automation potential—such as software, robots, or artificial intelligence (AI)—the share of overall employment directly affected by telephone automation was relatively small. Relative to these technologies, however, telephone call switching has three important differences. First, whereas robots, software, and AI are broad categories of technologies which have (or are likely to have) heterogeneous effects on different kinds of work, and research is often unclear on whether these technologies represent automation

[3]This allows us to complement studies such as Acemoglu and Restrepo (2022), which also examines how demographic groups' exposure to automation affects employment and wages—but where it is difficult to isolate discrete impacts or separate the effects of automation on existing and future workers.

[4]For example, overall female labor force participation was relatively low and growing in this period—though for the demographic we focus on, it was by 1940 close to current levels. Educational attainment was also rapidly growing across cohorts. Cohort differences, however, will be accounted for by fixed effects.

or capital-augmenting technological progress, mechanical call switching was *explicitly, specifically* labor-replacing. Second, we think there are there are few specific instantiations of these technologies today which would come to bear on a population as large as telephone operators. Third, local operators were eliminated more abruptly than most occupations which may be at risk of being automated today. In this context, we think displacement potential was intrinsically high, yet local economies nevertheless appear to adjust over relatively short horizons.

We proceed as follows. Section 1 reviews the history of the U.S. telephone industry, the automation of telephone operation, and concurrent trends in the labor market 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 show that cutovers significantly reduced the number of telephone operators in a city in the decades to come. In Section 5 we examine the effects of cutovers on incumbent telephone operators. In Section 6, we study what happened to subsequent generations of young women after these jobs were automated away and 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 known as "call switching". This work was fast-paced and labor-intensive. 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. 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. In a Women's Bureau report, Erickson (1946) summarized the job requirements as follows:

> [A]n 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 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 ...

Besides the minimum age requirements, most of these qualifications appear to have applied throughout the 1910 to 1940 period we study in this paper.[6] 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 of up to 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.[7] With 40% turnover rates, as much as 15% of cohorts born at the turn of the century might have *ever* been an operator.[8] Among young women age 16 to 20, for example, "telephone operator" was the fifth largest occupation, and given its concentration in one industry, "telephone operator in the telephone industry" was the single most common occupation-industry pair for this group. AT&T as a whole was the largest U.S. employer

[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).

[6]An additional requirement was race: AT&T did not hire black operators until after 1940 (Green 2001).

[7]Population statistics throughout the paper are based on the authors' calculations from census data.

[8]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.

of women in the 1910s, and by the early 1920s it was the country's largest employer overall, with telephone operators comprising around half of 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 continued with manual operation until improvements in the technology and rising costs of manual operation made automation more attractive (Feigenbaum and Gross 2023).

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. 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.

In Figure I 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.[9] In Section 2 we document

[9]We end our sample in 1940 in part because at the time of writing, complete count census data were only available through 1940, and in part because World War II halted cutovers (due to copper shortages) and presented a distinct shock to female labor demand (Goldin and Olivetti 2013; Jaworski 2014).

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 I about here]

By 1940, telephone operation in the telephone industry comprised $<1.5\%$ of employment for young, white, American-born women (down from its peak of $\approx 4\%$) and had fallen to the 11th most common occupation-industry pair for those under age 20.

1.4 Broader context: Trends in female labor force participation

Prior to the early 1900s, the stigma of being a working woman was quite high, and most women who worked did so out of necessity. This changed over the following decades: from 1900 to mid-century, female labor force participation grew steadily, accompanied by a large increase in demand for clerical and office workers (Goldin 1984). In 1900, only around 20% of non-farm working women were in white-collar jobs; by 1950, nearly 50% were. Office work was "nicer, cleaner, shorter-hour, and thus more 'respectable"' (Goldin 2006), though it could be repetitive, and turnover was high and returns to experience low. With the rise in demand for these 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—reflecting a labor force participation rate for this demographic, and especially young women, close its current level (see BLS 2022).

Changes in female labor force participation, educational attainment, and social norms are all important background trends in this period. Marriage bars—formal policies or legislation that discouraged or precluded the hiring of married women or the retention of women upon marriage—were among the more distinctive features of certain jobs in the early 1900s (Goldin 1988), though their incidence rose and fell over time.[10] Our study also covers the period when graduates of America's High School Movement hit the labor market (Goldin 1998): across women born from 1890 to 1925, mean educational attainment grew from just over 8 years for the 1890 birth cohort to nearly 11 years for the 1925 cohort (Goldin and Katz 2008). These differences across cohorts will be subsumed by fixed effects in our empirical design, which exploits the staggered diffusion of mechanical switching across cities and includes many cities without cutovers, which comprise a control group. Moreover, if marriage bars or increasing school attendance were coincident with the time and place of mechanization, we would expect to see even larger declines in post-cutover employment rates of young women—in contrast to the muted effects we will find below.

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

[10] Though most common among public school teachers, Goldin (1988) finds marriage bars present in some clerical employment. Green (2001) recounts AT&T had a general policy against hiring married women when single women were available, but explains that this rarely bound in practice and notes significant regional variation in operators' marriage rates. We see both single and married operators in our data (see Table II).

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-level adjustments to automation.

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).

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. 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 switchboard.

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 II maps the cities with cutovers in our data. Merging these data with 1940 city populations from the census, we find that by 1940, 86 of the largest 100 U.S. cities, and 40% of the largest 500, had at least one cutover, and 53.8% of the U.S. urban population lived in cities where telephone service was mechanized. The fraction of this population exposed to dial was greatest in the Northeast (58.9%) and lowest in the South (47.8%).

[Figure II 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.5 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 Figure B.2 provides 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 piecemeal fashion—motivating our empirical focus on smaller cities.

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 a city's 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.[11]

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 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 $\leq$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).[12]

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, birthplace, and birth year (Abramitzky et al. 2021). 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.[13] 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,

[11] 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.

[12] We drop the handful of cities with a population $\leq$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.6)—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 performed.

[13] One exception is Olivetti and Paserman (2015), who pseudo-link people over time using their likely socio-economic status, as inferred from their first names, to avoid linking women on surnames.

rather than prediction, to tell us which records belong to the same person. We apply this method to linking incumbent telephone operators (and demographically-matched control women), but our approach to building a longitudinal sample of young women could be applied to other questions and analyses that require linked census data (e.g., Buckles et al. 2023).

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.[14] Second, we look for each of these women on FamilySearch, a public genealogy platform with an open wiki-style family tree (Price et al. 2021), where users create pages for deceased individuals—usually their own ancestors—and attach links to historical records, including entries from Federal Censuses, marriage records, and birth certificates. 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 on the tree.[15]

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 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

[14] 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 $\leq$100,000 in 1920, which is our core sample throughout the paper. 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.

[15] 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.

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 genealogical data can help. Suppose we start with a telephone operator in 1920 in New York named Daisy Fay. The 1920 census tells us Daisy was born in 1902 in Kentucky. With traditional census linking methods (e.g., Ferrie 1996; Abramitzky et al. 2021), we would search the 1930 census for a Daisy Fay, born in 1902 in Kentucky, likely with some tolerance for enumeration or transcription 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 may falsely match two distinct people. 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 1930 census record, 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 attached in FamilySearch to both the 1920 and 1930 censuses, she will make our sample.

The set of operators in this sample is inevitably not random. There are two reasons we do not think this selection is likely to be a threat to inference. First, the likelihood of being matched to the tree or linked across censuses is not correlated with our cutover treatment (Appendix B.3). Second, when we artificially limit our sample to women who are always-single or always-married in t and $t+10$ (i.e., the sample we would restrict to with traditional linking methods) we find similar results to those in our full sample. Selection is also not a problem unique to our source and setting: Bailey et al. (2020) document the general unrepresentativeness of most historical linked samples made via algorithms. To account for this bias, we follow Bailey et al. (2020) 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, and weight all regressions with inverse propensities to obtain representative results.[16]

3 Characteristics of Telephone Operators and Cutover Cities

3.1 Characteristics of telephone operators

Table I 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] Key features include age, race, middle name/initial, name commonness, name length, marital status and fertility, and state of birth and residence in the base year, which helps us account for selection into the FamilySearch sample vis-à-vis descendants or genealogists—especially because FamilySearch is affiliated with the Church of Jesus Christ of Latter-Day Saints.

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 I about here]

We can measure the characteristics of telephone operators directly in the census data—including counting how many were young women. Table II 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 (≤25) to older (26+) women, who were often senior operators, and 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 II 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 2023), we study what propelled AT&T's automation of telephone operation and why it took nearly 60 years to complete. Drawing on company records and empirical evidence, we find that automation was primarily a response to the technical demands of the growing telephone network, rather than labor market conditions. Though manual switching served early telephone networks well, expansion revealed its limits, as its complexity rose quickly in large markets. As AT&T grew, switchboards thus became system bottlenecks, service quality fell, and operator requirements (and the marginal cost of new subscribers) exploded (Lipartito 1994). Mechanization was an effort to slow this cost growth and support the firm's continued expansion.

We verify this in Table III, where we relate cutovers to city characteristics, measured in 1910 (pre-treatment) where possible, and otherwise for the earliest period observed. The outcome variable in Columns (1) to (3) is an indicator for whether a city had a cutover pre-1940, and in Columns (4) to (6) is the year of its first cutover. We regress these outcomes by OLS on a wide range of city characteristics, including population, demographics, education and income, labor force characteristics, and telephone operator union activity.[17] Across all columns, population is the main determinant of cutovers, explaining more variation then any other variable—including state fixed effects, which are included in all columns.[18] In a full horserace regression, we find that cities with cutovers before 1940 were larger, richer, and more likely to have unionized telephone operators (Column 3), though population has nearly ten times the explanatory power of other variables.[19] Conditional on having a pre-1940 cutover, larger cities had earlier cutovers (Column 6).[20]

[Table III about here]

The results in 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 (see Feigenbaum and Gross 2023). In addition to city and year fixed effects, we will 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 on changes in outcomes of interest, which will reinforce our confidence in our ability to identify the effect of cutovers on other outcomes.

Notwithstanding the evidence above, a remaining concern is that automation may have been endogenous to labor market conditions: if tight labor markets both drive cutovers and soften their impacts, this could confound our results. We are reassured by two observations. First, cutovers would be shaped by expected labor demand, but their impacts by realized demand—and due to macroeconomic volatility, these might diverge. Second, we perform robustness checks controlling for projected local employment growth, and our results are unchanged.

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

[17] Population proxies for the size of local telephone markets, which is not systematically observable across our sample. In large U.S. cities (population >50,000) reported in AT&T's annual internal publication *Bell Telephones in Principal Cities* (AT&T 1915), population and subscribers correlate nearly perfectly.

[18] The R^2 of a regression of $\mathbb{1}$(Any cutover pre-1940) on state fixed effects alone is 0.04, and that of cutover year on state fixed effects alone is 0.05. The partial R^2 of log population in each case is around 0.2.

[19] Despite the evidence that cities where operators had unionized were more likely to be cut over by 1940, organized labor is unlikely to play an important role in this paper, as independent operator unions were replaced by company unions in the early 1920s. This fact might also explain why the relationship between historical (pre-1920) operator unionization and cutovers is statistically weaker—and absent in some specifications.

[20] Appendix C provides additional evidence. Appendix Table C.1 shows mean city characteristics by the timing of a city's first cutover, illustrating these patterns in the raw data. Appendix Figure C.1 shows that cutovers were not related to prior changes in labor market outcomes that are the focus of this paper, except for an increasing share of young women working in telephone operation—consistent with AT&T's problem. Appendix Table E.6 shows that cutovers were not related to other, potentially-coincident technological changes.

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 incumbent operators and future generations of young women. An important first step, however, is to evaluate how mechanical switching affected demand for telephone operators. In this section, we establish that—consistent with contemporary reports—both the number of telephone operators and the share of young, white, American-born women who were operators plummeted after local telephone service was mechanized.

4.1 Empirical approach

We take two empirical approaches to studying the effects of dial. Here and in Section 6, we analyze effects on local labor markets with a two-way fixed effects (TWFE) specification, exploiting the staggered adoption of mechanical switching and comparing outcomes before and after each city's first cutover. In Section 5, we use our linked samples and estimate the effects of cutovers on incumbent operators, comparing those in cities with cutovers to those without, and further comparing these operators to a matched control set of demographically- and economically-similar women. In all of our analyses, our focus will be on cities with population ≤100,000 in 1920, where automation was typically a discrete event (Appendix Figure B.2).[21]

Our first set of results will estimate the following event-study specification:

$$Y_{ijt} = \sum_s \beta_s D_{it}^s + \zeta_{ij} + \eta_{jt} + X_{ijt}\phi + \varepsilon_{ijt} \tag{1}$$

on a panel at the city-age-year level, where i, j and t index city, age, and census year, respectively; D_{it}^s are treatment indicators in event time, with s indexing years since a city's first cutover (i.e., D_{it}^s indicates that city i in year t had a cutover s years ago); ζ_{ij} and η_{jt} are fixed effects; and X_{ijt} are time-varying controls. Treatment is measured at the city level, and effects are estimated relative to the immediate pre-treatment period, which serves as the reference category 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).

In nearly all specifications we will restrict attention to a single subpopulation (e.g., white, American-born women age 16 to 25, pooled or by age). Our outcome variables generally take the form of the log number of people in that subpopulation of a certain type (e.g., the log number of telephone

[21] For this analysis, we pare our sample to the 2,845 cities with 1920 population ≤100,000, without a cutover before 1917 (which were rare and were only performed by independent telephone companies, which typically competed in rural areas outside of our sample), and for which we can produce a balanced panel of young, white, American-born women. Of these, 261 have a cutover pre-1940. In Appendix C.8, we also consider the "large" cities in the AT&T data, for which we know the fraction of subscribers with dial service by 1940, and study long-difference outcomes (1910 to 1940) as a function of this intensive measure of adoption.

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. We cluster standard errors at the city level.

Our standard set of controls X_{ijt} consists of log city population crossed by age and year fixed effects, which account for differential trends, for different ages, in larger and smaller cities.[22] These controls are important because population is closely related to cutovers (Section 3), and may also correlate with outcomes. For example, high school completion rates among 16 to 18 year-olds were rising throughout this period, and differentially so in larger cities. These trends made these 16 to 18 year-old young women less likely to be working, mechanically reducing their employment rates for reasons unrelated to cutovers. Age-specific population trends will account for these background differences, which otherwise risk confounding our results. As an empirical matter, these controls eliminate differential pre-trends across the outcomes we study.

After establishing that the effect of cutovers is an immediate, permanent, level decline in the fraction of young women who were telephone operators, which is a difference-in-difference (DID) result, we will replace event studies with a staggered DID specification for other outcomes (that is, TWFE with a binary treatment, replacing D^s_{it} in Equation (1) with D_{it}, which indicates whether city i in year t is post-cutover). This yields the following specification:

$$Y_{ijt} = \beta \cdot \mathbb{1}(\text{Post-Cutover})_{it} + \zeta_{ij} + \eta_{jt} + X_{ijt}\phi + \varepsilon_{ijt} \tag{2}$$

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 cause a small but significant decrease in this share, of around 0.2 percentage points (p.p., in blue). However, as we know from Table II, 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 III shows the effects of cutovers on the (log) number of young, white American-born women

[22] The underlying model we have in mind is one where automation was profitable in markets above a certain scale, but where this threshold was falling over time, as the technology improved. Crossing city population by year fixed effects accounts for a relationship between population and cutovers specific to each moment in time. Because the local population could potentially be endogenous to the treatment, we measure population excluding the young, white, American-born women that are the focus of our analysis.

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 2023)—it subsequently dropped by 50 to 80% (Panel A). Our higher-frequency estimates indicate that the cutover effect kicked in immediately (Panel B).[23]

[Figure III about here]

In Figure IV 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 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. This view of the data also makes clear that the effect is in essence a DID result, motivating our use of a staggered DID specification (i.e., TWFE with staggered, binary treatment) throughout much of the rest of the paper. Panel (B) estimates this staggered 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, long-lasting negative shock to labor demand.

[Figure IV about here]

In Appendix C.4, we evaluate the robustness of these results to other estimation methods. A flurry of recent papers has highlighted the potential challenges of estimating TWFE models with staggered treatment, especially in the presence of treatment effect heterogeneity or dynamic effects, and when most or all of the sample is treated. To a first order, we do not expect these challenges will 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 IV suggests this shock was a pure DID, without time-varying effects. Even so, in Appendix Figure C.4 we present robustness checks using the estimators of Sun and Abraham (2021), Callaway and Sant'Anna (2021), and Borusyak, Jaravel and Spiess (2021), where we find consistent results across all four approaches. In light of this evidence, we use OLS for the remainder of the paper.

We present three other robustness checks in the online appendix. First, although we are estimating these effects in all cities in the continental U.S. meeting our sampling criteria, our measurement of

[23] 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.

cutovers is in part dependent on the geographic coverage of our historical newspaper data sources. To address 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 (Appendix C.5). Second, we also estimate the effects of dial in larger cities using the AT&T sample and a long differences strategy—exploiting the intensity of local dial penetration in large cities from 1920 to 1940—and find quantitatively similar results throughout the paper (Appendix C.8). Third, we examine the effects of cutovers on successive cohorts of 26 to 35 year-old women, motivated by our findings in Section 5 that older incumbent operators experienced more adverse effects. Because telephone operation comprised a smaller share of older (26+) women's employment, the effects on future cohorts are smaller but directionally similar to those on younger women (Appendix D.2).

5 Effects on Incumbent Telephone Operators

Contemporary sources offer hints of what might have happened to incumbent operators after telephone service was mechanized. 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 surveys of displaced operators in two cities, both of which are in our sample. Of the 78 women surveyed, a year later 18 were re-employed by the telephone company (including at exchanges in other cities), 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, telephone companies "sought the cooperation" of local businesses "in finding possible work for the operators affected" (Best 1933).

In this section, we systematically examine the effects of automation on incumbent operators, complementing contemporary studies like Best (1933). To do so, we turn to our linked sample of young women and ask what happened to those who were telephone operators in the 1920 or 1930 census and whose jobs were subsequently replaced by mechanical technology.

5.1 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:

$$Y_{ict}^{t+10} = \beta_1 \cdot \mathbb{1}(\text{Operator})_i \cdot \mathbb{1}(\text{Cutover})_{ct} + \beta_2 \cdot \mathbb{1}(\text{Operator})_i + \delta_{ct} + X_i \phi + \varepsilon_{ict} \quad (3)$$

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.[24] In our most demanding specification, we replace the city-year fixed effects with operator-and-control-worker pair fixed effects, which conditions comparisons to within individual operators and their associated control women (and subsumes the city-year fixed effects). In the tables below, we present results pooling the 1920-30 and 1930-40 linked samples. We cluster standard errors by city and use inverse propensity weights to account for selection in our linking procedure (Bailey et al. 2020).

5.2 Effects on incumbent telephone operators

We begin our analysis in Table IV 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 show results for year-t operators of all ages (Columns 1 and 2), and subsequently break out effects for ages 16-20 (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 IV about here]

Echoing our results from Section 4, cutovers significantly reduced the likelihood of employment as telephone operators in the telephone industry. Table IV, 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 IV, 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 V, Panel (A) that cutovers put many incumbent operators out of work. Operators who were over age 25 in the base year were roughly 7 p.p. less likely to be working after a cutover, relative to peers in untreated cities—accounting for more than half of the displacement of

[24]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 (± 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.

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 V 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 VI, 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.[25] The occupation score of operators exposed to cutovers and still working a decade later on average fell 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 VI about here]

In Appendix Tables D.2 and D.3 we also examine the effects of cutovers on migration. We measure migration in a variety of ways—whether operators were more likely to move more than 10, 25, or 50 miles, or whether they were more likely to be living in a different city, local labor market, or state than they were ten years prior—using geolocated data from the Census Place Project (Berkes, Karger and Nencka 2023). Across all measures, we find increased migration of incumbent operators after cutovers, with these effects are driven by older incumbents (ages 21-25 or 26+). That automation induced migration is intuitive, but that it did so for women in an era of low female labor force participation and social expectations of women not holding long careers is more surprising. The

[25] 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. To answer this question we construct occupation scores for women in 1940 as the median earnings reported among all women in 1940 in each occupation, analogous to how IPUMS creates occupation scores for the entire population in 1950.

magnitudes are not large and are statistically imprecise in places, but, taken together, they suggest that incumbent operators, especially those with more time invested in their careers as operators, were more likely to move away from mechanized cities.

6 Effects on Future Cohorts of Young Women

The evidence in Sections 4 and 5 shows that mechanical switching decimated demand for young telephone operators and drove incumbent operators into lower-paying occupations or out of the labor force entirely. Did future generations of young women entering labor markets where these opportunities had vanished fare as poorly? If not, where did they find work? In this section, we return to our city by demographic group panel, and our staggered DID empirical design in Section 4, and evaluate how telephone automation affected local labor markets.

6.1 Employment rates and substitute occupations

Table VII 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 (16 to 25, 16 to 20, and 21 to 25). The first column presents the effect of cutovers on the fraction of each group working as telephone operators, which provides a reference point for effect sizes in other outcomes. We find no effects on young women's employment rates. We likewise find no effects on the fraction in school or married, and a modest impact on fertility for the youngest women in our sample.[26] We can rule out unemployment increases of the magnitude of the shock itself at just above the 10% significance level, and can rule out greater impacts at lower levels.

[Table VII about here]

One concern is the possibility that these results could be confounded. For example, if automation is more likely to take place when labor demand is growing (Dechezleprêtre et al. 2021), this may have softened the impact on employment. We undertake several additional analyses to probe this possibility. The first is to control for measures of expected demand growth (see Appendix C.6). To do so, we construct shift-share instruments to project local employment growth from local industry employment shares in each census year and next-decade, leave-one-out national industry growth rates.[27] We then control for this variable in levels and in percentiles (which compresses outliers). In both cases, our results are unchanged. We also control for cities' 1910 industry employment shares, crossed with year fixed effects, and our results remain unchanged.

In a complementary, backward-looking set of robustness checks, we test for pre-trends. Appendix Figure C.1 first presents balance tests in which we compare prior-decade changes for cities which (i) experienced their first cutover in the next decade, to those which (ii) would not be cut over

[26] Follow-up analysis on the marginal fertility result suggests it may be spurious: this effect is statistically detectable only for 20 year-olds, but not 19 year-olds, 21 year-olds, or other ages.

[27] Because complete count census data are not yet available for 1950, we use the IPUMS 1% sample to compute 1940-50 national industry growth rates (in this case, not leave-one-out, since the sample does not report city).

to dial for at least another decade. We find no systematic differences in employment rate changes in the run-up to cutovers. In Appendix Figures C.2 and C.3, we plot complete 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. We also undertake DID due diligence in Appendix C.7, estimating the effects of cutovers by census decade and cutover decade, where we find that these results are time-independent. Finally, we also examine local population changes: if marginally employable women migrated away after cutovers (as some incumbent operators did; Section 5), then local employment rates might have been sustained by selective out-migration. We find that local population (both total and of young, white, American-born women) was growing more rapidly in advance of cutovers and continued growing after cutovers—consistent with service area growth being AT&T's motivation for automating call switching, but suggesting against population declines as an explanation for our results.

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 A.2). 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 A.3), which we consider the closest substitutes. In the analyses below, we restrict our attention to service sector jobs, where most of the adjustments appear to have taken place.

Table VIII estimates the 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.[28] Growth in middle-skill secretarial jobs and low-skill service jobs offset most of the operator jobs lost to automation. When examined by age, we find that "older" young women often moved into similar-paying secretarial jobs, whereas those of younger ages were more likely to be in lower-paying service jobs, like food service.[29] Consistent with this evidence, the final column estimates the effects of cutovers on occupation scores, finding a small but statistically significant decline, particularly for the youngest women in our sample—reflecting the earlier evidence that although some women entered similar-wage jobs to telephone operation (like office

[28]The effect of cutovers on telephone operation employment in Column (1) of both Tables VII and VIII are of different magnitudes because each table is measuring outcomes within (slightly) different subpopulations. In Table VII, we study outcomes as a share of the white, American-born, female population, while in Table VIII we focus on *working* white, American-born, women. 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.

[29]The magnitudes of these effects reinforce that young women's employment grew disproportionately in these occupations. Had future generations reallocated according to base employment rates, the share of young women in secretarial work would have increased by by $1.52 \times \frac{11.61}{100-1.52} = 0.179$ p.p. (versus the estimated 0.54 p.p.), and the share in restaurant work to increase by $1.52 \times \frac{4.15}{100-1.52} = 0.064$ p.p. (versus 0.83 p.p.). Other common occupations for women in this period besides the ones shown in the table include factory work, private household work, teaching, and nursing. We do not find that these occupations grew significantly after cutovers.

work), some were in slightly lower-paying jobs (like food service).

[Table VIII about here]

Robustness checks on Tables VII and VIII parallel those in Section 4.2. We obtain similar results—most notably, no effect on employment rates—when we restrict to cities with continuous newspaper coverage between 1917 and 1940 (Appendix C.5), in our large city, long-differences analysis (Appendix C.8), and for 26 to 35 year-old women (Appendix D.2).

6.2 Why was employment so stable?

Why did local labor markets adjust so smoothly to the automation of telephone operation? We explore six possibilities. We first consider whether our results can be explained by inelastic supply, examining whether (i) the labor market re-equilibrated at lower wages, or (ii) the influx of would-be operators into substitute occupations displaced other demographic groups from these jobs. We then consider mechanisms through which this shock may have been offset by growing labor demand, probing whether: (iii) dial switching directly increased labor demand in complementary occupations; (iv) the lower cost or improved efficiency of dial telephones may have increased aggregate productivity, and in turn aggregate labor demand (via scale effects); (v) other technological changes (e.g., electrification) may have coincided with mechanical call switching and increased productivity growth and labor demand; and (vi) new uses for labor emerged that absorbed this newly-abundant population—e.g., whether automation may have "endogenously generated incentives for firms to introduce new labor-intensive tasks" (Acemoglu and Restrepo 2019a).

6.2.1 Supply-based explanations

Inelastic supply Conceptually, labor markets can sustain overall employment rates after a negative demand shock in a large occupation (like telephone operation) in two ways: (i) if supply is inelastic—in which case wages will decline—or (ii) because demand recovers. Similarly, there are two adjustment mechanisms at the occupation level: when young, white, American-born women's employment grows in other occupations, then either (i) labor supply shifted out while the demand curve was unchanged, and wages fell, or (ii) both the labor supply and demand curves shifted out, and wages were stable. We attempt to distinguish between these possibilities by studying the relation of cutovers to workers' wages, using data from the 1940 census.[30] We first calculate individuals' weekly wage, as census-reported 1939 wage income over 1939 weeks worked. We then compute local mean and median wages of fine-grained demographic groups, overall and in specific occupations. We use these data to compare wages of: (i) young, white, American-born women in cities with cutovers between 1938 and 1940 (approximating a regression discontinuity design around 1939, the year of the measured wages); (ii) young vs. older white, American-born women in

[30] The 1940 census was the first to record respondents' wages. The advantage of these data is that they are measured at the individual level and can be used to compare wages of demographic groups more or less exposed to telephone automation, in cities that have or have not been cut over to dial by 1940. Their limitation is that we only observe a cross-section, rather than a panel. To our knowledge, no other source of wage data exists for earlier periods with sufficient granularity to combine them with 1940 census-reported wages in a panel.

cities with and without cutovers (exploiting their differential exposure to the telephone industry's automation); and (iii) young, white, American-born women vs. men.

Across these tests, we do not find evidence of systematic wage declines or differences in relation to cutovers (Appendix E.1.1). These results should be interpreted with some caution, given standard errors (we cannot statistically rule out wage changes of ±10%), which may be due to heterogeneous effects, noise in the wage data, or more limited power afforded by a smaller set of cutovers (72 vs. the 261 in our main sample). But the lack of a clear, detectable effect on overall wages of young, white, American-born women—despite the size of this shock—suggests our results are unlikely to be explained by inelastic supply. Moreover, the absence of a detectable effect on wages in substitute occupations suggests against the view that the labor market simply moved down the demand curve in these occupations, settling at higher employment and lower wages.

Crowd-out We also examine whether would-be operators crowded out other workers from substitute occupations. To do so, we estimate the effects of cutovers on young, white, American-born women's share of employment in these occupations (which should rise if they are crowding out others). Although cutovers led to a large decline in this group's share of telephone operators (consistent with our understanding and evidence that mechanical switching mainly affected junior operators), they had no impact on its share of other occupations, suggesting that the reallocation of would-be operators into other occupations did not displace other populations who already, or who would have otherwise, had the jobs these women took up (Appendix E.1.2).

6.2.2 Demand-driven explanations

Direct effects on labor demand In principle, dial service may have created demand for complementary workers, such as technicians to maintain the mechanical equipment (though these were, in practice, nearly all male), or office clerks to perform non-automated residual operator tasks. Insofar as it supported a growing telephone network, mechanization may have also generated demand from other employers for private (internal) operators, or for office workers to manage growing call volumes. Figure V rules out several of these adjustment channels, showing that cutovers had no effect on young, white, American-born women's employment in non-operator jobs in the telephone industry or telephone operator jobs in non-telephone industries.[31]

Productivity growth and scale effects We next consider whether mechanical switching may have had wider productivity impacts (beyond the telephone industry) and raised demand for other workers. Any such productivity gains would have had to run through lower telephone service prices or higher service quality. In historical sources, however, we see that cutovers were typically accompanied by telephone rate increases, rather than decreases (i.e., AT&T did not pass through its cost savings; see Appendix Figure A.4 for examples from newspapers). The technical efficiency savings of dial service also appear to be minuscule: in Appendix E.2.2, we show that it likely yielded annual time savings of less than 1.5 hours per business telephone. Against this evidence, it is unlikely

[31] Dial service was also unlikely to generate much demand for office workers outside of the telephone sector, for two reasons. First, the majority of telephone subscribers were residential and placed their own calls. Second, the time cost of telephone dialing was small (seconds per call), and most firms with telephones would need at most a small fraction of a full-time secretary to manage their telephone call volume.

that productivity gains from cheaper or improved telephone service, and any resulting expansion in output and increase in labor demand, can explain our results.

Contemporaneous technological change We also consider whether other technologies might have coincided with cutovers and offset their impacts. We focus on electricity and motor vehicles, each of which diffused rapidly between 1900 and 1940 and had significant impacts on the organization of production. To evaluate whether these changes coincided locally with telephone industry automation, in Appendix E.2.3 we identify associated occupations and estimate whether they grew or contracted after mechanical switching was adopted. We find that telephone operators per capita fell sharply after cutovers, but we find no concurrent changes in, e.g., electricians, auto mechanics, or truck drivers per capita. We interpret this evidence as indicating that cutovers did not locally coincide with the diffusion of these other important technologies.

Countervailing demand growth The remaining possibility we explore is that demand grew in other occupations, harnessing newly-abundant workers after telephone service was mechanized. Acemoglu and Restrepo (2018) predict that in these contexts, firms may endogenously create new uses for labor as old uses get automated, and that this is how employment rates can be sustained even as increasingly more tasks are performed by capital. Acemoglu and Restrepo (2018) label this process "task reinstatement", in reference to the invention of new tasks in which labor has a comparative advantage, explaining that "automation may endogenously generate incentives for firms to introduce new labor-intensive tasks" (Acemoglu and Restrepo 2019a). In practice, innovation that leads to new work need not be technological: new work can also emerge from organizational innovation, with employers finding creative new applications for labor, which we think is more likely to be the underlying source of new work in the setting we study.[32]

The Acemoglu and Restrepo (2018) mechanism is difficult to evaluate directly without measures of the specific task content of workers' jobs. But we find several pieces of evidence across surrogate endpoints which are consistent with this mechanism. The first is that we see employment growth in occupations that are broadly similar in skill and demographics to telephone operators—a pattern we think is unlikely to occur by chance. These substitute occupations may at first seem like "old" work: typists, secretaries, and stenographers existed before mechanical call switching was adopted. There are two possibilities that could embody task reinstatement. One is that the underlying task content of these jobs expanded. A second, distinct possibility is that local labor demand grew for existing uses of these workers in new sectors. For example, if doctors began hiring stenographers to take patient notes, the job (stenographer) would not have been new, the task (note-taking) not new, but the medical application was new. It would have also carried a new title, such as "Medical stenographer"—which is an actual title that emerged in this era.

In an influential recent paper, Autor et al. (2023) study the emergence of new work by measur-

[32] In the endogenous process which Acemoglu and Restrepo (2018) describe, automation in the aggregate reduces the cost of labor, making further automation less attractive and encouraging innovation creating new uses for labor. Though this mechanism operates through prices, wages need not observably decline before demand rebounds, for two reasons: first, markets can adjust on expectations, and second, they may adjust faster than we can measure changes in wages—especially because cutovers were public knowledge and widely known.

ing new titles listed in government occupational dictionaries, many of which arise as specialized variants of existing titles. Motivated by this argument, and by the medical stenographer example (and others like it), we use our data to study the emergence of new occupation-industry pairings: the proliferation of specific types of work to new industries.[33] Empirically, we ask whether in the aftermath of telephone automation, firms began to employ young, white, American-born women as secretaries, stenographers, waitresses, and so on in industries that had not previously employed them. Our focus is especially on locally-new work, and we are agnostic on whether it is a product of invention or diffusion (both would be consistent with theory).

The data offer some descriptive clues and context. For example, secretarial and stenographic work was broadening in this era: in 1910, the top 5 industries for these workers accounted for 63% of their total; by 1940, this share was 46%. Food service workers were more concentrated (two industries, *Eating and drinking places* and *Hotels and lodging places*, account for >90% of them in every decade), but were growing quickly in drug stores, a new setting.

In Appendix E.3.1, we investigate the effect of cutovers on the share of young, white, American-born women's employment in a given occupation and in local industries which had not previously (to 1900) employed a worker in that occupation. We find significant growth in typist, stenographic, and secretarial employment in new industries, but not existing industries, with most of the effect in Table VIII attributable to new industries—consistent with the conjecture that employment in these fields was enabled by the growth of new work. We do not see similar patterns for other occupations, however, suggesting that either (i) our occupation x industry measures are too coarse to pick up on this phenomenon for industrially-concentrated jobs like waitressing, or (ii) demand in these occupations may have grown for existing uses of labor.[34]

6.2.3 Limits to demand reinstatement

This evidence of countervailing demand growth raises the question of how general this result might be—and under what conditions it is likely to arise. For example, general-purpose technologies or innovation-led structural transformation may induce complementary innovation that develops new uses for labor at the same time as old uses obsolesce. Certain sectors may be more (re)inventive. When aggregate demand is slack, innovation may be weakly incentivized, and displacement effects of automation may dominate—generating employment declines.

We explore these questions in Appendix E.3.2, where we examine how the effects of cutovers interact with the local technological and economic environment. We do not find differential effects across cities by technological conditions. Effects do, however, relate to two economic factors: manufacturing intensity and Great Depression severity. Because manufacturing—in our era and sample

[33]Though census data include occupation strings (the raw responses to "what is your occupation?"), these are often too generic (e.g., most secretaries respond "Secretary") and sometimes too varied (e.g., due to transcription errors) to be used to measure new work analogously to Autor et al. (2023)'s use of occupational dictionaries. We believe occupation-industry pairings are more cleanly measured and provide similar information.

[34]A third possibility is that demand in these other occupations was unchanged, and would-be operators' reallocation into these fields put downward pressure on wages. Although our wage analysis in Appendix E.1.1 suggests against this possibility, data challenges limit strong conclusions. Reinstating demand growth and wage declines could each be present in different occupations.

cities—was a predominantly male sector, manufacturing-intensive cities may have been less likely to endogenously generate new demand for young women in white-collar work. We also find that in cities with the most severe contractions during the initial downturn of the Great Depression—sometimes called the Great Contraction (Friedman and Schwartz 1963)—cutovers were followed by employment declines. This suggests that aggregate demand has a direct impact on whether, when, and to what degree labor demand can recover from large automation shocks. That the estimated effect is monotonic in depression severity bolsters this takeaway.

6.3 Connecting the results for incumbents and future cohorts

Taken together, the results of Sections 5 and 6 suggest the effects of automation on employment vary by age. Older incumbent workers, who may have spent years building occupation- and firm-specific human capital that is suddenly obsolete, are more adversely affected. Younger workers—including future generations not yet in the labor force, or not even yet born—are more adaptive to an evolving labor market. This, in our view, is where the two results meet.

These heterogeneous effects of automation by age are consistent with the task-based view of automation, where mismatched tasks and skills can impede labor market adjustments (Acemoglu and Restrepo 2019a), as well as with recent evidence from Humlum (2021), who finds that the welfare impacts of industrial robots are concentrated in older displaced workers. These difficulties are magnified when automation eliminates an entire occupation and forecloses future opportunities in that field, as in this paper.

7 Conclusion

The automation of telephone operation is among the largest discrete automation shocks in history. The specificity of the job, which is coincident with the automated task (call switching), makes it a unique opportunity to study what happens to employment when technology replaces an entire major category of work, and to connect the evidence to task-based theories of automation and technical change. 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. Around 2% of this group's jobs were permanently replaced by machines overnight, one city at a time.

We find that 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. By contrast, the shock did not reduce future cohorts' employment rates. Instead, demand in comparable middle-skill office jobs and lower-skill service jobs grew to absorb future young workers, and did so fairly quickly. Though these results validate contemporary concerns over what would happen to existing operators whose jobs were replaced, anxiety over the opportunities available to future generations proved to be somewhat misplaced, as future workers found work in other fields—often taking up similar-quality jobs.

We consider these results to be a distinctive reference point in the growing literature on how automation affects workers and labor markets. We find that while dislocations do occur, new tasks for labor can develop fairly quickly. The speed of adjustment suggests there may even be latent demand for these workers in new sectors—i.e., the lawyers or physicians who would have previously liked to hire a legal or medical stenographer but faced too much competition for young women workers from the local telephone company. A residual question, however, is how general historical episodes such as this one may be. Here, a few points are worth noting. Through the lens of theory, the factors at play are thought to be time-invariant. Jobs which were growing in this period (like office work) were a natural source of countervailing labor demand—yet our evidence indicates that they grew in new, not-yet-seen directions after telephone operation was automated. Moreover, the automation shocks we study occurred relatively abruptly. Most automation threats today are slated to take place over longer horizons, providing future workers more time to adapt their educational investments and early career choices to a changing labor market.

The demographics of telephone operators are also relevant to our findings. Telephone operators were typically young white women, a group that occupied a very specific position in the economic and social structure of the early 20th century. Operators (and potential future operators) were not only directly exposed to automation, but may have also had access to a wider range of other work opportunities than other women. At the same time, female labor force participation, though rising, was not universal, and many jobs in this era had explicit or implicit gender bars. Labor market discrimination could influence the impacts of automation in our setting—sharpening or attenuating its effects. The racial and class dynamics in the U.S. in this era could also have set the conditions for negative spillovers on other demographic groups (e.g., Black women), who might have been pushed out of occupations that former or would-be operators took up. However, we do not see evidence of spillovers: wages did not fall in substitute occupations, and young white women's share of these jobs did not grow. Whether social divisions may shape the incidence of automation's impact on workers in other contexts is a question we leave for future research.

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 I: 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
Working population share	42.5%	45.2%	38.9%	28.3%	37.7%	40.6%	44.4%	45.2%
Percent of working pop. who are tel. operators in tel. industry	3.2%	4.5%	4.0%	1.3%	2.3%	3.3%	3.3%	1.5%
Percent of working pop. who are 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 of working pop. who are tel. operators in tel. industry, by 1920 city size (%)								
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 II: 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 III: Determinants of automation: What explains cutovers?

	Any cutover by 1940?			Timing of earliest cutover		
	(1)	(2)	(3)	(4)	(5)	(6)
Ln(Population)	0.146***	0.138***	0.137***	-1.744***	-1.746***	-1.986***
	(0.007)	(0.007)	(0.007)	(0.200)	(0.218)	(0.247)
Percent black		-0.002**	-0.004		0.126**	0.085
		(0.001)	(0.003)		(0.063)	(0.165)
Percent foreign		-0.000	-0.001		0.024	0.063
		(0.001)	(0.002)		(0.047)	(0.137)
Percent MS grads, 1940		-0.000	-0.000		-0.111	-0.133
		(0.001)	(0.001)		(0.111)	(0.112)
Percent HS grads, 1940		0.001	0.002		-0.017	-0.022
		(0.002)	(0.002)		(0.099)	(0.114)
Ln(Avg. income, 1940)		0.117***	0.107***		0.315	0.882
		(0.033)	(0.033)		(2.762)	(2.860)
Average occupation score		-0.000	0.001		0.225	0.365
		(0.003)	(0.004)		(0.341)	(0.367)
Unionized by 1920		0.086**	0.086**		-2.042	-1.793
		(0.036)	(0.036)		(1.285)	(1.297)
Had strike by 1920		0.065	0.064		0.566	0.717
		(0.052)	(0.052)		(1.498)	(1.478)
Percent female			-0.002			0.197
			(0.003)			(0.134)
Percent f/n			0.006			0.076
			(0.006)			(0.360)
Percent f/n/w			-0.006			-0.112
			(0.006)			(0.328)
Percent f/n/w/y			0.002			-0.022
			(0.003)			(0.342)
F/n/w/y pct. working			0.000			0.044
			(0.001)			(0.050)
F/n/w/y pct. operators			-0.005			-0.290
			(0.006)			(0.405)
N	2992	2986	2986	332	332	332
R^2	0.25	0.26	0.26	0.31	0.34	0.35
Y mean	0.13	0.13	0.13	1929.08	1929.08	1929.08
State FEs	Y	Y	Y	Y	Y	Y

Notes: Table presents horserace regressions of (i) an indicator for whether a city had its first cutover by 1940 (Columns 1 to 3) and (ii) the timing of that first cutover (measured in decimal years; Columns 4 to 6). All explanatory variables are measured for cities in 1910 except for income and educational attainment, which were only collected by the census in 1940. 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). Heteroskedasticity-robust SEs in parentheses.

Table IV: 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?							
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 and Control Worker Pair 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	0.22	0.44	0.15	0.35	0.21	0.41	0.34	0.55
Y Mean	0.04	0.04	0.03	0.03	0.04	0.04	0.12	0.12
	Panel B: Has other job in telephone industry?							
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 and Control Worker Pair 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	0.03	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	0.01	0.01
	Panel C: Is telephone operator in other industry?							
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 and Control Worker Pair 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	0.04	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	0.01	0.01

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. (2020), 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 V: 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 and Control Worker Pair 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	0.41	0.41	0.35	0.35	0.42	0.42	0.57	0.57
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 and Control Worker Pair 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	0.71	0.71	0.79	0.79	0.69	0.69	0.43	0.43
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 and Control Worker Pair 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	0.51	0.51	0.59	0.59	0.49	0.49	0.28	0.28

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. (2020), 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 VI: 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)								
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 and Control Worker Pair 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	0.28	0.28	0.21	0.21	0.29	0.29	0.39	0.39
Panel B: Log occupation score								
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 and Control Worker Pair 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	2.1	2.1	2.0	2.0	2.1	2.1	2.1	2.1
Panel C: Decline in occupation score decile								
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 and Control Worker Pair 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	0.22	0.22	0.23	0.23	0.21	0.21	0.19	0.19

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. (2020), 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 VII: Changes in work, education, marriage, and fertility patterns around cutovers

Panel A: White, American-born women ages 16 to 25

	Percent of the group that is:				
	Tel. oper.	Working	In school	Married	Has children
Post-cutover	-0.66***	0.03	0.12	0.08	0.22
	(0.05)	(0.43)	(0.25)	(0.30)	(0.21)
N	113752	113752	113752	113752	113752
R^2	0.42	0.83	0.95	0.95	0.93
Cities	2845	2845	2845	2845	2845
Cut over	261	261	261	261	261
Y Mean	1.15	40.35	21.30	34.92	19.85

Panel B: White, American-born women ages 16 to 20

	Percent of the group that is:				
	Tel. oper.	Working	In school	Married	Has children
Post-cutover	-0.75***	-0.01	0.25	-0.04	0.24*
	(0.08)	(0.57)	(0.46)	(0.25)	(0.12)
N	56884	56884	56884	56884	56884
R^2	0.45	0.86	0.92	0.90	0.85
Cities	2845	2845	2845	2845	2845
Cut over	261	261	261	261	261
Y Mean	1.21	37.09	38.49	16.57	7.28

Panel C: White, American-born women ages 21 to 25

	Percent of the group that is:				
	Tel. oper.	Working	In school	Married	Has children
Post-cutover	-0.57***	0.08	-0.01	0.20	0.21
	(0.05)	(0.46)	(0.13)	(0.38)	(0.31)
N	56868	56868	56868	56868	56868
R^2	0.36	0.74	0.75	0.82	0.79
Cities	2845	2845	2845	2845	2845
Cut over	261	261	261	261	261
Y Mean	1.09	43.66	3.85	53.55	32.60

Notes: Tables present staggered difference-in-difference estimates, by age, of the effects of dial cutovers on the percent 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 percent 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 VIII: Changes in employment shares in select occupations around cutovers

Panel A: White, American-born women ages 16 to 25

	Conditional on working, percent employed as or in							
	Tel. oper.	Off. mach.	Typist/secr.	Office clerk	Sales clerk	Beautician	Waitress	Ln(Occscore)
Post-cutover	-1.51***	0.05*	0.52**	-0.26	0.08	0.12**	0.81***	-0.010**
	(0.12)	(0.03)	(0.25)	(0.20)	(0.21)	(0.06)	(0.20)	(0.005)
N	111485	111485	111485	111485	111485	111485	111485	110671
R^2	0.37	0.42	0.61	0.53	0.41	0.46	0.49	0.76
Cities	2845	2845	2845	2845	2845	2845	2845	2845
Cut over	261	261	261	261	261	261	261	261
Y Mean	2.93	0.14	11.61	4.57	9.82	1.04	4.15	1.88

Panel B: White, American-born women ages 16 to 20

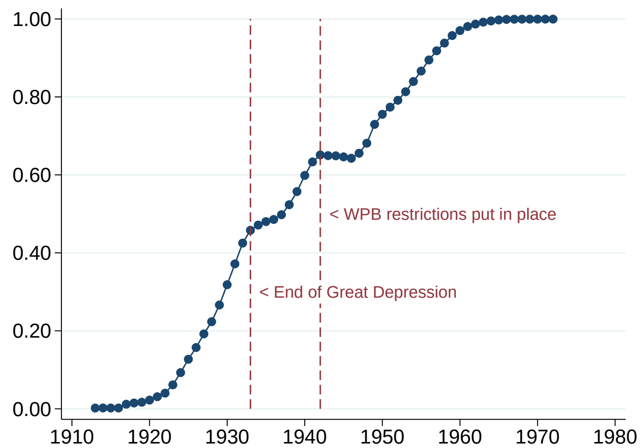
	Conditional on working, percent employed as or in							
	Tel. oper.	Off. mach.	Typist/secr.	Office clerk	Sales clerk	Beautician	Waitress	Ln(Occscore)
Post-cutover	-1.88***	0.03	0.49	-0.21	0.18	0.12	1.20***	-0.019***
	(0.20)	(0.02)	(0.30)	(0.22)	(0.32)	(0.08)	(0.27)	(0.006)
N	54997	54997	54997	54997	54997	54997	54997	54421
R^2	0.38	0.37	0.61	0.51	0.42	0.41	0.46	0.72
Cities	2845	2845	2845	2845	2845	2845	2845	2845
Cut over	261	261	261	261	261	261	261	261
Y Mean	3.27	0.10	9.77	4.27	10.55	0.71	4.62	1.78

Panel C: White, American-born women ages 21 to 25

	Conditional on working, percent employed as or in							
	Tel. oper.	Off. mach.	Typist/secr.	Office clerk	Sales clerk	Beautician	Waitress	Ln(Occscore)
Post-cutover	-1.16***	0.06*	0.55**	-0.31	-0.02	0.11	0.44**	-0.002
	(0.09)	(0.04)	(0.28)	(0.21)	(0.18)	(0.07)	(0.17)	(0.004)
N	56488	56488	56488	56488	56488	56488	56488	56250
R^2	0.32	0.47	0.58	0.55	0.38	0.48	0.55	0.58
Cities	2845	2845	2845	2845	2845	2845	2845	2845
Cut over	261	261	261	261	261	261	261	261
Y Mean	2.60	0.18	13.47	4.87	9.10	1.38	3.68	1.99

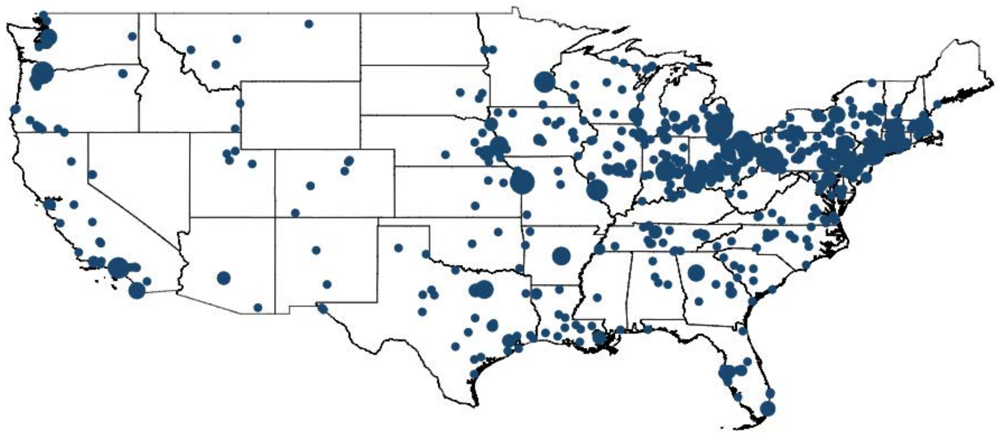
Notes: Tables present staggered difference-in-difference estimates, by age, of the effects of dial cutovers on young, white, American-born women's employment shares in select occupations, across successive cohorts, for cities with population $\leq$100k in 1920. The left-most column provides the effect of cutovers on the percent 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. The final column estimates effects on (log) average occupation scores. 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.

Figure I: 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 II: Cities in data with cutovers by 1940

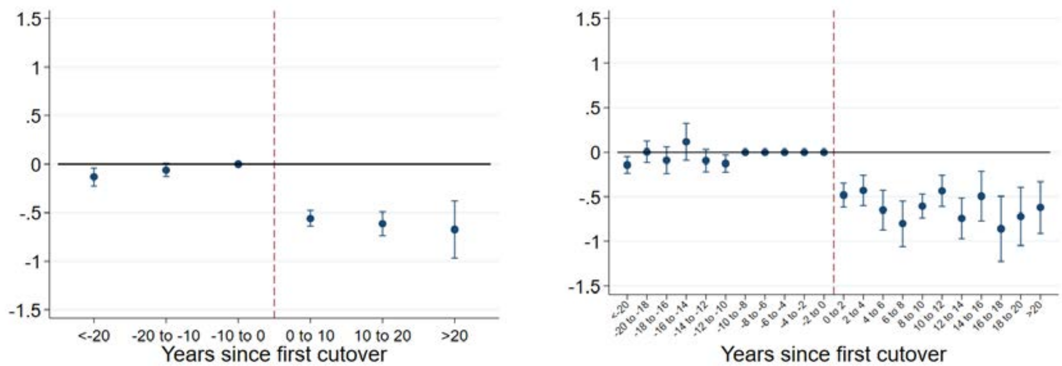


Notes: Figure maps the cities with a dial cutover in the AT&T and newspapers data through 1940. Bubble sizes are proportional to the # of reported cutovers through 1940.

Figure III: 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)

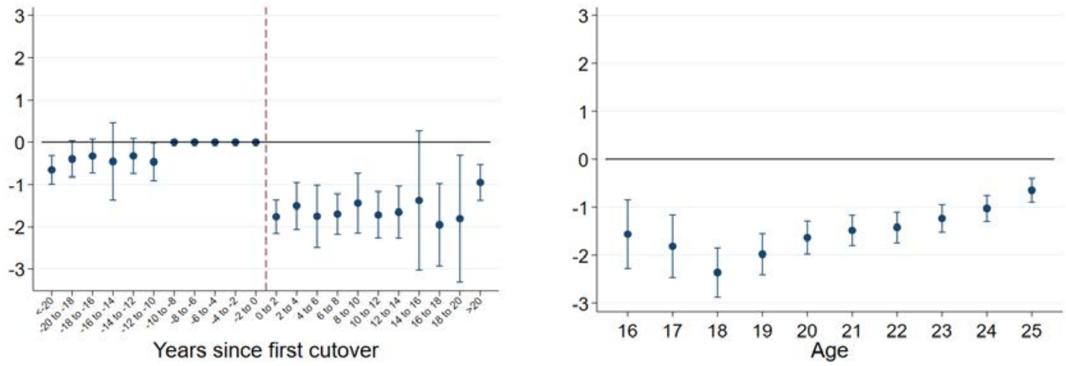


Notes: Figure shows event study estimates of the effects of dial cutovers on the (log) number of young, white, American-born women in successive cohorts who are telephone operators in the telephone industry (+1), for the small city sample (population ≤100k in 1920), with 10- and 2-year event windows. 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 IV: Effect of dial cutovers on the percent of working young, white, American-born women who are telephone operators in the telephone industry (event study and DID by age)

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

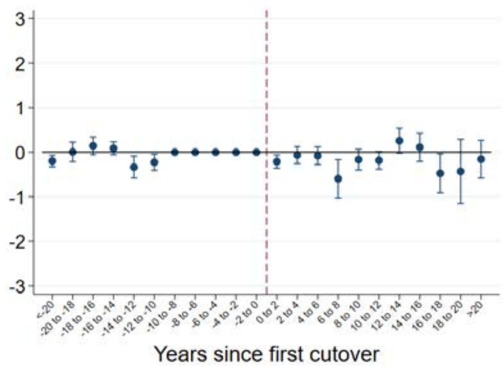
Panel (B)
Percent who are tel. operators (DID by age)



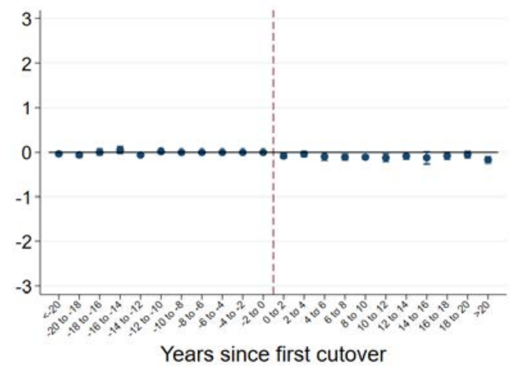
Notes: Figure shows event study and staggered difference-in-difference estimates (by age) of the effects of dial cutovers on the percent of working young, white, American-born women in successive cohorts who are telephone operators in the telephone industry, for the small city sample (population ≤100k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

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

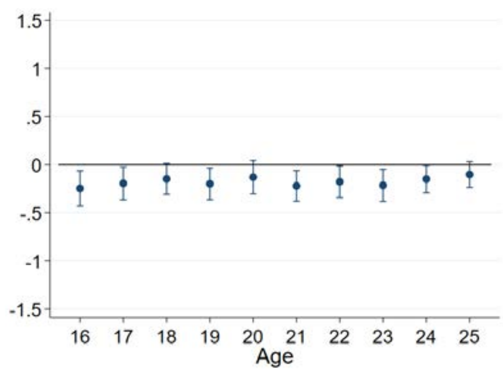
Panel (A)
Other jobs in the tel. industry (2-yr intervals)



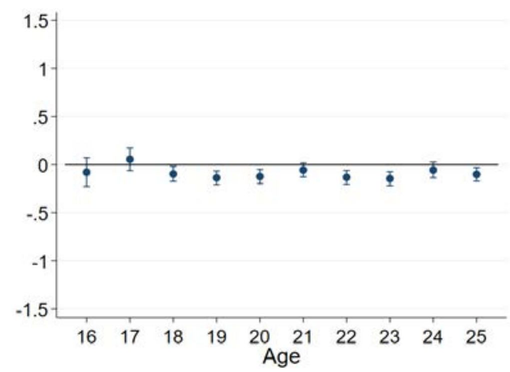
Panel (B)
Tel. operator in other industries (2-yr intervals)



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 dial cutovers on the percent 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 $\leq$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 staggered difference-in-differences estimates, by age. We plot the estimates on the same scale (-3 to 3 p.p.) as the previous figures to ease comparison. Error bars represent 95% confidence intervals, computed from SEs clustered at the city level.

Online Appendix

Answering the Call of Automation: How the Labor Market Adjusted to the Mechanization of Telephone Operation

James J. Feigenbaum and Daniel P. Gross

Appendix Table of Contents

A Historical Background

This appendix provides supplementary material to accompany the discussion of U.S. telephone industry history, mechanical switching, and the labor market for young women in Section 1. Table A.1 provides descriptive statistics on the U.S. telephone industry from 1902 to 1932. 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, and Figure A.4 shows newspaper articles discussing regulatory telephone rate changes around dial cutovers, which suggest cutovers were typically accompanied by telephone rate increases, justified by the capital expense.

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.

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	1930
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	3,368,089	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	6	4
Central office installation and maintenance men	13	13	29	19	18	25
All employees (all classes):						
Male	102	155	147	115	123	113
Female	307	297	213	179	166	160
Total	409	452	360	294	289	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: Before change to dial system	Number of operators: After change to dial system	Number of operators: If operators had increased in same ratio as calls	Loss in employment opportunities for operators: Estimated number	Loss in employment opportunities for operators: 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	[2] 75.5
No. 5	11.3	171.4	119	270	323	53	16.4
No. 6	100.0	13.8	158	63	180	120	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	190	[2] 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	[2] 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

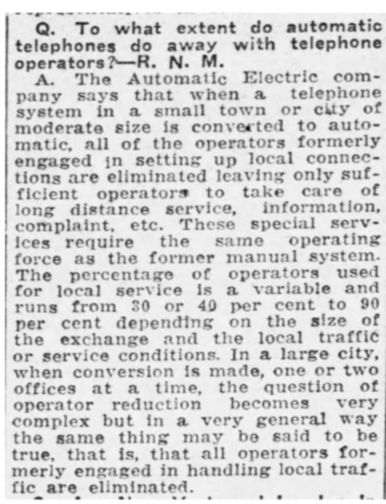
Operation of a dial system at the proposed new exchange building of the Mountain States Tel. & Tel Co. will not result in layoff of many telephone switchboard operators and may even bring about added employment, Frank D. Sawyer, district manager of the company, informed Mayor Fred M. Abbott today.

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

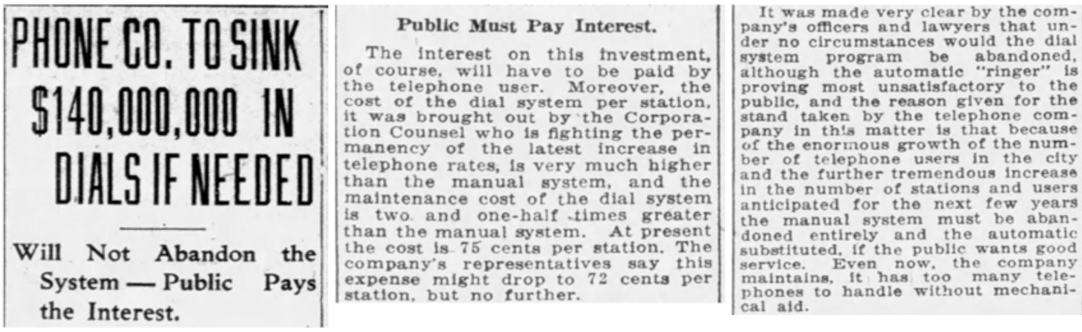
CUPID BEATS OUT OLD MAN HUNGER

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. Tweny-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: Newspaper reports of telephone rate changes around cutovers, 1925 to 1940



PHONE CO. TO SINK $140,000,000 IN DIALS IF NEEDED

Will Not Abandon the System — Public Pays the Interest.

Public Must Pay Interest.

The interest on this investment, of course, will have to be paid by the telephone user. Moreover, the cost of the dial system per station, it was brought out by the Corporation Counsel who is fighting the permanency of the latest increase in telephone rates, is very much higher than the manual system, and the maintenance cost of the dial system is two and one-half times greater than the manual system. At present the cost is 75 cents per station. The company's representatives say this expense might drop to 72 cents per station, but no further.

It was made very clear by the company's officers and lawyers that under no circumstances would the dial system program be abandoned, although the automatic "ringer" is proving most unsatisfactory to the public, and the reason given for the stand taken by the telephone company in this matter is that because of the enormous growth of the number of telephone users in the city and the further tremendous increase in the number of stations and users anticipated for the next few years the manual system must be abandoned entirely and the automatic substituted, if the public wants good service. Even now, the company maintains, it has too many telephones to handle without mechanical aid.

The Brooklyn Daily Eagle, Brooklyn, NY, 1925

Mayor Says Extra Dial Phone Charge Is Discriminatory

Asks Public Service Company to Abolish 25-Cent Monthly Payment.

SPECIAL DISPATCH TO THE GLOBE-DEMOCRAT.

JEFFERSON CITY, MO., January 16.—Complaint of Mayor Victor J. Miller of St. Louis against the Southwestern Bell Telephone Company over the dial system of operating private branch exchanges in the St. Louis exchange of the Bell was heard today by the State Public Service Commission. A number of witnesses for both the city and the company were heard today.

The complaint filed several months ago by Mayor Miller through the legal department of the city charges the rate of 25 cents each month for dial service is unwarranted and discriminatory and should be abolished by the commission. The city contends automatic service should be supplied without extra charge.

CITY LOSES ITS FIGHT ON DIAL PHONE CHARGE

Public Service Commission Turns Down Complaint on Monthly Fee of 25 Cents Per Phone in Private Boards.

LEVY WAS OPPOSED ON BASIS IT DISCRIMINATED

State Ruling Says Amount Is Just and Reasonable—Similar Billing Already Is in Force in 44 Other States.

St. Louis Globe Democrat, St. Louis, MO, 1928;
St. Louis Star and Times, St. Louis, MO, 1929

Figure A.4: Summary of select OSRD research programs (cont'd)

Dial Telephone System Is To Be Installed In Tyler

New Rates Planned

In agreeing to install the dial stem in Tyler, the telephone company officials are asking authority the City Commission to put a w rate schedule into effect, which ll call for an increase in the telephone rates, but which will make e rates here on about an even r with those charged in other ies where the dial telephone system is in operation.

Where the present rate for one-e business telephones is now $5 r month, the telephone company oposes to increase this rate to $6 r month. For one-party residence es, the present rate is $2.50 per nth and under the new proposed l system rate it would be $3 r month.

...

"NOW, THEREFORE, BE IT ORDAINED BY THE COMMISSION OF THE CITY OF TYLER:

"1. That the commission looks with favor upon any sound project or plan having for its purpose the upbuilding of the city or the improvement of conditions in the city, and hereby endorses the plan of the Southwestern Bell Telephone Company to enlarge and improve its property and service in Tyler by the construction and installation, as aforesaid, of a new modern dial telephone plant and system.

"2. That when the new buildi and system have been completed a are placed in operation, the com pany may place into effect a sche ule of rates providing for an i crease in the present one-party bus ness rate of $1; in the present on party residence rate of 50c, in t present two-party residence rate 25c; in the rate for business sem public service for messages over t guarantee of ¼c, and in the rate f private branch exchange trunks $1.50. All other rates and charg now being made by the compa shall continue in effect witho change.

The Tyler Courier-Times, Tyler, TX, 1939

stood crumbled and crashed.

Authorities here estimated that 75,000 homes, 42 per cent of those in an earthquake area of 2,400 square miles, had been destroyed in the big earthquake Tuesday night.

Dial Phone Change Approved By Board

Engineers for the state utilities commission reported favorably today for rate increases and installation of automatic dial equipment in nine communities, including West Lafayette and Conesville, served by the Ohio Bell Telephone Co.

Local officials and citizens have 30 days to file objections. The engineers said telephone subscribers in the communities have agreed to the higher rates if the automatic equipment is installed.

Other communities affected are Fletcher, Slineville, Lena, Chesterland, Bloomingville, Castalia and Christianburg.

The Cochocton Tribune, Coshocton, OH, 1939

Notes: Figure reproduces newspaper headlines and/or content from the following articles: "Phone Co. to Sink $140,000,000 in Dials If Needed," *The Brooklyn Daily Eagle* (Battle Creek, MI), November 22, 1925; "Mayor Says Extra Dial Phone Charge Is Discriminatory," *The St. Louis Globe-Democrat* (St. Louis, MO), January 17, 1928; "City Loses Its Fight on Dial Phone Charge" *The St. Louis Star and Times* (St. Luis, MO), July 18, 1929; "Dial Phone Change Approved By Board," *The Coshocton Tribune* (Coshocton, OH), January 30, 1939; "Dial Telephone System Is To Be Installed In Tyler" *The Tyler Courier-Times* (Tyler, TX), September 24, 1939. All articles accessed from Newspapers.com.

Employment of young women

We can also characterize the nature of work for young women in the period we study, vis-à-vis occupation, employment, and wage profiles, especially as we think about which occupations women might enter when telephone operation is obsolete. Table A.2 lists the most common occupations for young, white, American-born women from 1910 to 1940, sorted by rank in 1920, where we see that "telephone operators" was the 6th most-common job for this group in 1920, dropping to 11th by 1940, presumably as a result of the diffusion of mechanical switching.

Table A.2: Top occupations for white, American-born women age 16 to 25, 1910-1940

	Rank			
Occupation	1910	1920	1930	1940
Operative and kindred workers	1	1	2	1
Stenographers, typists, and secretaries	4	2	1	2
Salesmen and sales clerks	2	3	3	3
Clerical and kindred workers	10	4	4	5
Teachers	5	5	5	9
Telephone operators	8	6	7	11
Bookkeepers	7	7	8	7
Private household workers	3	8	6	4
Laborers	13	9	12	12
Waiters and waitresses	12	10	9	6

Notes: Table ranks the top 10 occupations employing white, American-born women age 16-25 in the U.S. in 1920, and shows their ranks in other years.

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".[1]

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 A.3 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 6 when we examine the post-cutover occupations of young women.

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

Table A.3: 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).

B Data Appendix

B.1 Data on dial cutover location and 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.

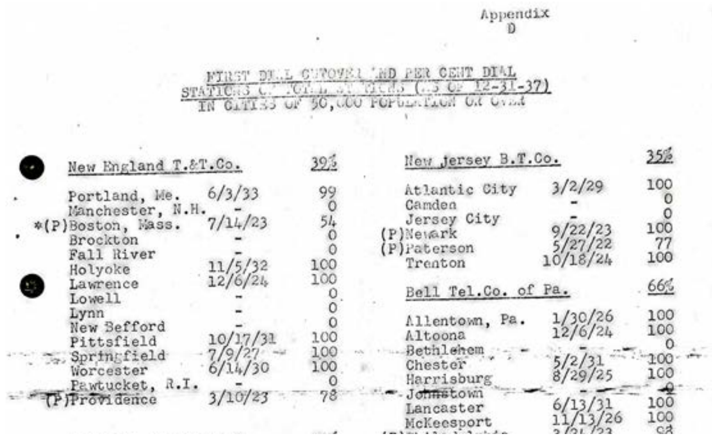
Data from AT&T's corporate archives

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.[2] 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).[3] 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.

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 below provides 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.

[2] 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.
[3] 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



Appendix
D

FIRST DIAL CUTOVER AND PER CENT DIAL
STATIONS [illegible] (AS OF 12-31-37)
IN CITIES OF 50,000 POPULATION OR OVER

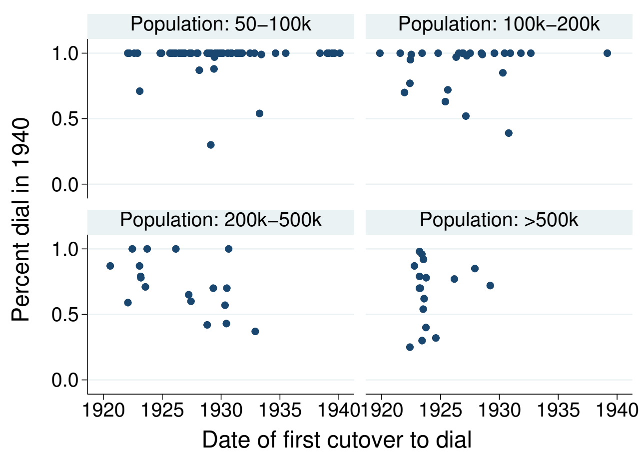
New England T.&T.Co.		39%
Portland, Me.	6/3/33	99
Manchester, N.H.	-	0
*(P)Boston, Mass.	7/14/23	54
Brockton	-	0
Fall River	-	0
Holyoke	11/5/32	100
Lawrence	12/6/24	100
Lowell	-	0
Lynn	-	0
New Bedford	-	0
Pittsfield	10/17/31	100
Springfield	7/9/27	100
Worcester	6/14/30	100
Pawtucket, R.I.	-	0
(P)Providence	3/10/23	78

New Jersey B.T.Co.		35%
Atlantic City	3/2/29	100
Camden	-	0
Jersey City	-	0
(P)Newark	9/22/23	100
(P)Paterson	5/27/22	77
Trenton	10/18/24	100

Bell Tel.Co. of Pa.		66%
Allentown, Pa.	1/30/26	100
Altoona	12/6/24	100
Bethlehem	-	0
Chester	5/2/31	100
Harrisburg	8/29/25	100
Johnstown	-	0
Lancaster	6/13/31	100
McKeesport	11/13/26	100

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).

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.

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.[4] 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")

(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[5] 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

[4] 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.

[5] 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.

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 thus treat it as the true date. We otherwise use the article language (e.g., "recent", "soon", "ongoing", mentions of specific calendar seasons) to approximate the month and 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.

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.[6]

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:

[6] 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.

			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 5 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.

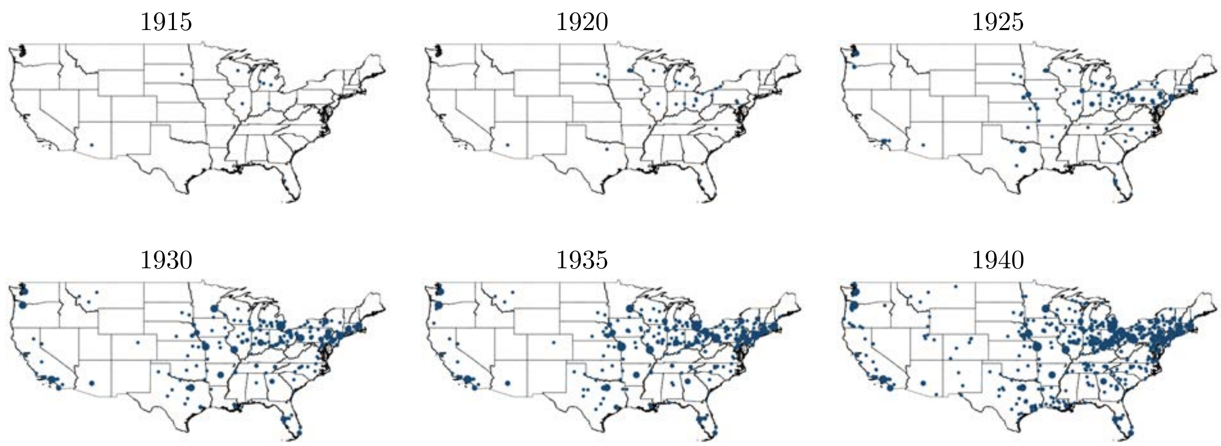
We aggregate these data up to the city x month level, identifying months in which each city was reported to have experienced a dial cutover, and henceforth we call each such city-month a "cutover" (we assume that when there are multiple reported cutovers in a given city in the same month, these are part 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.[7]

[7]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.

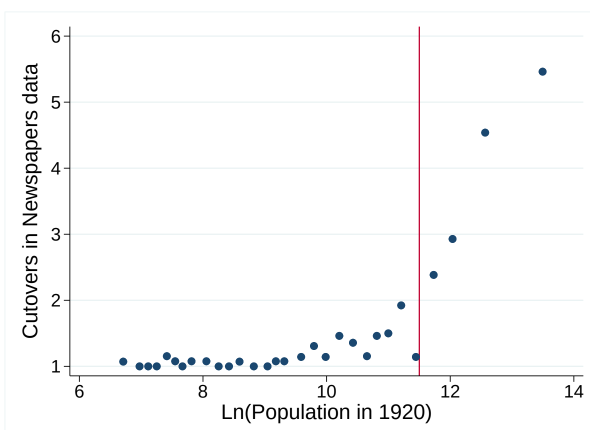
Among these, the average and median cutover took place in 1931. Figure B.3 below maps these cutovers, illustrating their expanding geographic incidence—which is the variation at the heart of this paper. Figure B.4 shows a binned scatterplot of a city's number of cutovers in the Newspapers data against 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.3: Newspapers city-level data: Expanding geography of dial, 1915 to 1940



Notes: Figure maps 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.

Figure B.4: Newspapers city-level data: Number of reported cutovers vs. 1920 population

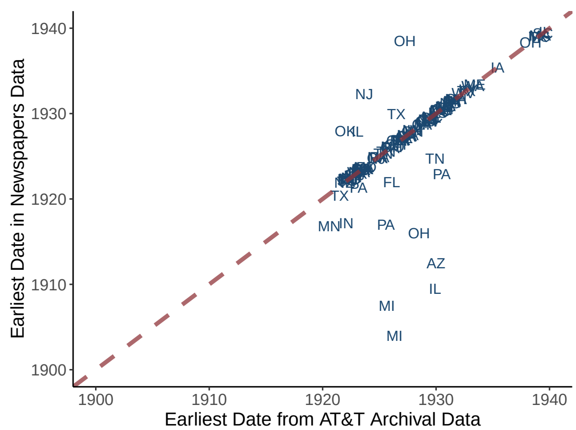


Notes: Figure shows a binned scatterplot of cities' number of reported cutovers, measured as the number of distinct months between 1919 and 1940 with a cutover reported in our Newspaper data, against log 1920 population, with a line drawn at 100k population (the log of which is ≈11.5). The figure illustrates 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.5 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.5: 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.[8] 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

[8]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.

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).[9] Our primary dataset aggregates these individuals in the complete count data up to the level of:

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.

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.

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

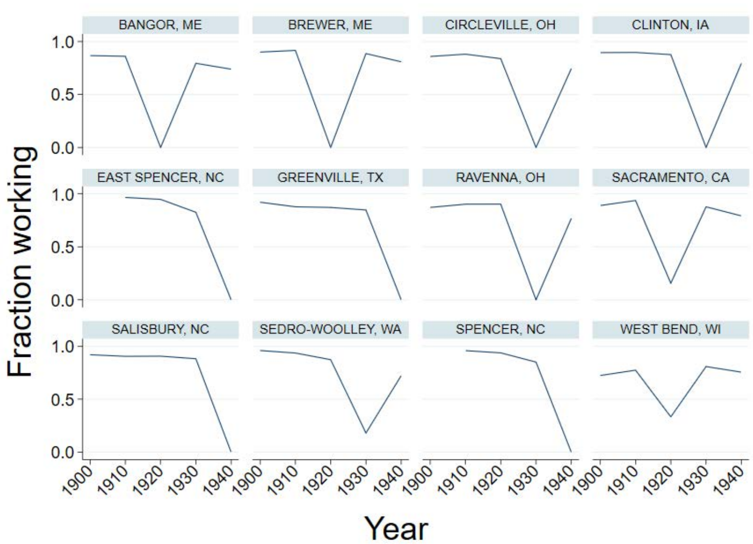
[9]The non-farm restriction is made for precision but has little bearing on our results: only 1% of the population in our sample cities are recorded as belonging to farm households in 1910, and <1% in later years.

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 $\leq$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.6 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.6: Examples of cities with outlier changes in the population with an 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.[10] However, as we showed in Table II, 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. 2021). 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 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

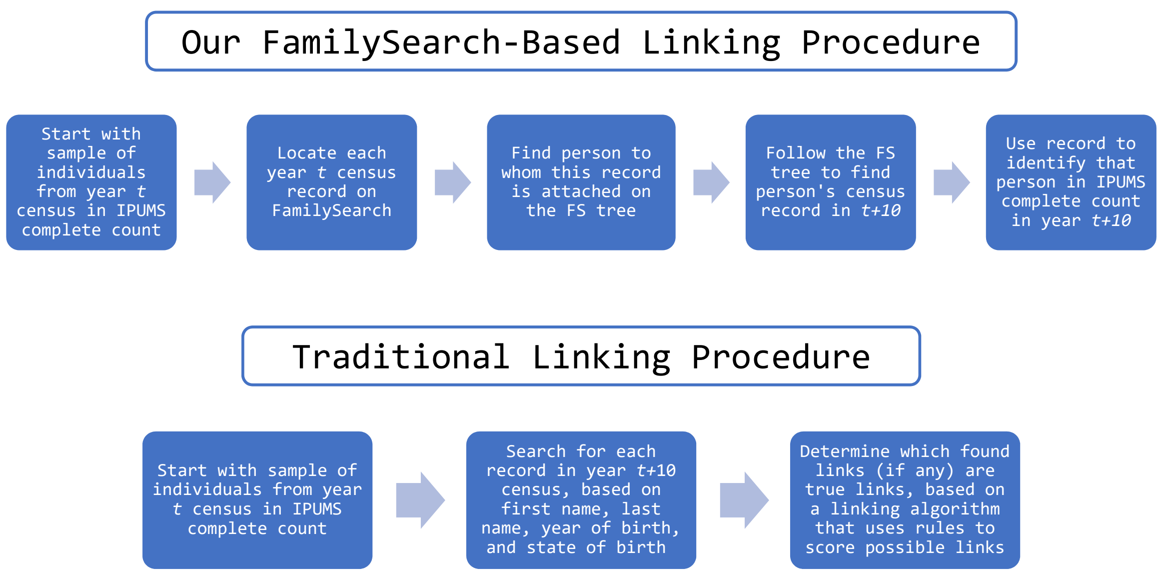
[10] For an overview of automated census linking, see Abramitzky et al. (2021).

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. Later in this appendix 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.7 we illustrate our procedure as it compares to traditional linking methods, and in Figure B.8 we document the specifics of our approach at each of the steps described below.

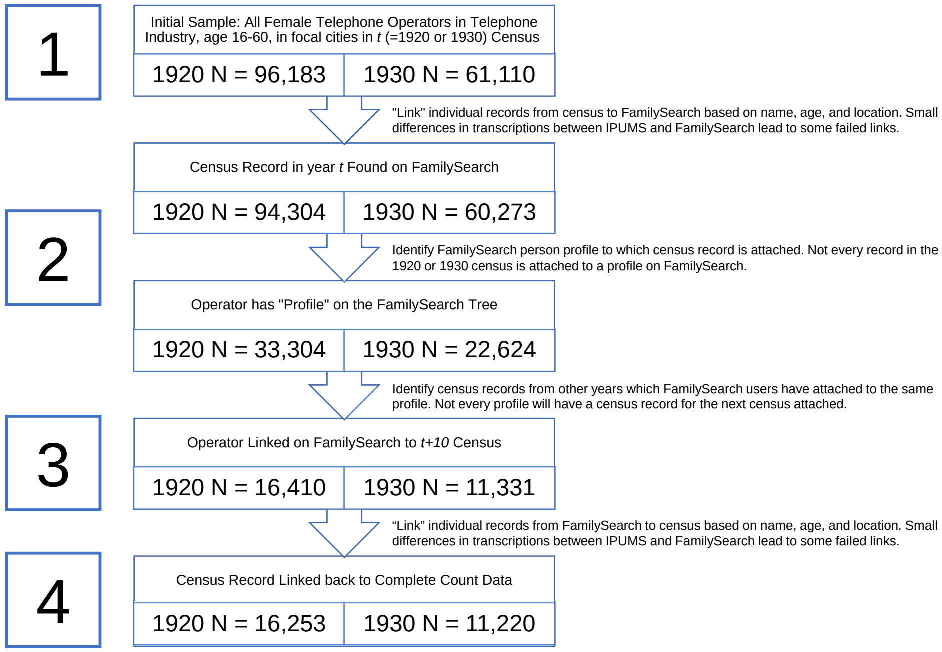
Figure B.7: FamilySearch-based versus Traditional Linking Procedures



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.[11] Second, we look for each telephone operator on FamilySearch, a public genealogy platform with an open wiki-style family tree (Price et al. 2021), where users create pages for deceased individuals—usually their own ancestors but not always—and attach links to historical records, including entries from Federal

[11] 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.

Figure B.8: Sampling at Each Stage of the FamilySearch-based Procedure



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.[12]

Third, we query the FamilySearch tree for links to the next census. That is, we begin with the set of operators 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.[13]

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

[12]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.

[13]Our link rates are high, and primarily reflect that once an operator has been matched to the FamilySearch tree, the tree will connect them to multiple census records. High link rates also reflect the depth of the FamilySearch tree and some of the advantages to a genealogical approach to record linking (Buckles et al. 2023). When we compare census link rates under existing linking methods, like those in the Census Linking Project (CLP; Abramitzky et al. 2020), to the product of our on-the-tree rate and our linked-to-the-next-census rate, we find that this product is similar to (if not slightly lower than) CLP link rates, albeit on a population (young women who might marry) that is substantially more challenging to link using traditional methods.

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. (2021) 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.

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. (2020) 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.[14]

Bailey et al. (2020) construct these weights in two steps, per footnote 33 of that paper:

1. Run a probit regression of link status (i.e., whether an individual is matched) on the following variables: an indicator for middle name; length of first, middle, and last names; polynomials in day of birth and age; an index for how common the first and last names are; whether or not one has siblings and number of siblings; and length of the names of one's parents.
2. Compute inverse propensity scores as $\frac{1-p}{p}\frac{m}{1-m}$, where p is the predicted likelihood of being matched based on the 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.[15] The IPWs account for any differential propensity to stay in or leave the sample being driven at any stage in the linking process.

[14] 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.

[15] 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.

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.
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. (2020) 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.
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. (2020) 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.

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[16] we also want to explore in more detail the representativeness of the tree.[17] We do so in this section, tracing out which operators in our initial

[16]We are not the first to propose using FamilySearch data. Price et al. (2021) 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. (2022) study the correlation between longevity and education using FamilySearch links as well.

[17]An initial reassurance is Buckles et al. (2023b), who show that father-son pairs measured in FamilySearch and using traditional linking methods have similar rates of intergenerational mobility.

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

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.

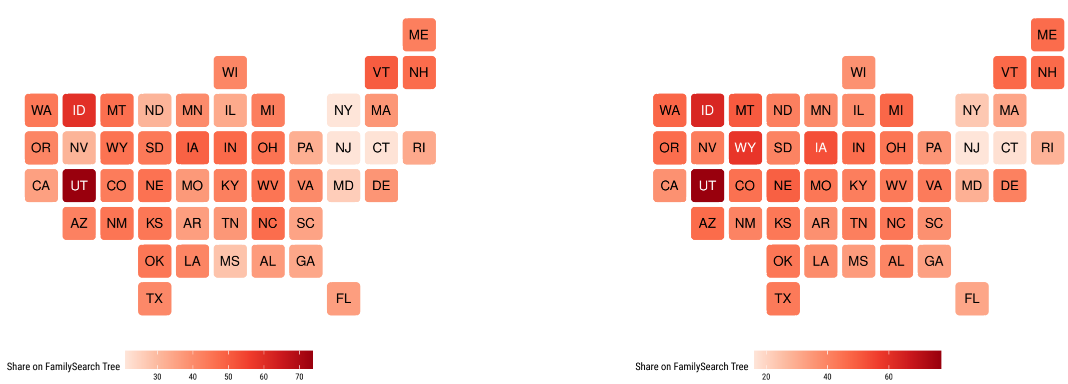
Of the 157,293 telephone operators we went looking for on the FamilySearch tree, we found 55,928 (35.6%) on the tree. As Table B.2 shows, the share-on-the-tree is very similar for operators in 1920 and in 1930. Why are some operators on the tree and others are not? One simple reason is that the operators 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.9 and Figure B.10, 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.

Figure B.9: Share of Telephone Operators on the FamilySearch Tree by State of Birth

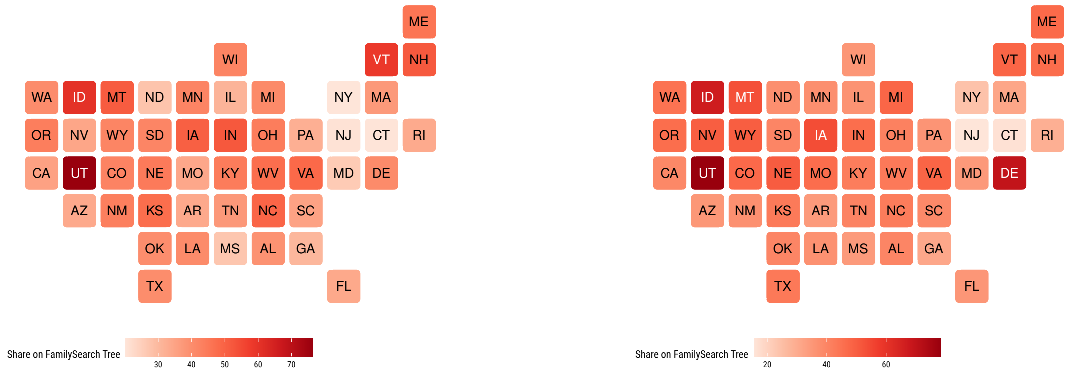


(a) 1920

(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.10: Share of Telephone Operators on the FamilySearch Tree by State of Residence

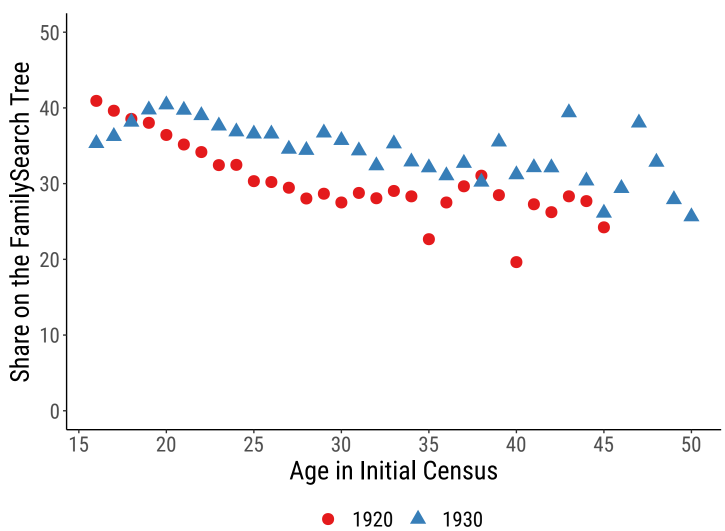


(a) 1920 (b) 1930

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.11. 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.

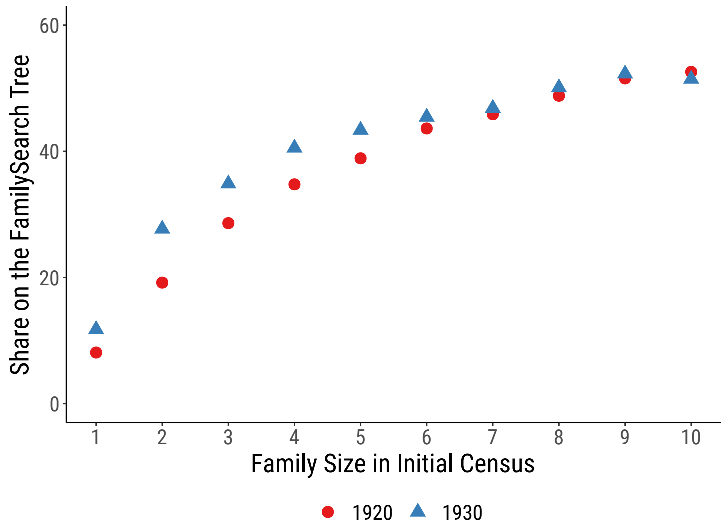
Figure B.11: Share of Telephone Operators on the FamilySearch Tree by Age



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.12. In both 1920 and 1930, the share of women operators on the tree who were in households alone is quite low (<10% in 1920 and about 12% in 1930). This rises monotonically with family size in both censuses.[18]

Figure B.12: Share of Telephone Operators on the FamilySearch Tree by Family Size

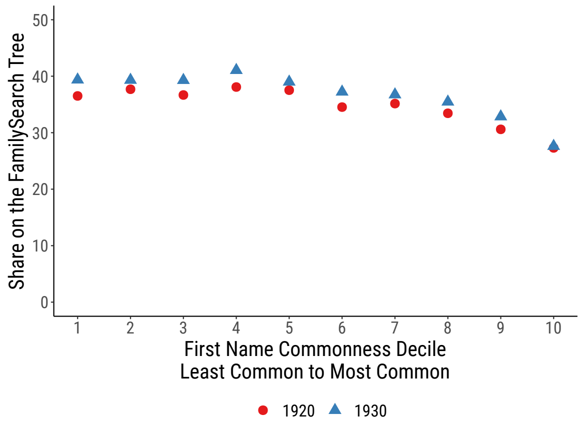


Note: We plot the share of telephone operators who were found on the tree in the same census we identified them as telephone operators in the telephone industry according to the size of their family in their household in the initial census. Specifically, we use the IPUMS variable `famsize`.

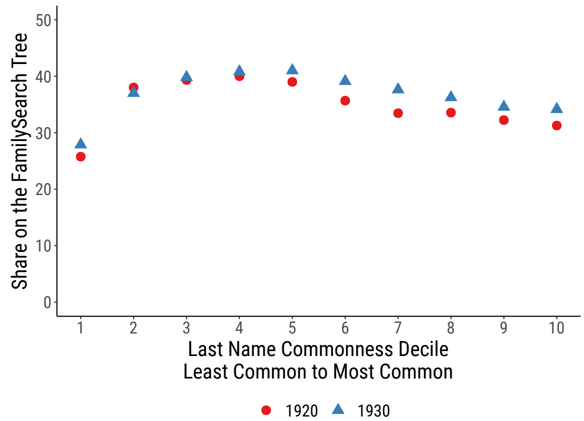
Name commonness is a pervasive challenge for census linking, whether by hand or by automated methods (Abramitzky et al. 2021; Bailey et al. 2020). When there are multiple people with the same name, humans and algorithms can struggle to determine which is the correct person and which is the doppelganger. 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.13a, 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.

[18]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.13: Share of Telephone Operators on the FamilySearch Tree by Name Commonness



(a) Commonness of First Name

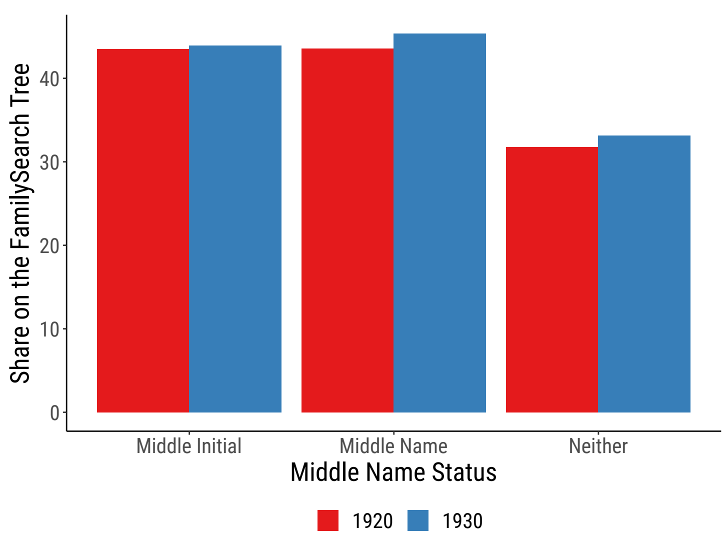


(b) Commonness of Last Name

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.14: Share of Telephone Operators on the FamilySearch Tree by Middle Name Status



Note: Not every person enumerated in the complete count censuses report a middle name or middle initial. Whether or not someone reports this extra identifying information affects the ease of linking them, census to census or to other records on FamilySearch. 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 whether or not they report a middle name or middle initial or neither.

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 an insignificant 0.8 p.p.

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

	On the FamilySearch Tree	Linked to Next Census
	(1)	(2)
Cutover Previous Decade	0.003 (0.007)	0.008 (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.

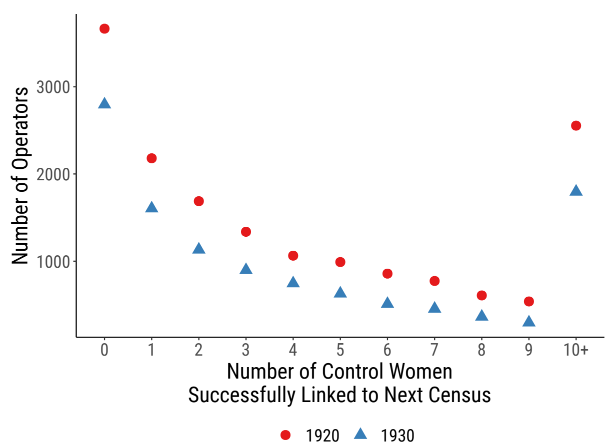
As we document in Table B.4, we locate approximately 21 control women for each telephone operator in 1920 and 1930. We then follow the linking procedure outlined in Appendix Subsection B.3.1 to locate these control women on FamilySearch and find them in the following census. Of 459,786 matched control women, 191,761 are on the tree (41% versus 36% for operators).[19] 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

	Control Women		On the Tree		Linked to Next Census	
Year	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.15). 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.

Figure B.15: Control Women Linked to Next Census per Telephone Operator



[19]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".

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 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 I).

County- and city-level heterogeneity

We use a number of additional data sources when we investigate heterogeneity in Appendix C. In addition to the complete count census data, we incorporate data from the Census of Agriculture, to measure county-level household electrification (although technically for farms, we believe it a useful proxy for local electrification generally); from the American Federation of Labor (AFL), to measure city-level AFL organizers (AFL 1919); and from Fishback et al. (2005) on county per-capita retail sales growth from 1929 to 1933, to measure local severity of the Great Depression. When these variables are available only at the county level, we assign to each city in our city sample values of the surrounding county. When cities span county boundaries, we compute weighted averages, weighting by the city population in each associated county.

C Robustness Checks

C.1 Characteristics of cities with earlier and later cutovers

Table C.1 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. Consistent with Table III of the paper, 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.[20] Table III of the paper shows that in a multivariate regression, nearly all of the explanatory power loads onto population, with t-stats of around 20. That market size and scale economies explain the adoption of mechanical switching is consistent with our findings in concurrent work exploring the drivers and frictions in AT&T's automation of the telephone network (Feigenbaum and Gross 2023).

Table C.1: Mean 1910 characteristics of cities by timing of earliest cutover

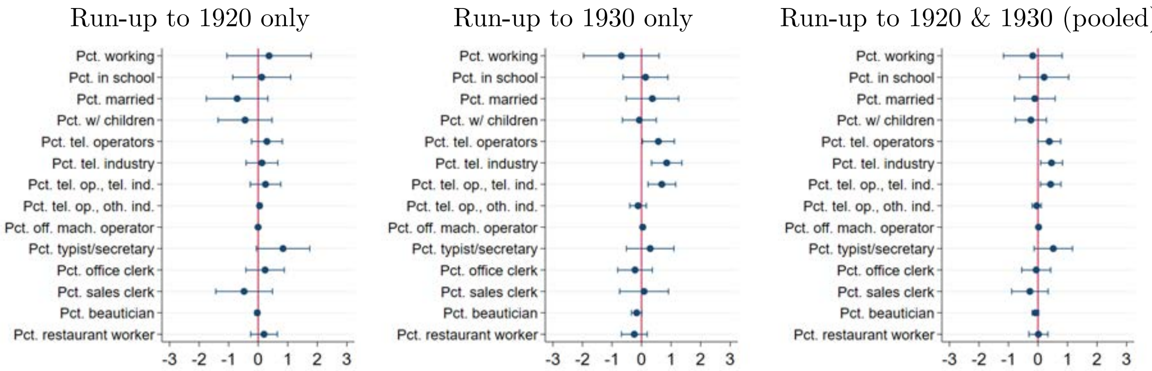
Characteristic	pre-1920	1921-1925	1926-1930	1931-1935	1936-1940	post-1940
Population 16+ (1000s)	38.92	116.82	43.87	18.41	9.14	4.06
	(55.49)	(248.98)	(80.23)	(27.30)	(13.33)	(6.68)
Average age	27.93	27.97	28.15	28.32	27.70	27.75
	(2.07)	(2.06)	(2.31)	(2.38)	(2.80)	(3.15)
Percent female	48.46	50.08	48.94	50.08	50.03	50.34
	(4.25)	(3.04)	(5.66)	(5.33)	(4.54)	(5.61)
Percent f/n/w/y	12.25	11.62	11.46	11.74	11.96	12.32
	(2.43)	(2.30)	(2.61)	(2.21)	(2.57)	(2.99)
Percent working	60.54	60.35	60.81	59.60	58.96	57.55
	(5.27)	(5.05)	(5.69)	(5.64)	(5.83)	(7.28)
Percent operators	0.19	0.21	0.19	0.17	0.19	0.21
	(0.10)	(0.12)	(0.14)	(0.11)	(0.11)	(0.15)
F/n/w/y percent working	41.17	40.68	40.23	44.01	36.71	35.09
	(7.79)	(12.09)	(10.32)	(11.86)	(12.31)	(12.12)
F/n/w/y percent operators	1.16	1.36	1.19	1.02	1.12	1.21
	(0.65)	(1.09)	(0.87)	(0.67)	(0.79)	(0.97)
Unionized by 1920	0.17	0.26	0.19	0.09	0.08	0.03
	(0.38)	(0.44)	(0.40)	(0.29)	(0.28)	(0.18)
Had strike by 1920	0.07	0.10	0.09	0.03	0.03	0.01
	(0.26)	(0.30)	(0.28)	(0.17)	(0.18)	(0.11)
Observations	29	62	114	67	60	2660

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). 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.

[20] We measure local unions in 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).

C.2 Pre-treatment balance tests

Figure C.1: Pre-treatment run-up balance tests for the outcomes studied throughout the paper

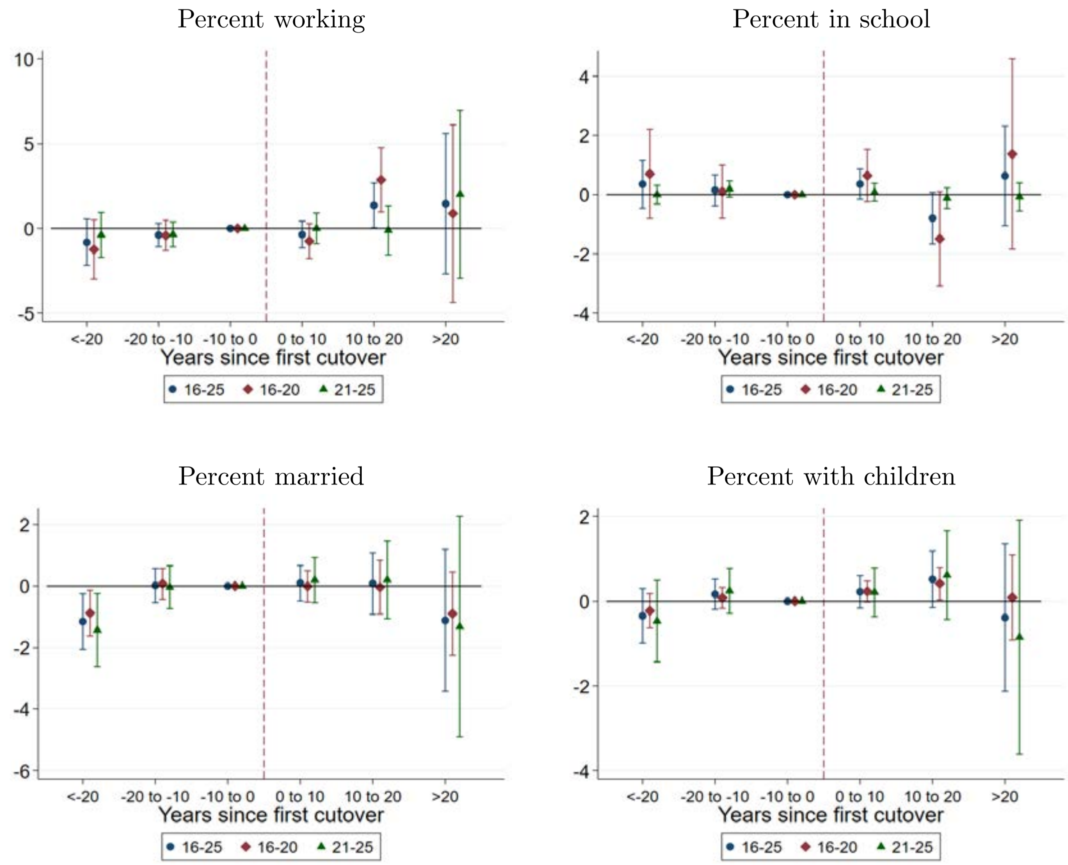


Notes: Figures estimate differences in the prior 10-year *changes* in each of the variables shown across cities with versus without an imminent cutover. For example, the left panel examines cities which in 1920 had not yet had a cutover in our data, calculates the 1910 to 1920 change in a given outcome (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 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 $\leq$100k in 1920. Error bars represent 95% confidence intervals, computed from SEs clustered at the city level.

C.3 Event studies for labor market outcomes, by age

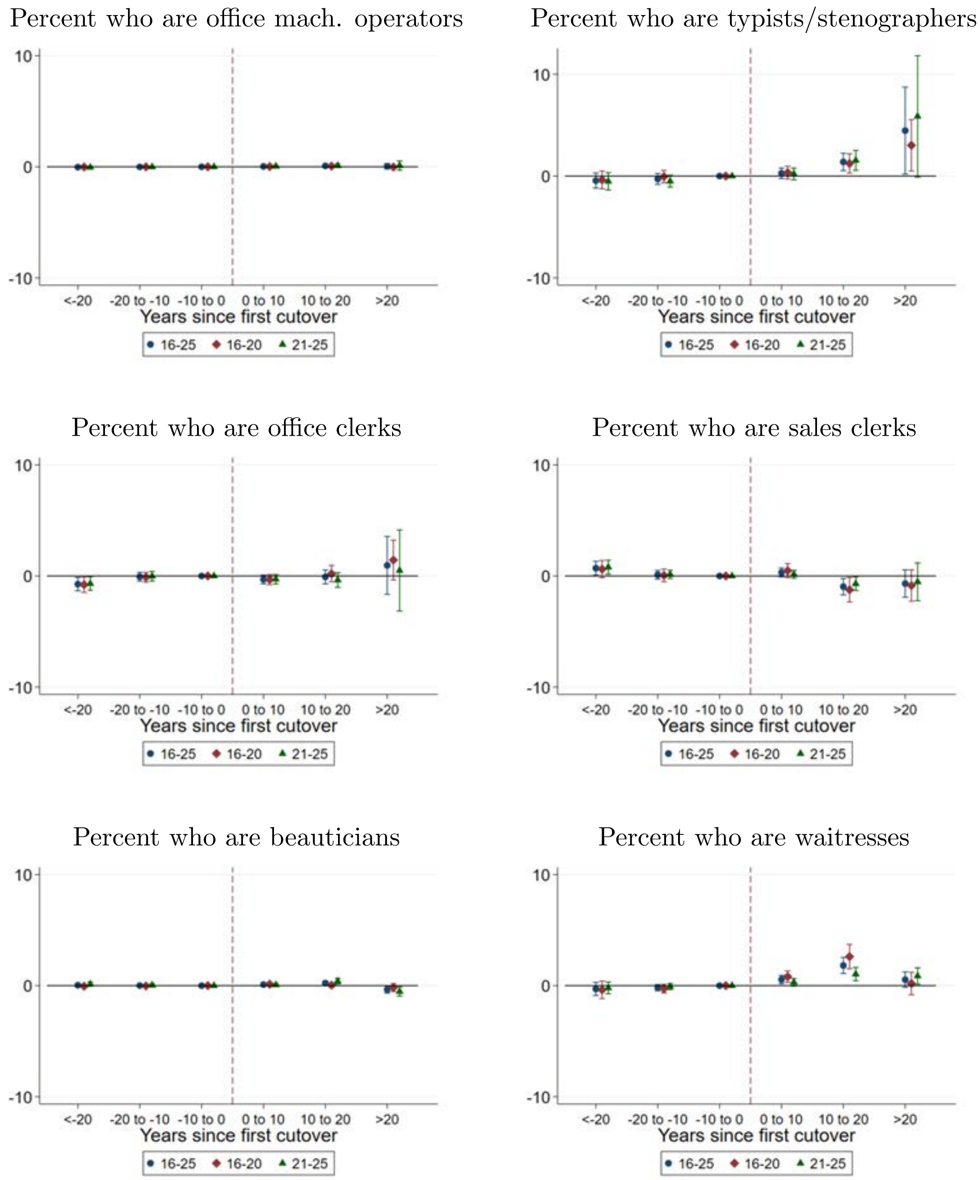
In Figure C.2 and C.3, we show the event study estimates for labor market outcomes of successive cohorts of young, white, American-born women around cutovers, splitting the population into the same age groups as in Tables VII and VIII: 16 to 25, 16 to 20, and 21 to 25.

Figure C.2: 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 dial cutovers on the percent of young, white, American-born women in successive cohorts who are working, 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 C.3: Changes in
employment shares in select occupations for young, white, American-born women around cutovers

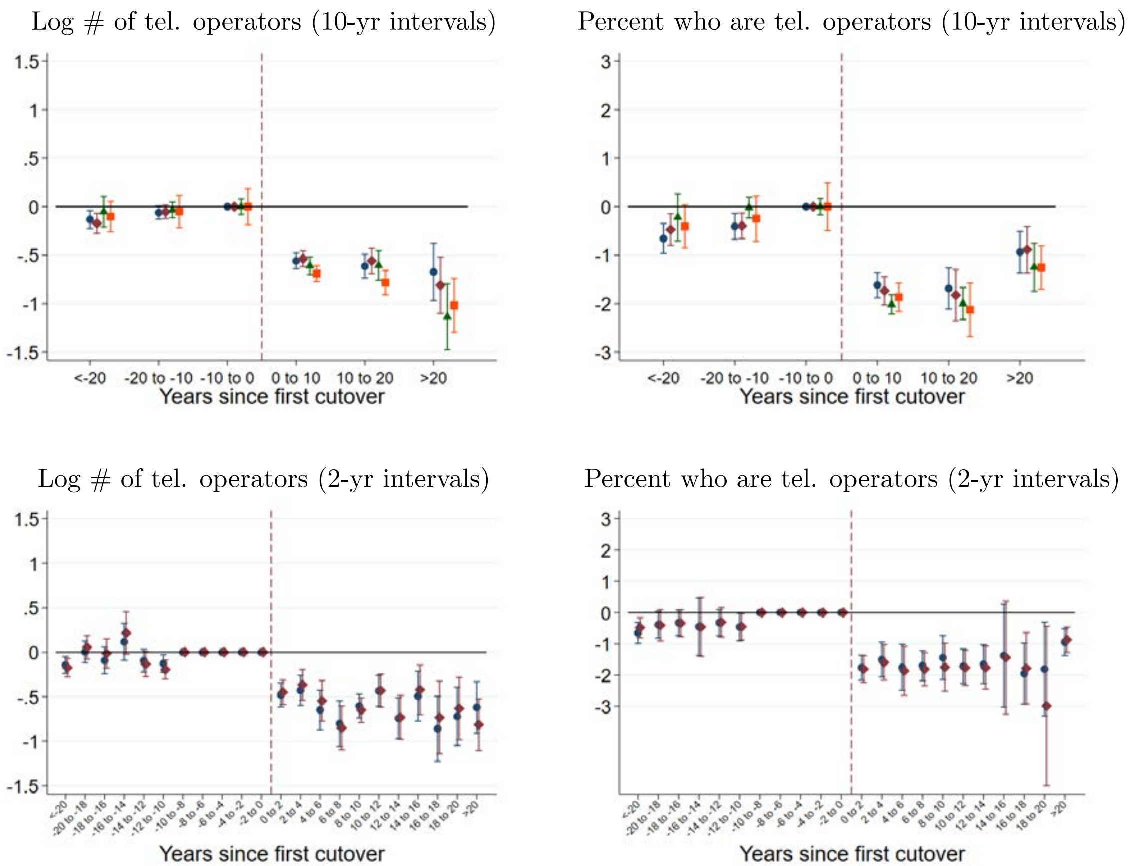


Notes: Figure shows event study estimates, by age, of the effects of dial cutovers on young, white, American-born women's employment shares in select occupations, across successive cohorts, for cities with population $\leq$100k in 1920. Error bars represent 95% confidence intervals, computed from SEs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

C.4 Robust event study estimation methods

Appendix Figure C.4 presents a robustness check on the event study results in Figures III and IV 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 C.4: Effect of dial cutovers on the log number of young, white, American-born women and percent of the same who are telephone operators in the telephone industry (robust event study)



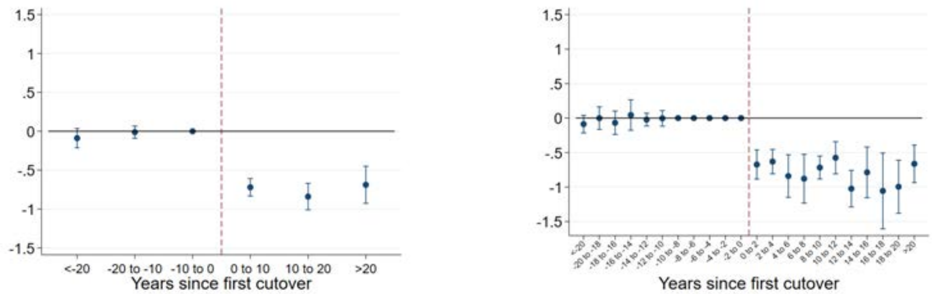
Notes: Left panels show event study estimates of the effects of dial cutovers on the (log) number of young, white, American-born women in successive cohorts who are telephone operators in the telephone industry (+1), for the small city sample (population ≤100k in 1920), with 10- and 2-year event windows. 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 percent 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 (2021) (red diamond), (iii) Callaway and Sant'Anna (2021) (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.

C.5 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 C.5 to C.7, and Tables C.2 to C.6, are counterparts to Figures III to V, and Tables IV to VIII, in the body of the paper.

Figure C.5: Effect of dial cutovers on the log number of young, white, American-born women who are telephone operators in the telephone industry (event study)
sample restricted to cities with continuous newspaper coverage from 1917 to 1940

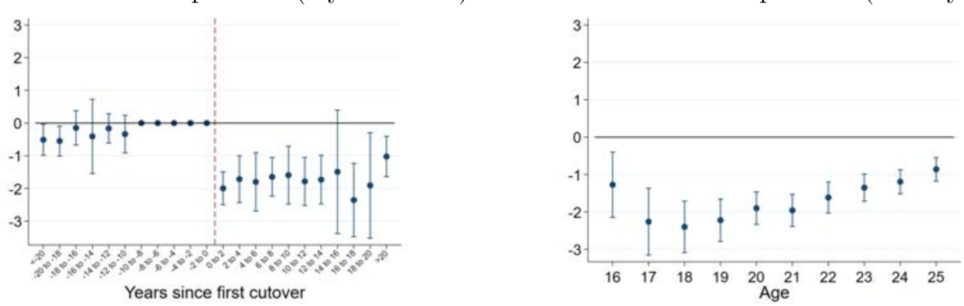
Panel (A) Log # of tel. operators (10-yr intervals) — Panel (B) Log # of tel. operators (2-yr intervals)



Notes: Figure shows event study estimates of the effects of dial cutovers on the (log) number of young, white, American-born women in successive cohorts who are telephone operators in the telephone industry (+1), 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 C.6: Effect of dial cutovers on the percent of working young, white, American-born women who are telephone operators in the telephone industry (event study and DID by age)
sample restricted to cities with continuous newspaper coverage from 1917 to 1940

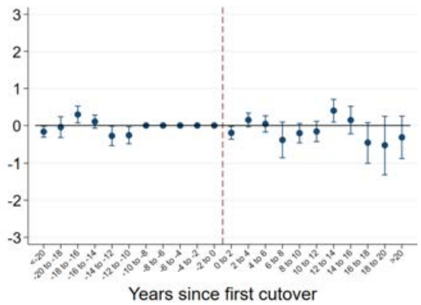
Panel (A) Percent who are tel. operators (2-yr intervals) — Panel (B) Percent who are tel. operators (DID by age)



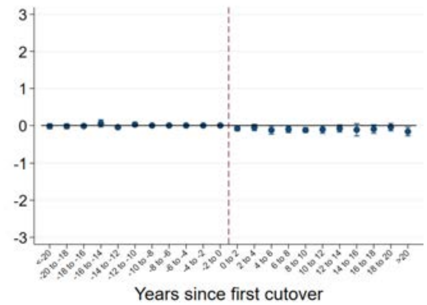
Notes: Figure shows event study and staggered difference-in-difference estimates (by age) of the effects of dial cutovers on the percent of working young, white, American-born women in successive cohorts who are telephone operators in the telephone industry, for the small city sample (population ≤100k 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 C.7: Effect of dial cutovers on the percent of working young, white, American-born women with other jobs in the telephone industry or who are telephone operators in other industries
sample restricted to cities with continuous newspaper coverage from 1917 to 1940

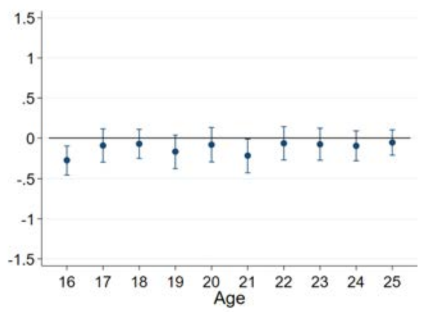
Panel (A)
Other jobs in the tel. industry (2-yr intervals)



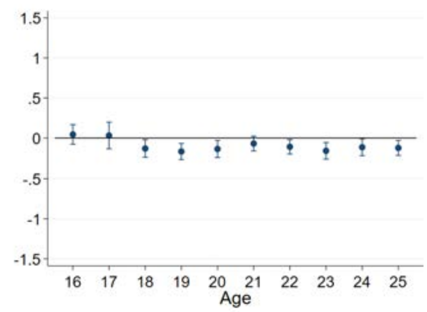
Panel (B)
Tel. operator in other industries (2-yr intervals)



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 dial cutovers on the percent 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 staggered DID estimates, by age. We plot the estimates on the same scale (-3 to 3 p.p.) as the previous figures to ease comparison. Error bars represent 95% confidence intervals, computed from SEs clustered at the city level.

Table C.2: Effects of dial cutovers on the probability of being a telephone operator or having a non-operator job in the telephone industry

sample restricted to cities with continuous newspaper coverage from 1917 to 1940

	All Ages		16-20		21-25		26+	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Is telephone operator in telephone industry?								
Operator × Post-cutover	-0.058***	-0.055***	-0.038***	-0.033**	-0.051**	-0.068***	-0.086***	-0.070*
	(0.015)	(0.016)	(0.013)	(0.015)	(0.020)	(0.023)	(0.027)	(0.036)
Operator	0.217***	0.213***	0.159***	0.152***	0.222***	0.236***	0.344***	0.351***
	(0.009)	(0.009)	(0.009)	(0.010)	(0.014)	(0.016)	(0.019)	(0.023)
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City × Year FEs	Yes	No	Yes	No	Yes	No	Yes	No
Operator and Control Worker Pair FEs	No	Yes	No	Yes	No	Yes	No	Yes
Observations	88,973	88,973	44,500	44,500	34,116	34,116	10,357	10,357
Adjusted R^2	0.17	0.41	0.12	0.34	0.17	0.41	0.27	0.52
Dependent variable mean	0.04	0.04	0.03	0.03	0.03	0.03	0.10	0.10
Panel B: Has other job in telephone industry?								
Operator × Post-cutover	0.005	0.007	0.007	0.011**	0.005	0.004	0.005	0.013
	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.012)	(0.016)
Operator	0.023***	0.021***	0.015***	0.014***	0.021***	0.020***	0.049***	0.042***
	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.008)	(0.009)
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City × Year FEs	Yes	No	Yes	No	Yes	No	Yes	No
Operator and Control Worker Pair FEs	No	Yes	No	Yes	No	Yes	No	Yes
Observations	88,973	88,973	44,500	44,500	34,116	34,116	10,357	10,357
Adjusted R^2	0.03	0.30	0.007	0.21	0.02	0.28	0.06	0.41
Dependent variable mean	0.008	0.008	0.007	0.007	0.007	0.007	0.02	0.02
Panel C: Is telephone operator in other industry?								
Operator × Post-cutover	0.001	0.002	-0.003	-0.001	0.011*	0.016**	0.002	0.002
	(0.005)	(0.006)	(0.007)	(0.006)	(0.006)	(0.007)	(0.011)	(0.013)
Operator	0.025***	0.026***	0.022***	0.022***	0.018***	0.016***	0.042***	0.039***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.008)	(0.011)
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City × Year FEs	Yes	No	Yes	No	Yes	No	Yes	No
Operator and Control Worker Pair FEs	No	Yes	No	Yes	No	Yes	No	Yes
Observations	88,973	88,973	44,500	44,500	34,116	34,116	10,357	10,357
Adjusted R^2	0.04	0.30	0.05	0.26	0.04	0.24	0.07	0.52
Dependent variable mean	0.006	0.006	0.005	0.005	0.005	0.005	0.01	0.01

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, and to cities with continuous newspaper coverage from 1917 to 1940. 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. (2020), 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 C.3: Effects of dial cutovers on the probability of working, getting married, or having children
sample restricted to cities with continuous newspaper coverage from 1917 to 1940

	All Ages		16-20		21-25		26+	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel A: Still working?				
Operator × Post-cutover	-0.036**	-0.029*	-0.034	-0.016	0.000	-0.016	-0.075***	-0.087**
	(0.015)	(0.015)	(0.023)	(0.023)	(0.021)	(0.026)	(0.028)	(0.037)
Operator	0.012	0.010	0.009	0.000	-0.016	-0.005	0.048**	0.063**
	(0.009)	(0.010)	(0.013)	(0.015)	(0.016)	(0.020)	(0.022)	(0.027)
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City × Year FEs	Yes	No	Yes	No	Yes	No	Yes	No
Operator and Control Worker Pair FEs	No	Yes	No	Yes	No	Yes	No	Yes
Observations	88,973	88,973	44,500	44,500	34,116	34,116	10,357	10,357
Adjusted R^2	0.06	0.09	0.03	0.09	0.04	0.08	0.10	0.15
Dependent variable mean	0.40	0.40	0.36	0.36	0.42	0.42	0.57	0.57
				Panel B: Got married? (cond'l on unmarried in pre-period)				
Operator × Post-cutover	0.014	0.001	0.030	0.006	-0.033*	-0.027	0.054	0.077*
	(0.012)	(0.013)	(0.020)	(0.019)	(0.019)	(0.027)	(0.036)	(0.046)
Operator	0.037***	0.041***	0.025**	0.033**	0.059***	0.056***	0.015	0.019
	(0.009)	(0.009)	(0.013)	(0.013)	(0.013)	(0.021)	(0.027)	(0.035)
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City × Year FEs	Yes	No	Yes	No	Yes	No	Yes	No
Operator and Control Worker Pair FEs	No	Yes	No	Yes	No	Yes	No	Yes
Observations	83,538	83,538	43,899	43,899	32,054	32,054	7,585	7,585
Adjusted R^2	0.09	0.13	0.05	0.12	0.04	0.08	0.10	0.13
Dependent variable mean	0.71	0.71	0.78	0.78	0.69	0.69	0.44	0.44
				Panel C: Had children? (cond'l on none in pre-period)				
Operator × Post-cutover	0.016	0.011	0.017	0.004	-0.005	0.006	0.039	0.029
	(0.015)	(0.018)	(0.022)	(0.025)	(0.021)	(0.028)	(0.026)	(0.031)
Operator	0.015	0.013	0.023	0.028*	0.022	0.000	-0.017	-0.037*
	(0.011)	(0.012)	(0.016)	(0.016)	(0.016)	(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 and Control Worker Pair FEs	No	Yes	No	Yes	No	Yes	No	Yes
Observations	87,027	87,027	44,430	44,430	33,657	33,657	8,940	8,940
Adjusted R^2	0.06	0.11	0.03	0.10	0.04	0.09	0.10	0.16
Dependent variable mean	0.51	0.51	0.58	0.58	0.49	0.49	0.28	0.28

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, and to cities with continuous newspaper coverage from 1917 to 1940. 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. (2020), 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 C.4: Effects of dial cutovers on the probability of persisting in the same occupation/industry and future occupation scores
sample restricted to cities with continuous newspaper coverage from 1917 to 1940

	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)							
Operator × Post-cutover	-0.075***	-0.091***	-0.097***	-0.103***	-0.076*	-0.142***	-0.062	-0.041
	(0.024)	(0.028)	(0.032)	(0.039)	(0.040)	(0.046)	(0.042)	(0.073)
Operator	0.290***	0.307***	0.304***	0.315***	0.301***	0.330***	0.278***	0.282***
	(0.014)	(0.017)	(0.019)	(0.026)	(0.028)	(0.035)	(0.029)	(0.042)
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City × Year FEs	Yes	No	Yes	No	Yes	No	Yes	No
Operator and Control Worker Pair FEs	No	Yes	No	Yes	No	Yes	No	Yes
Observations	36,014	36,014	15,858	15,858	14,250	14,250	5,906	5,906
Adjusted R^2	0.09	0.16	0.10	0.20	0.08	0.11	0.11	0.10
Dependent variable mean	0.25	0.25	0.19	0.19	0.27	0.27	0.35	0.35
	Panel B: Log occupation score							
Operator × Post-cutover	-0.057***	-0.078***	-0.065***	-0.074**	-0.065***	-0.078**	-0.024	-0.017
	(0.013)	(0.018)	(0.020)	(0.033)	(0.023)	(0.031)	(0.021)	(0.030)
Operator	0.076***	0.095***	0.111***	0.134***	0.050***	0.070***	0.068***	0.078***
	(0.011)	(0.012)	(0.014)	(0.025)	(0.017)	(0.018)	(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 and Control Worker Pair FEs	No	Yes	No	Yes	No	Yes	No	Yes
Observations	31,870	31,870	13,711	13,711	12,784	12,784	5,375	5,375
Adjusted R^2	0.09	0.15	0.14	0.17	0.09	0.13	0.12	0.09
Dependent variable mean	2.1	2.1	2.0	2.0	2.1	2.1	2.1	2.1
	Panel C: Decline in occupation score decile							
Operator × Post-cutover	0.079***	0.086***	0.071**	0.074*	0.097***	0.152***	0.049	0.024
	(0.021)	(0.025)	(0.029)	(0.040)	(0.037)	(0.048)	(0.038)	(0.053)
Operator	0.035**	0.011	0.064***	0.038	0.023	0.003	0.006	-0.021
	(0.014)	(0.017)	(0.022)	(0.032)	(0.028)	(0.033)	(0.028)	(0.037)
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City × Year FEs	Yes	No	Yes	No	Yes	No	Yes	No
Operator and Control Worker Pair FEs	No	Yes	No	Yes	No	Yes	No	Yes
Observations	26,802	26,802	11,294	11,294	10,818	10,818	4,690	4,690
Adjusted R^2	0.05	0.11	0.07	0.15	0.06	0.12	0.04	0.08
Dependent variable mean	0.22	0.22	0.23	0.23	0.22	0.22	0.21	0.21

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, and to cities with continuous newspaper coverage from 1917 to 1940. 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. (2020), 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 C.5: 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 25

		Percent of the group that is:			
	Tel. oper.	Working	In school	Married	Has children
Post-cutover	-0.78***	-0.31	-0.13	0.05	0.01
	(0.06)	(0.58)	(0.34)	(0.34)	(0.24)
N	20664	20664	20664	20664	20664
R^2	0.52	0.88	0.97	0.97	0.96
Cities	517	517	517	517	517
Cut over	137	137	137	137	137
Y Mean	1.18	41.68	20.44	35.60	19.65

Panel B: White, American-born women ages 16 to 20

		Percent of the group that is:			
	Tel. oper.	Working	In school	Married	Has children
Post-cutover	-0.85***	-0.35	-0.20	-0.10	0.18
	(0.10)	(0.76)	(0.61)	(0.31)	(0.15)
N	10332	10332	10332	10332	10332
R^2	0.56	0.90	0.94	0.94	0.91
Cities	517	517	517	517	517
Cut over	137	137	137	137	137
Y Mean	1.28	38.57	37.37	17.24	7.36

Panel C: White, American-born women ages 21 to 25

		Percent of the group that is:			
	Tel. oper.	Working	In school	Married	Has children
Post-cutover	-0.71***	-0.28	-0.06	0.21	-0.15
	(0.06)	(0.57)	(0.17)	(0.44)	(0.36)
N	10332	10332	10332	10332	10332
R^2	0.45	0.81	0.83	0.88	0.86
Cities	517	517	517	517	517
Cut over	137	137	137	137	137
Y Mean	1.07	44.73	3.84	53.59	31.69

Notes: Tables present staggered difference-in-difference estimates, by age, of the effects of dial cutovers on the percent of young, white, American-born women in successive cohorts who are working, 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 percent 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 C.6: 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 25

	Conditional on working, percent employed as or in							100*
	Tel. oper.	Off. mach.	Typist/secr.	Office clerk	Sales clerk	Beautician	Waitress	Ln(Occscore)
Post-cutover	-1.71***	0.04	0.44	-0.19	0.57*	0.08	0.59**	-1.51**
	(0.15)	(0.03)	(0.31)	(0.24)	(0.29)	(0.07)	(0.25)	(0.61)
N	20430	20430	20430	20430	20430	20430	20430	20346
R^2	0.46	0.51	0.73	0.59	0.49	0.56	0.60	0.84
Cities	517	517	517	517	517	517	517	517
Cut over	137	137	137	137	137	137	137	137
Y Mean	2.84	0.16	12.69	4.59	10.23	1.04	4.38	189.23

Panel B: White, American-born women ages 16 to 20

	Conditional on working, percent employed as or in							100*
	Tel. oper.	Off. mach.	Typist/secr.	Office clerk	Sales clerk	Beautician	Waitress	Ln(Occscore)
Post-cutover	-2.02***	0.02	0.57	-0.24	0.72*	0.12	0.87**	-2.41***
	(0.26)	(0.03)	(0.40)	(0.27)	(0.43)	(0.08)	(0.34)	(0.89)
N	10126	10126	10126	10126	10126	10126	10126	10064
R^2	0.48	0.47	0.72	0.55	0.50	0.44	0.56	0.80
Cities	517	517	517	517	517	517	517	517
Cut over	137	137	137	137	137	137	137	137
Y Mean	3.24	0.11	10.53	4.29	11.13	0.70	4.81	179.62

Panel C: White, American-born women ages 21 to 25

	Conditional on working, percent employed as or in							100*
	Tel. oper.	Off. mach.	Typist/secr.	Office clerk	Sales clerk	Beautician	Waitress	Ln(Occscore)
Post-cutover	-1.40***	0.05	0.32	-0.13	0.42*	0.04	0.32	-0.64
	(0.12)	(0.04)	(0.34)	(0.27)	(0.25)	(0.09)	(0.23)	(0.53)
N	10304	10304	10304	10304	10304	10304	10304	10282
R^2	0.40	0.53	0.69	0.64	0.44	0.61	0.68	0.68
Cities	517	517	517	517	517	517	517	517
Cut over	137	137	137	137	137	137	137	137
Y Mean	2.44	0.20	14.80	4.89	9.36	1.37	3.96	198.63

Notes: Tables present staggered difference-in-difference estimates, by age, of the effects of dial cutovers on young, white, American-born women's employment shares in select occupations, across successive cohorts, for cities with population $\leq$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 percent 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. The final column estimates effects on (log) average occupation scores. 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.

C.6 Controlling for expected demand growth

A possibility not yet ruled out is that automation was endogenous to future labor demand growth (e.g., Dechezleprêtre et al. 2021)—which threatens identification in estimating its effects on employment rates. The risk is that automation investments are made in response to rising labor costs, and growing economy-wide labor demand can push up wages and also soften the impacts of automating of any particular job. In this case, (predictable) labor demand growth is an omitted variable that correlates positively with both treatment and outcome.

To address this possibility, we would like to control for a measure of expected labor demand growth. We take two approaches to doing so. Our primary approach is to construct a time-varying Bartik instrument for expected (next-decade) employment growth, interacting cities' base year industry shares with national (leave-one-out) industry growth rates. Although employment growth is not the same as demand growth, we think it is a reasonable proxy, and the two should correlate as long as labor supply is not inelastic. We calculated this measure for all workers, for female workers, and for young, American-born female workers (our focal demographic). The results we present here focus on the latter, since this is the group that AT&T's automation decisions were sensitive to, but results are robust to this choice. We construct a shift-share control for next-decade employment growth in two ways, which we label "levels" and "percentiles":

1. The first is a standard Bartik, taking cities' industry shares in 1910, 1920, 1930, and 1940, and interacting them with 1910-20, 1920-30, 1930-40, and 1940-50 nationwide leave-one-out industry growth rates. Because we do not yet have complete count data for 1950, we use the IPUMS 1% sample to calculate 1940-50 nationwide industry growth rates (in this case, not leave-one-out, because the 1% sample does not have city recorded).

2. We then take this instrument and converted it to percentiles, recognizing that there may be more information in bins—which compress outliers—than in levels.

In unreported testing, we find that these measures do correlate significantly with cities' next-decade cutovers—albeit only mildly, and with far less explanatory power than city population has in predicting cutovers (see Table III of the paper). As we show in Table C.7 below, however, controlling for these instruments does not change our basic results. The reason, as we determined in additional analysis, is that these instruments do not predict local employment growth very well, especially for narrow groups like the ones AT&T hired telephone operators from—which can be idiosyncratic against national trends. An additional reason why these instruments do a poor job of predicting employment growth may also be that in the context of the Great Depression, it is intrinsically hard to predict 10-year ahead employment growth in 1920 and 1930.

Given how hard it is to predict employment growth with the wealth of data we have at our disposal, a reasonable question is whether an AT&T manager could ever have a hope of doing so. To permit more flexibility, we run regressions controlling for the instrument's underlying variation, interacting cities' industry mix in a base year (1910) with observation years (1910-1940). For tractability, we group up IPUMS industry codes to more aggregated SIC sectors (agriculture, construction, manufacturing, etc.), following IPUMS (which provides these categories), and control for sector shares of cities' young, white, American-born women's employment in 1910, interacted with observation years. This alternative control also has no effect on our results.

Table C.7: Post-cutover changes in work, estimating effects by decade

	Percent who are tel. oper., controlling for:				Percent who are working, controlling for:			
	Baseline	Bartik, lev.	Bartik, pct.	Ind. mix	Baseline	Bartik, lev.	Bartik, pct.	Ind. mix
Post-cutover	-0.66***	-0.66***	-0.66***	-0.66***	0.03	0.00	0.05	-0.10
	(0.05)	(0.05)	(0.05)	(0.05)	(0.43)	(0.43)	(0.43)	(0.39)
N	113684	113684	113684	113684	113684	113684	113684	113684
R^2	0.42	0.42	0.42	0.43	0.83	0.83	0.83	0.84
Cities	2843	2843	2843	2843	2843	2843	2843	2843
Cut over	261	261	261	261	261	261	261	261
Y Mean	1.15	1.15	1.15	1.15	40.35	40.35	40.35	40.35

Notes: Tables present staggered difference-in-difference estimates of the effects of dial cutovers on the percent of young, white, American-born women in successive cohorts who are (i) telephone operators in the telephone industry, and (ii) working, for cities with population $\leq$100k in 1920. Effects are estimated for a baseline specification (reproducing results from the body of the paper) and with assorted controls for expected employment growth. 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.

Together, this evidence suggests to us that the null effect of cutovers on young women's employment are unlikely to be confounded by endogenous automation. A key reason, by our reading, seems to be that changes in local labor demand are hard to predict.

C.7 Effects of cutovers by cutover decade

In this subsection, we estimate differential effects of cutovers by decade—in effect, breaking out our two-way fixed effects (TWFE) estimation into its constituent difference-in-differences (DID). Table C.8 reports results for two of our key outcomes: (i) the share of young, white, American-born women working as telephone operators, and (ii) the share working at all.

Columns (1) and (2) of this table estimate a TWFE specification over the full panel, interacting the post-cutover dummy with indicators for 1920s and 1930s cutovers. The baseline (reference) category comprises cities with cutovers pre-1920, of which there are a few. One feature to note is that standard errors increase substantially, since we have fewer cities in each treatment bin. Notwithstanding the increase in standard errors, we continue to see large, negative effects of cutovers on the share of this population working as telephone operators, with no statistical differences by cutover decade (Column 1). We also continue to see no evidence of changes in employment rates (Column 2). Although these comparisons are potentially somewhat underpowered, the baseline parameter (first row) is near zero, and the interactions with cutover decade (second and third rows) are still within confidence intervals, even using Table VII's standard errors. Columns (3) and (4) estimate a 1920-1930 specific DID, and Columns (5) and (6) the 1930 to 1940 DID. These columns again estimate the specification from the body of the paper (only now over a shorter panel). The results are economically and statistically similar to those in the paper.

Table C.8: Post-cutover changes in work, estimating effects by decade

	Full panel		1920-1930		1930-1940	
	Tel. oper.	Working	Tel. oper.	Working	Tel. oper.	Working
Post-cutover	-0.74***	0.01	-0.84***	-0.22	-0.68***	0.35
	(0.12)	(1.63)	(0.09)	(0.57)	(0.08)	(0.53)
* cutover in 1920s	0.02	0.23				
	(0.13)	(1.76)				
* cutover in 1930s	0.15	-0.21				
	(0.13)	(1.79)				
N	113752	113752	56696	56696	54366	54366
R^2	0.42	0.83	0.61	0.90	0.63	0.91
Cities	2845	2845	2836	2836	2719	2719
Cut over	261	261	117	117	135	135
Y Mean	1.15	40.35	1.50	41.91	1.07	39.49

Notes: Tables present staggered difference-in-difference estimates of the effects of dial cutovers on the percent of young, white, American-born women in successive cohorts who are (i) telephone operators in the telephone industry, and (ii) working, for cities with population ≤100k in 1920. Effects are estimated separately (i) by cutover decade (first two columns) and (ii) for the 1920-1930 and 1930-1940 (next four columns). 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.

C.8 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.

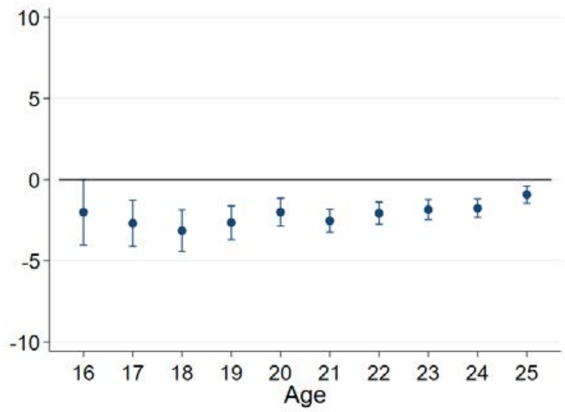
Specifically, we relate the AT&T data on the fraction of a city's subscribers with dial service as of 1940 to 1920-to-1940 long-differenced outcomes, as follows:

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

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.

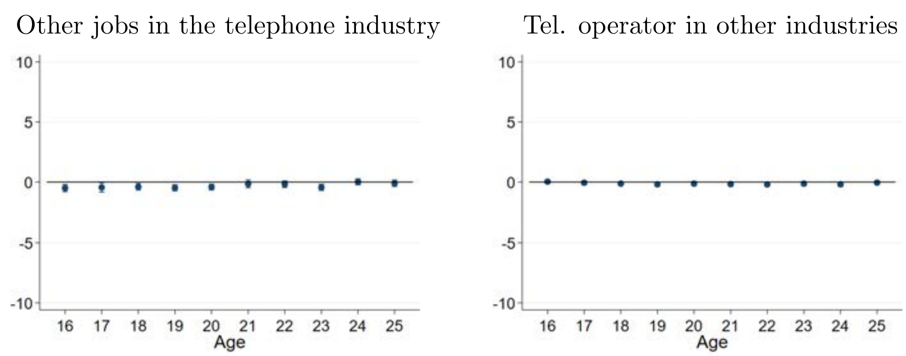
Results from these regressions are presented below, in a format analogous to the by-age difference-in-difference results in the paper. Figure C.8 estimates the effects on the percent of young, white, American-born women who are telephone operators in the telephone industry (for comparison to Figure IV in the body of the paper). Tables C.9 and C.10 estimate effects on the percent of women who are working, in school, married, have children, and are in assorted occupations (for comparison to Tables VII and VIII in the body of the paper).

Figure C.8: Effect of dial cutovers on the pct. of working young, white, American-born women in a given city who are telephone operators in the telephone industry (long differences, by age)
sample restricted to cities in the AT&T large-city data whose first cutover occurred post-1920



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 percent 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 C.9: Effect of dial cutovers on the pct. of working young, white, American-born women with other jobs in the telephone industry or who are telephone operators in other industries
sample restricted to cities in the AT&T large-city data whose first cutover occurred post-1920



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 percent 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 (-10 to 10 p.p.) as the previous figures to ease comparison. Error bars represent 95% confidence intervals, computed from robust SEs.

Table C.9: Changes in work, education, marriage, and fertility patterns around cutovers
sample restricted to cities in the AT&T large-city data whose first cutover occurred post-1920

Panel A: White, American-born women ages 16 to 25

	Percent of the group that is:				
	Tel. oper.	Working	In school	Married	Has children
Percent dial in 1940	-0.84***	0.44	-1.47**	-1.65	0.00
	(0.15)	(1.17)	(0.64)	(1.17)	(0.72)
N	1580	1580	1580	1580	1580
R^2	0.45	0.85	0.88	0.28	0.06
Y Mean	-1.64	-8.75	9.14	-0.27	-0.91

Panel B: White, American-born women ages 16 to 20

	Percent of the group that is:				
	Tel. oper.	Working	In school	Married	Has children
Percent dial in 1940	-0.83***	1.91	-2.79**	-1.15	-0.08
	(0.23)	(1.81)	(1.20)	(0.93)	(0.42)
N	790	790	790	790	790
R^2	0.20	0.75	0.81	0.09	0.10
Y Mean	-2.38	-22.86	17.66	-2.52	-0.55

Panel C: White, American-born women ages 21 to 25

	Percent of the group that is:				
	Tel. oper.	Working	In school	Married	Has children
Percent dial in 1940	-0.85***	-0.95	-0.24	-2.13	0.08
	(0.13)	(1.31)	(0.23)	(1.45)	(1.05)
N	790	790	790	790	790
R^2	0.23	0.11	0.30	0.16	0.03
Y Mean	-0.96	4.23	1.30	1.80	-1.25

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 percent of young, white, American-born women in successive cohorts who are working, 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 percent 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 C.10: Changes in employment shares in select occupations around cutovers
sample restricted to cities in the AT&T large-city data whose first cutover occurred post-1920

Panel A: White, American-born women ages 16 to 25

	Conditional on working, percent employed as or in							100*
	Tel. oper.	Off. mach.	Typist/secr.	Office clerk	Sales clerk	Beautician	Waitress	Ln(Occscore)
Percent dial in 1940	-2.16***	0.06	1.64	0.19	-0.17	0.27*	1.88***	-0.55
	(0.34)	(0.11)	(1.25)	(0.68)	(0.58)	(0.14)	(0.60)	(1.59)
N	1580	1580	1580	1580	1580	1580	1580	1580
R^2	0.28	0.33	0.31	0.03	0.24	0.32	0.09	0.57
Y Mean	-3.23	0.77	-0.84	1.92	1.20	1.86	5.37	-14.31

Panel B: White, American-born women ages 16 to 20

	Conditional on working, percent employed as or in							100*
	Tel. oper.	Off. mach.	Typist/secr.	Office clerk	Sales clerk	Beautician	Waitress	Ln(Occscore)
Percent dial in 1940	-2.51***	0.06	2.29	0.47	-0.57	0.13	2.10**	-1.17
	(0.56)	(0.08)	(1.40)	(0.72)	(0.85)	(0.10)	(0.84)	(2.14)
N	790	790	790	790	790	790	790	790
R^2	0.13	0.31	0.17	0.05	0.28	0.27	0.06	0.46
Y Mean	-4.42	0.48	-3.38	1.63	0.90	1.37	6.04	-19.97

Panel C: White, American-born women ages 21 to 25

	Conditional on working, percent employed as or in							100*
	Tel. oper.	Off. mach.	Typist/secr.	Office clerk	Sales clerk	Beautician	Waitress	Ln(Occscore)
Percent dial in 1940	-1.84***	0.06	1.04	-0.08	0.20	0.41**	1.66***	0.03
	(0.24)	(0.16)	(1.20)	(0.86)	(0.47)	(0.21)	(0.52)	(1.31)
N	790	790	790	790	790	790	790	790
R^2	0.22	0.07	0.04	0.00	0.05	0.04	0.08	0.06
Y Mean	-2.13	1.04	1.50	2.20	1.47	2.31	4.75	-9.11

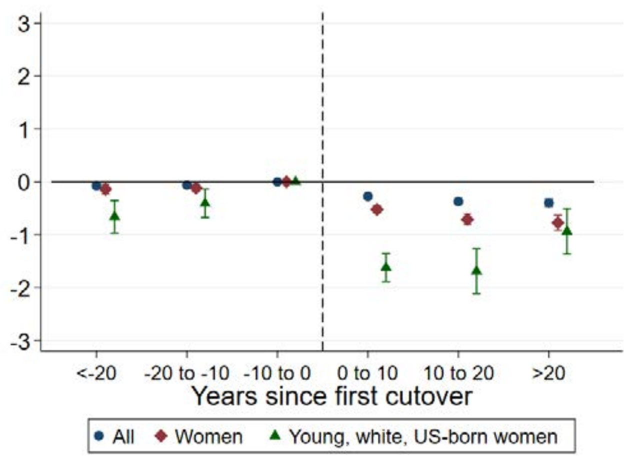
Notes: Tables present long differences estimates, by age, of the effects of local dial adoption (measured as the fraction of a city on dial as of the 1940 Census [April 1940]) on young, white, American-born women's employment shares in select occupations. Sample restricted to cities in the AT&T data whose first cutover occurred after the 1920 Census (January 1920). The left-most column provides the effect of cutovers on the percent 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. The final column estimates effects on (log) average occupation scores. All regressions include log city size controls. *, **, *** represent significance at the 0.1, 0.05, and 0.01 levels, respectively. Robust SEs in parentheses.

D Supplementary Results

D.1 Differential effects of cutovers across subpopulations

Figure D.1 presents event study estimates of the effects of dial cutovers on the percent of different population groups who are telephone operators in the telephone industry, estimated with 10-year intervals. Consistent with the evidence from Table II 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 percent 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 dial cutovers on the percent 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. All point estimates include confidence intervals, which are in some cases narrow.

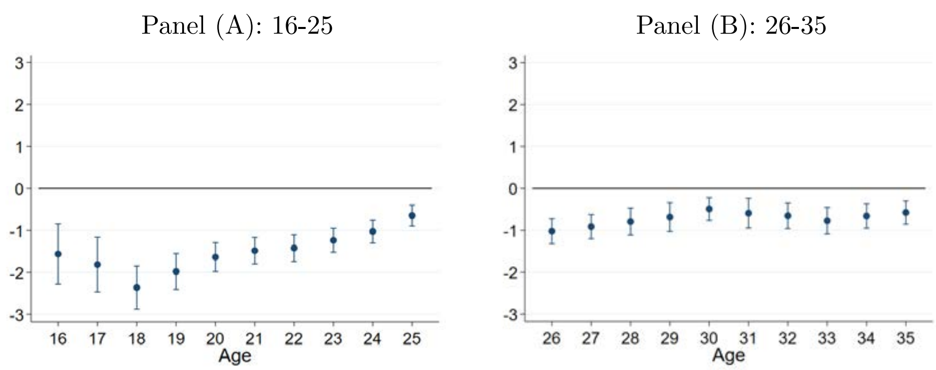
D.2 Effects of cutovers on older (26-35) workers

Our focus on white, American-born women age under 25 years old is motivated by our understanding that this is the demographic group most exposed to automation. Telephone operation was a young woman's job: local operators (that is, the women supporting local telephone service) were mostly young, entry-level workers and comprised a significant majority of telephone operators pre-automation—and this was the work that got mechanized. This can be seen in part in Table II. That said, AT&T (and other telephone companies) did employ some older operators, who comprised a relatively larger fraction of the operating force in the later decades of our study period (in part, but perhaps not entirely, because the entry-level work was automated).

In light of the evidence (from our longitudinally-linked operator sample) that older *incumbent* operators were more adversely affected by automation (Table IV), a natural question is how the effects of automation on successive cohorts of older workers compare to the effects on successive cohorts of younger workers (which have been our focus thus far). To better understand this question, we examine age-specific impacts of automation, expanding the evidence to a wider age range (16 to 35, vs. the 16 to 25 age range we study in the body of the paper).

Figure D.2, which shows the age-specific impacts of cutovers on the percent of these women who are telephone operators, by age. Panel (A) reproduces Figure IV(B) from the paper. Panel (B) repeats its analysis for older ages. We see that both younger and older working women were less likely to be telephone operators in the telephone industry post-cutover, but that the magnitudes of these effects are smaller for older women. The reason is straightforward: their base rates were lower. Telephone operation was a particularly important job for young women, employing a much larger share of younger working women than older working women.[21] As a result, older women, at the population level, were less exposed to mechanical call switching.

Figure D.2: Effect of cutovers on the percent of working white, American-born women who are telephone operators in the telephone industry (DID by age)



Notes: Figure shows event study and staggered difference-in-difference estimates (by age) of the effects of dial cutovers on the percent of working white, American-born women in successive cohorts who are telephone operators in the telephone industry, for the small city sample (population ≤100k in 1920). Panel (A) presents these estimates for ages 16-25 (reproducing Figure IV), and Panel (B) for 26-35. Error bars represent 95% confidence intervals, computed from SEs clustered at the city level.

Building on this initial evidence, Table D.1 estimates a variant on Table VII(A) of the paper—which

[21] In raw averages, telephone operation's share of white, American-born women's employment peaked at around age 18-19 at >4%, but declined with age, down to ≈2% by age 30 and lower at older ages.

studies employment rates for young, white, American-born women age 16 to 25—now reproducing it for those age 26 to 35. The share of these women working as telephone operators declines after cutovers, but we don't find evidence of a decline in employment.

Table D.1: Changes in work, education, marriage, and fertility patterns around cutovers: White, American-born women ages 26 to 35

	Percent of the group that is:				
	Tel. oper.	Working	In school	Married	Has children
Post-cutover	-0.25***	0.57	0.04	-0.21	-0.02
	(0.03)	(0.38)	(0.07)	(0.28)	(0.33)
N	113652	113652	113652	113652	113652
R^2	0.33	0.70	0.41	0.67	0.73
Cities	2845	2845	2845	2845	2845
Cut over	261	261	261	261	261
Y Mean	0.55	29.78	1.10	75.05	57.90

Notes: Tables present staggered difference-in-difference estimates, by age, of the effects of dial cutovers on the percent of white, American-born women age 26-35 in successive cohorts who are working, in school, married, and have children, for cities with population ≤100k in 1920. The left-most column provides the effect of cutovers on the percent 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.

D.3 Effects of cutovers on migration, incumbent telephone operator sample

Table D.2: Effects of dial cutovers on migration distance

	All Ages		16-20		21-25		26+	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Migrated more than 10 miles?								
Operator × Post-cutover	0.040***	0.027**	0.015	0.003	0.049***	0.053**	0.046***	0.057**
	(0.011)	(0.012)	(0.016)	(0.021)	(0.016)	(0.022)	(0.016)	(0.024)
Operator	-0.018***	-0.013**	0.013	0.017*	-0.027**	-0.028**	-0.068***	-0.065***
	(0.005)	(0.006)	(0.008)	(0.010)	(0.011)	(0.012)	(0.010)	(0.013)
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City × Year FEs	Yes	No	Yes	No	Yes	No	Yes	No
Operator and Control Worker Pair FEs	No	Yes	No	Yes	No	Yes	No	Yes
Observations	152,353	152,353	72,063	72,063	59,768	59,768	20,522	20,522
Adjusted R^2	0.11	0.14	0.13	0.16	0.12	0.13	0.12	0.13
Dependent variable mean	0.29	0.29	0.30	0.30	0.31	0.31	0.22	0.22
Panel B: Migrated more than 25 miles?								
Operator × Post-cutover	0.037***	0.030***	0.010	0.009	0.056***	0.047**	0.032**	0.048**
	(0.008)	(0.010)	(0.012)	(0.014)	(0.015)	(0.021)	(0.015)	(0.022)
Operator	-0.020***	-0.018***	0.011	0.011	-0.035***	-0.034***	-0.058***	-0.053***
	(0.005)	(0.005)	(0.008)	(0.009)	(0.009)	(0.011)	(0.009)	(0.011)
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City × Year FEs	Yes	No	Yes	No	Yes	No	Yes	No
Operator and Control Worker Pair FEs	No	Yes	No	Yes	No	Yes	No	Yes
Observations	152,353	152,353	72,063	72,063	59,768	59,768	20,522	20,522
Adjusted R^2	0.11	0.12	0.13	0.15	0.12	0.11	0.12	0.11
Dependent variable mean	0.24	0.24	0.24	0.24	0.25	0.25	0.17	0.17
Panel C: Migrated more than 50 miles?								
Operator × Post-cutover	0.032***	0.024***	0.005	0.005	0.051***	0.036*	0.027**	0.037*
	(0.007)	(0.009)	(0.011)	(0.012)	(0.013)	(0.018)	(0.013)	(0.019)
Operator	-0.021***	-0.018***	0.010	0.010	-0.037***	-0.035***	-0.058***	-0.054***
	(0.005)	(0.005)	(0.008)	(0.008)	(0.009)	(0.011)	(0.008)	(0.010)
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City × Year FEs	Yes	No	Yes	No	Yes	No	Yes	No
Operator and Control Worker Pair FEs	No	Yes	No	Yes	No	Yes	No	Yes
Observations	152,353	152,353	72,063	72,063	59,768	59,768	20,522	20,522
Adjusted R^2	0.10	0.11	0.12	0.14	0.11	0.11	0.11	0.11
Dependent variable mean	0.20	0.20	0.20	0.20	0.21	0.21	0.14	0.14

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. We use data from the Census Place Project (Berkes et al. 2023) to geolocate our sample and define the geographic extent of cities and labor markets. Following Bailey et al. (2020), 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 D.3: Effects of dial cutovers on migration outcomes

	All Ages		16-20		21-25		26+	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A: Moved City?							
Operator × Post-cutover	0.036***	0.024*	0.020	0.009	0.037**	0.039*	0.033*	0.055*
	(0.012)	(0.013)	(0.018)	(0.022)	(0.019)	(0.021)	(0.019)	(0.028)
Operator	-0.017***	-0.013*	-0.001	0.003	-0.015	-0.013	-0.050***	-0.049***
	(0.006)	(0.007)	(0.009)	(0.010)	(0.011)	(0.012)	(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 and Control Worker Pair 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	0.16	0.20	0.18	0.23	0.15	0.18	0.17	0.21
Dependent variable mean	0.43	0.43	0.45	0.45	0.44	0.44	0.33	0.33
	Panel B: Moved Labor Market?							
Operator × Post-cutover	0.035***	0.021*	0.011	0.001	0.050***	0.042**	0.026	0.043*
	(0.009)	(0.011)	(0.014)	(0.017)	(0.016)	(0.021)	(0.017)	(0.026)
Operator	-0.019***	-0.013**	0.004	0.010	-0.025**	-0.022*	-0.051***	-0.049***
	(0.005)	(0.006)	(0.008)	(0.009)	(0.010)	(0.012)	(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 and Control Worker Pair FEs	No	Yes	No	Yes	No	Yes	No	Yes
Observations	152,353	152,353	72,063	72,063	59,768	59,768	20,522	20,522
Adjusted R^2	0.14	0.17	0.16	0.20	0.14	0.16	0.14	0.15
Dependent variable mean	0.35	0.35	0.37	0.37	0.37	0.37	0.26	0.26
	Panel C: Moved State?							
Operator × Post-cutover	0.018**	0.005	0.005	-0.011	0.028**	0.033**	0.009	0.002
	(0.007)	(0.008)	(0.010)	(0.011)	(0.011)	(0.015)	(0.014)	(0.018)
Operator	-0.016***	-0.014***	0.001	0.006	-0.024***	-0.028***	-0.039***	-0.038***
	(0.004)	(0.005)	(0.007)	(0.008)	(0.007)	(0.009)	(0.007)	(0.008)
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City × Year FEs	Yes	No	Yes	No	Yes	No	Yes	No
Operator and Control Worker Pair 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	0.07	0.08	0.09	0.11	0.08	0.09	0.08	0.09
Dependent variable mean	0.14	0.14	0.15	0.15	0.15	0.15	0.10	0.10

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. We use data from the Census Place Project (Berkes et al. 2023) to geolocate our sample and define the geographic extent of cities and labor markets. Following Bailey et al. (2020), 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.

E Adjustment Mechanisms

E.1 Shifts in labor supply

A potential explanation for our finding that future cohorts' found work in other occupations after telephone operation was mechanized could be that rather than demand shifting out to accommodate these workers, demand held steady, supply shifted out, and equilibrium wages fell.

A first take on whether our results may be explained by labor supply shifts can be had by looking at the magnitudes of the estimates in Table VIII, which estimates changes in occupation shares of young women's employment around cutovers. If the reallocation of workers into substitute occupations is proportional to the overall distribution, this would be consistent with a supply-driven phenomenon: would-be operators dispersed into other jobs that employed people like them, at the rate at which those jobs employed people like them. Conversely, if reallocation were more concentrated, this would seemingly require some countervailing demand growth. The evidence is consistent with the latter: a few specific occupations absorb a much larger share of would-be operators than overall distributions would suggest. Table VIII(A) indicates that around 35% of would-be operators became secretaries/typists/stenographers, despite that this represented only about 10% of young, white, US-born women's employment. Roughly 55% entered restaurant work, despite that it represented only about 5% of the group's overall employment across the panel.

E.1.1 Any evidence of declining wages?

A more direct test of the question of whether the results could be explained by labor supply shifts requires studying wages. Data on wages are available in the complete count census data—but only in 1940, when the census first asked respondents for their 12-month wage income in 1939 and full time equivalent weeks worked in 1939. Dividing one by the other allows us to compute an estimate of workers' weekly wage in 1939, which we will use in this analysis.

Ideally, we would like to supplement these data with a measure of pre-1940 wages at the city and occupation level, especially for young, white, American-born women. This would enable calculating differences and relating them to the automation this paper studies. We have explored several potential data sources for this problem, but none had sufficient breadth (city or occupational coverage) and granularity (wages by occupation, sex, age) to serve this end.

We thus looked for ways to use the wage measures in the 1940 complete count data—which are recorded at the individual level, for the complete U.S. population—to evaluate whether equilibrium wages fell in occupations that (seemingly) replaced telephone operation as a source of employment for young, white, American-born women. Our analysis will take a few complementary approaches. Specifically, we perform three analytical tests using the cross-sectional wage data in the 1940 census (where respondents were asked about their 1939 income), evaluating:

1. "RD-like" design: do we see wage differences for young, white, American-born women in these occupations in cities which had their first cutover just before vs. just after 1939?

   This approach is "RD-like" in that we look for evidence of a discontinuity around the year when wages are measured, in the form of a gap in average (or median) wages in cities whose first cutover is on either side of this threshold. If cities with cutovers around this time were broadly similar, but only (or primarily) differentiated by which side of the census question (pre/post 1939) their cutover falls, this approach should pick up on the wage effect—if one exists. If we think labor markets may take time to equilibrate, we can also vary the "RD" bandwidth. We do not expect cutovers are likely to be endogenous to the timing of the 1940 census (especially as it asked about wages in 1939, which will be the year of the discontinuity in this analysis), reinforcing our confidence in this approach.

Our first step was to create measures of the dependent variable (wages). The IPUMS variable INCWAGE measures wage and salary income, in dollars, from $0 to $5000, with a top-code at $5001+. The IPUMS variable WKSWORK1 measures FTE weeks worked. Starting with the complete count data, we first drop non-income earners and those with top-coded incomes (which are very few in number). We also drop individuals with non-wage income, since we want to focus on employees, though for the population we focus on (young women) this has very little effect on our sample. We compute a weekly wage as wage income divided by weeks worked. We then group up the data to the city level, calculating (i) the median wage, (ii) the mean wage, and (iii) the mean log wage overall and for the focal occupations. We do separately (a) for all women, and (b) for narrower demographic groups (at the sex-age-race-nativity level), which provides city-level information on occupation-specific wages in 1939 for our focal population: young, white, American-born women.

Figure E.1 shows a binned scatterplot of each of these wage measures, for young, white, American-born women in any occupation, across cities with cutovers in 1938-1940. The figure shows no evidence that workers in cities with pre-1939 cutovers have lower median or mean wages in 1939 (the outcome in these charts) relative to cities with post-1939 cutovers. This evidence cuts against the prediction that we would see lower wages for this population in cities which have had their telephone service cut over to dial.

These results are coarse, since they reflect wages across all occupations rather than for the specific occupations which gained the most would-be operators. Table E.1 provides a more refined view, estimating differences in the log median weekly wage of young, white, American-born women in each substitute occupation in cities which had their first cutover in 1938 or 1939, relative to 1940 (the omitted category). We still see no statistically detectable reduction in wages for workers in cities which had their first cutover pre-1939 versus post-1939. (One estimate is statistically significant, but in the positive direction.)

Figure E.1: Binned scatterplot of 1939 wages by cutover timing, 1938-1941

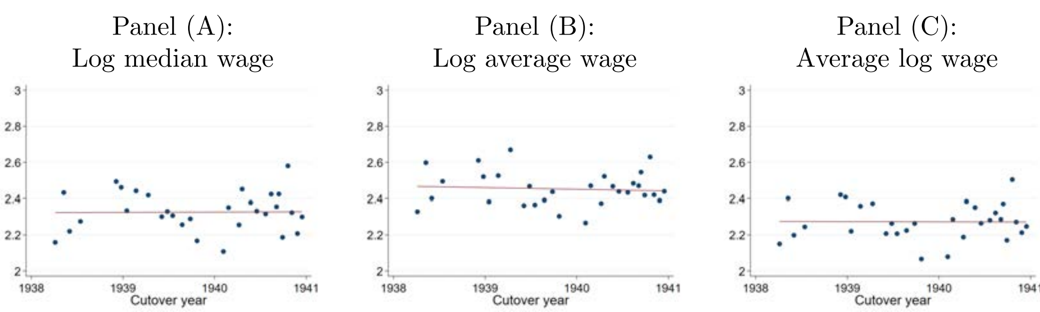


Table E.1: "RD-like" strategy: comparing young, white, American-born women's wages in select occupations in cities with 1938/1939/1940 cutovers

	Ln(Median weekly wage) of those employed as or in						
	All occs.	Off. mach.	Typist/secr.	Office clerk	Sales clerk	Beautician	Waitress
Cut over in 1938?	-0.022	-0.092	-0.025	-0.081	-0.001	0.095	0.131**
	(0.055)	(0.068)	(0.057)	(0.077)	(0.063)	(0.062)	(0.064)
Cut over in 1939?	0.000	-0.101	-0.037	-0.043	-0.011	0.047	0.013
	(0.045)	(0.074)	(0.046)	(0.056)	(0.054)	(0.062)	(0.061)
N	25330	1552	17059	13154	15912	7341	13508
R^2	0.44	0.10	0.24	0.19	0.12	0.10	0.09
Cities	2625	647	2596	2516	2589	2233	2519
Cut over	72	33	71	70	72	64	71
In 1938	13	8	13	13	13	13	13
In 1939	23	11	23	22	23	20	23
In 1940	36	14	35	35	36	31	35
Y Mean	2.26	2.84	2.57	2.46	2.30	2.44	2.10

Notes: Table presents results from a regression comparing the (log) median 1939 weekly wage of young, white, American-born women in the given occupation in cities which had their first cutover in 1938 or 1939, relative to cities with their first cutover in 1940 (the omitted category). Sample restricted to cities which had their first cutover in one of these years. All regressions include 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.

2. Age-based comparisons: do we see differential wage gaps between older and younger white, American-born female workers in our focal occupations in cities which have already been cut over to dial by 1939, versus those which have not (yet)?

   This test will use older workers as a comparison group for younger workers as we look for evidence that in cutover cities, young workers' wages in these occupations fell relative to those of older workers (and relative to the same difference in non-cutover cities). Insofar as the labor market segments on age, which we think is plausible—especially in office work, where entry-level workers often did not compete with advanced workers—this approach will deliver an estimate of the effect of cutovers on young women's wages.

   The results of this comparison are provided in Table E.2. We find no differential gaps in younger versus older white, American-born women's weekly wages (in 1939) in cities which have versus have not had their first cutover by 1940.

Table E.2: Comparing young, white, American-born women's wages in select occupations to older women's wages, in cities with vs. without a cutover by 1940

	Ln(Median weekly wage) of those employed as or in						
	All occs.	Off. mach.	Typist/secr.	Office clerk	Sales clerk	Beautician	Waitress
Post-cutover x 16-25	0.006	-0.003	0.000	-0.022	-0.009	0.007	0.000
	(0.011)	(0.026)	(0.011)	(0.016)	(0.011)	(0.017)	(0.014)
N	61245	4797	40430	32932	39025	16637	28929
R^2	0.48	0.12	0.33	0.26	0.16	0.11	0.09
Cities	2845	1050	2833	2825	2843	2642	2794
Cut over	261	179	261	261	261	255	260
Y Mean	2.52	2.96	2.79	2.68	2.44	2.56	2.18

Notes: Table presents results from a staggered difference-in-differences regression comparing the (log) median 1939 weekly wage of young, white, American-born women in the given occupation to that of older, white, American-born women in cities which have vs. have not yet had their first cutover by 1940. All regressions include 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.

3. Sex-based comparisons: do we see differential wage gaps between young, white, American born male and female workers in our focal occupations in cities which have already been cut over to dial by 1939, versus those which have not (yet)?

   This test similarly uses male workers as a comparison group for female workers in looking for evidence that in cutover cities, female workers' wages in these occupations fell relative to those of male workers (and relative to the same difference in not-cutover cities). Insofar as labor markets segment on sex—which we think is still potentially true—this approach will deliver an estimate of the effect of cutovers on young women's wages.

   The results of this comparison are shown in Table E.3. There is a moderate and statistically significant at the 10% level differential gaps in young, white, American-born women's weekly wages (in 1939) vs. young, white, American-born men's wages for office machine operators in cities which have versus have not yet had their first cutover by 1940. In other occupations, there are no statistically-detectable differences.

Table E.3: Comparing young, white, American-born women's wages in select occupations to young men's wages, in cities with vs. without a cutover by 1940

	Ln(Median weekly wage) of those employed as or in						
	All occs.	Off. mach.	Typist/secr.	Office clerk	Sales clerk	Beautician	Waitress
Post-cutover x Female	-0.008	-0.085*	0.038	-0.015	-0.006	0.049	0.031*
	(0.011)	(0.044)	(0.027)	(0.017)	(0.012)	(0.032)	(0.018)
N	55453	2744	23256	31175	39078	10559	22830
R^2	0.55	0.15	0.26	0.28	0.34	0.13	0.20
Cities	2846	872	2821	2829	2843	2518	2786
Cut over	261	161	260	260	261	248	258
Y Mean	2.40	2.84	2.61	2.60	2.47	2.49	2.21

Notes: Table presents results from a staggered difference-in-differences regression comparing the (log) median 1939 weekly wage of young, white, American-born women in the given occupation to that of young, white, American-born men in cities which have vs. have not yet had their first cutover by 1940. All regressions include 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.1.2 Spillovers to other populations?

A different but related adjustment margin might be that would-be operators displaced other demographic groups from the occupations they took up after telephone operation was automated. This would likewise be a supply-driven phenomenon, rather than a demand-driven story—and its impact would be a decline in the employment rate of *other* demographic groups as the shock rippled through local economies. Even if young, white, American-born women were not themselves adversely affected, this does not necessarily imply workers were unscathed.

An answer to this question requires a different outcome variable than what we have examined thus far: whereas much of the existing analysis evaluates substitute occupations' shares of young, white, American-born women's employment, this question is more concerned with demographic groups' share of each substitute occupations, and spillovers/crowd-out of other workers. We thus pivot from studying occupation shares of employment in specific demographic groups to studying demographic groups' share of each occupation, for our focal occupations.

This approach still presents challenges. The first issue is a conceptual issue, in that this approach effectively frames the pie of work as being fixed: when one group's share goes up, another's must go down. This could be interpreted as share-stealing, but it could also have a different explanation, if group 1 grew the pie. Group 2's share might then go down, but that doesn't imply anyone from group 2 doesn't have a job they would have otherwise had: it could instead reflect the direction in which the occupation grew complemented the skills of one group over another. A second, related issue is an econometric challenge: if we run a sequence of regressions with each group's share of a given occupation as outcomes, and our automation treatment as the independent variable, the effects we estimate will mechanically be interdependent. This problem is similar to those of demand estimation from market shares, where one cannot independently estimate effects of a shock on two firms' market shares—one can at best only estimate effects on the difference against an outside option. To work with the complete set of market shares across competing worker categories in the labor market, one would need to develop a demand system.

In light of these challenges, we focus on estimating effects of automation on young, white, American-born women's share of employment in each substitute occupation specifically (vs. all other groups which collectively make up the remainder). These tests will clarify whether this group's share of each occupation grew post-cutover, potentially displacing others.

Table E.4 presents the results. In the first column, we see that cutovers led to a very large decline in young, white, American-born women's share of telephone operators (consistent with prior evidence, as automation primarily knocked out the work of local/junior operators, and remaining operators were on average older). In the other columns, we see no evidence of changes in young, white, American-born women's share of these other occupations.

Table E.4: Young, white, American-born women's share of select occupations

	Young, white, American-born women's share of occupation						
	Tel. oper.	Off. mach.	Typist/secr.	Office clerk	Sales clerk	Beautician	Restaurant
Post-cutover	-10.04***	2.49	0.67	-0.41	-0.17	0.27	1.01
	(1.13)	(4.38)	(0.44)	(0.39)	(0.27)	(0.36)	(0.87)
N	10914	991	11133	11322	11383	11342	10579
R^2	0.72	0.64	0.75	0.65	0.75	0.80	0.68
Cities	2824	444	2841	2846	2846	2846	2823
Cut over	250	115	261	261	261	261	260
Y Mean	57.69	38.17	49.77	13.90	12.81	9.84	33.69

Notes: Tables present staggered difference-in-difference estimates, by age, of the effects of dial cutovers on young, white, American-born women's share of select occupations, for cities with population ≤100k in 1920. The left-most column provides the effect of cutovers on the percent of these women who were telephone operators in the telephone industry, as a reference point. The other occupations across columns are: (i) offce machine operators, (ii) typists, stenographers, and secretaries, (iii) other offce clerks, (iv) sales clerks, (v) beauty parlor workers, and (vi) waitresses. 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.

This evidence reinforces our view that the reallocation of these women into other occupations did not displace others who would have otherwise had the same jobs.

E.2 Complementarity, productivity, capital accumulation, and more

Neither supply shifts, nor displacement of other demographic groups, seem to explain why young, white, American-born women's employment rates were not affected by the automation of telephone operation. Although automation's first-order impact is nearly always displacement (of workers in the automated task(s)), a growing literature—anchored in the writing of Acemoglu and Restrepo (2018, 2019a,b, 2020)—has pointed out several other mechanisms through which countervailing demand growth might have arisen. For example, the technology may have (i) directly created demand for workers in complementary tasks (e.g., plant workers to service the machines, or secretaries to dial calls on behalf of subscribers), (ii) increased the marginal product of labor, due to automation investments and broader capital accumulation making labor relatively more productive, or (iii) increased aggregate productivity and demand. A fourth explanation, which this appendix builds up to, is the possibility of endogenous "task reinstatement": an equilibrating mechanism in which innovation creates/identifies new uses for labor where it has comparative advantage over capital—in essence, a form of directed technical change (Acemoglu 2002).

In this appendix, we evaluate several of these possibilities (all but task reinstatement) as potential explanations for the lack of an employment impact of mechanical call switching on future cohorts of young women. Other interpretations are also possible, among them the possibility that other technological changes with high potential for economic transformation coincided with the automation events we study. We examine (and rule out) this possibility last, after first testing (and also ruling out) alternative explanations to task reinstatement.

E.2.1 Related job creation unlikely to offset job loss, especially for young women

A first thing we want to examine is the possibility that the technology may have directly increased demand for labor in other occupations or industries. Some of this demand growth may have taken place within telephone companies, which now needed workers to install and maintain mechanical equipment, or to perform some of the ancillary tasks that (a subset of) operators also performed, like tracking call lengths and writing billing tickets. In parallel work (Feigenbaum and Gross 2023), we find evidence some employment growth within the telephone industry in occupations that we understand to have complemented mechanical switching technology, like mechanics and electrical engineers (who serviced the electro-mechanical switching equipment). But the employment growth in these occupations was neither large enough, nor female enough, to offset the massive loss of jobs for young women: there were only a few thousand such engineers, and essentially all were male. This is similarly the case for telephone service inspectors and managers.

Beyond the telephone companies, dial service could have conceivably directly grown demand in other sectors. The nature of the technology shifted some of the burden of connecting calls from telephone operators to users, which may have increased the value of office workers who might perform this task—like secretaries. There are two main reasons why the new work of dialing calls was unlikely to generate a sufficiently large expansion of demand to offset the decline in operators. The most important reason is that the majority of telephones were residential, and nearly all of these subscribers placed their own calls. A second reason is that even for business subscribers, the time cost of telephone dialing was small (seconds per call), and we think this cost was too small to materially affect demand for secretaries. Most firms would at most need a small fraction of a full time equivalent secretary to manage their telephone call volume. This is even more the case when considering that the incremental time and effort of dialing a telephone number over speaking to an operator is even smaller (see Table E.5). For these reasons, we think automation did not directly create enough complementary labor demand to explain our results.

E.2.2 Productivity growth and capital accumulation unlikely to offset job loss

For several reasons, productivity effects and capital investment are unlikely to create enough labor demand for young women workers to offset the jobs lost to automation. The first of these is that although mechanical switching significantly increased AT&T's labor efficiency (see Appendix Table A.1), the wider productivity impacts were likely small. Table E.5 provides back-of-the-envelope estimates of the potential for time savings across business telephone users, using data on the number of business telephones and telephone calls from the U.S. Census of Electrical Industries (U.S. Census Bureau 1902) and on AT&T experimental estimates of the average improvement in connection times under mechanical switching (AT&T 1923). Aggregating the 2.4 seconds saved per call across all business calls yields a total annual time savings of 8 million hours (roughly 4,000 full-time equivalents) across the economy in 1922. In practice, these time savings would have been widely distributed across telephone users, and not perfectly aggregable into FTEs. We estimate these savings to have been around 1.4 hours per telephone set.

Table E.5: Potential aggregate savings from mechanical switching at U.S. businesses

We calculate the potential aggregate savings of mechanical switching across all business telephone service by taking AT&T's estimated savings per telephone call, and aggregating across all business calls.

(1)	Bell system telephone calls (MM)	16,567	Census of Telephones (1922)
(2)	Assumed fraction business *(higher than the 61% business fraction of telephones)*	75%	Conservative assumption
(3)	Bell System business calls (MM)	12,425	Calculated: (1)*(2)
(4)	Avg. time saved per call (secs)	2.4	AT&T (1923)
(5)	Aggregate time savings (hours, MM)	8.1	Calculated: (3)*(4)/3600
(6)	Annual FTE-equivalents, at 2000 hrs per FTE	4,055	Calculated: (5)*1e6/2e3

In practice, these savings will be widely distributed among users; i.e., it's not that 4,000 FTEs will be saved at one employer but rather that small amounts of time will be saved by every user. These savings generally cannot be aggregated across firm boundaries into jobs.

What are the annual savings per business telephone station?

(7)	Bell system telephones (1000s)	9,515	Census of Telephones (1922)
(8)	Business fraction of telephones	61%	Census of Telephones (1922)
(9)	Bell System business telephones (1000s)	5,776	Calculated: (7)*(8)
(10)	Annual calls per business telephone	2,151	Calculated: (3)*1e3/(9)
(11)	Avg. time saved per call (secs)	2.4	AT&T (1923)
(12)	Annual savings per business telephone (hours)	1.40	Calculated: (10)*(11)/3600

Notes: Data from U.S. Census Bureau (1902) (see 1922 Census of Electrical Industries: Telephones) and AT&T (1923) (AT&T Plant and Engineering Conference of 1923).

The time savings are thus small, and the telecommunications that automation supported were not otherwise different from those under manual switching. Productivity gains across the economy resulting from the adoption of mechanical switching are thus ruled out as a driver of offsetting labor demand growth. Because its aggregate impacts (beyond displacement) were so limited, we likewise believe that mechanical switching was unlikely to trigger capital accumulation, or improvements in automation across other sectors of the economy. Moreover, insofar as countervailing demand does result from productivity gains or capital investment, there is (in our view) little reason to think it would be concentrated among young women specifically.

E.2.3 Mechanical switching not related to broader technological progress

The last question we pose is whether the effects of cutovers on employment rates (or lack thereof) may be confounded by concurrent technological changes with countervailing impacts on aggregate productivity, and in turn labor demand for all workers, including the young women which are the focus of our analysis. To do so, we compare changes in employment in occupations closely related to major technologies of the early twentieth century. By most accounts, the two most transformative technologies of this period were electricity and automobiles, each of which diffused rapidly between 1900 and 1940 (Eli et al. 2022). Electricity, in turn, supported the growth of American manufacturing. To examine whether these changes coincided locally with telephone industry automation, we identify associated occupations and estimate whether they grew after mechanical switching was adopted (which we treat as a sufficient statistic for wider change).

Table E.6 estimates the post-cutover difference-in-difference in the per-capita number of (1) telephone operators, (2) electricians, (3) machinists, (4) automotive mechanics, (5) deliverymen, and (6) truck drivers, for the same midsize and smaller cities we study throughout the paper. As the table statistic show, each of these occupations was large, with 60,000 to 370,000 workers by 1940, and some grew by orders of magnitude between 1910 and 1940 (e.g., the number of auto mechanics in these cities grew from 600 to 130,000). Telephone operators per capita fell sharply, by roughly half the mean, in cities post-cutover. We find no such effects for workers in other technologically-progressive occupations. We interpret this evidence as indicating that cutovers did not locally coincide with the diffusion of these other technologies, and thus that demand growth driven by co-occurring technological changes do not explain our (non)result.

Table E.6: Changes in number of workers in select professions per capita, 1910-1940

	(1) Tel. operators	(2) Electricians	(3) Machinists	(4) Auto mechanics	(5) Deliverymen	(6) Truck drivers
Post-cutover	-0.11***	-0.01	-0.05	0.01	0.02	0.00
	(0.01)	(0.01)	(0.06)	(0.01)	(0.01)	(0.02)
N	11384	11384	11384	11384	11384	11384
R^2	0.68	0.71	0.84	0.83	0.78	0.87
Cities	2846	2846	2846	2846	2846	2846
Cut over	261	261	261	261	261	261
Y Mean	0.25	0.29	1.06	0.38	0.33	0.84
Ct. in 1910 ('000s)	24.9	32.3	160.8	0.6	37.3	15.5
Ct. 1940 ('000s)	64.3	69.4	174.9	129.1	136.0	372.3

Notes: Table presents staggered difference-in-difference estimates of the post-cutover change in the number of workers in six different occupational categories, per adult population. The occupations shown are representative of (and proxy for) the major technological changes taking place across the U.S. in the first half of the twentieth century, such as electrification, industrialization, and the diffusion of automobiles. Table statistics show the total number of workers in each occupation in 1910 an 1940 for the sampled cities. Observations are at the city-year level, restricted to cities with population $\leq$100k in 1920. 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 Task reinstatement: Growing labor demand in other sectors

The remaining possibility is what Acemoglu and Restrepo (2018) label "task reinstatement", which is really endogenous technical change which creates new uses for newly abundant factors (in this case, workers). The thesis of this work is that task-creating innovation is a key equilibrating mechanism that sustains labor demand, and the labor share, even as increasingly more tasks have been automated over history. As these authors explain it, "whenever one of the two types of innovation [automation versus task creation] is more profitable, [innovative activity] will be allocated to that activity" (Acemoglu and Restrepo 2018, p. 1515).

We in part arrive at this interpretation by elimination of alternatives (in the prior subsections). A remaining challenge, however, is that although we have a clear example of automating technological change (mechanical call switching), there is not a single, obvious labor-augmenting technology on the other side of this story. Importantly, in our view, there does not need to be, as not all innovation is technological. Countervailing demand growth need not arise as a result of technological innovation specifically: organizational innovation, in our view, can be just as impactful for productivity and demand growth (and historically has been). This may take shape with the diffusion of old tasks into new sectors. Entrepreneurs find pockets of opportunity—and here, new ways to put existing categories of workers to productive use. In this subsection, we present evidence that is consistent with this type of endogenous economic adaptation.

E.3.1 Evidence from census data

Concretely, what we see in our case is countervailing employment growth in other occupations that employ workers who are similar in skill and demographics to operators. The challenge lies in that we do not know exactly what they were doing. To some readers, these substitute occupations may seem like "old work": secretaries, typists, and stenographers, food service workers, and others existed well before mechanical call switching. Against this fact, there are two versions of "task reinstatement" we might imagine. The first is that these workers were doing new things that had never been done before. The second is that local demand grew for existing uses of these workers in new sectors (a softer claim, in our view). Our interpretation runs closer to the latter: we think new sectors began to make use of these workers. To give a concrete example: around this time, hospitals and doctors began keeping patient records, and hiring stenographers to take patient notes (e.g., Siegler 2010; Gillum 2013). Though the job (stenographer) was not new, and the task (note-taking) was not new, the medical application of stenographic work was. Given this new specialization, and the introduction of a new, associated job title, this job could be considered "new work" (and has been by others in recent research, such as in Autor et al. 2023).

Similar examples abound. In this subsection, we take a closer look at this phenomenon (the emergence of new occupation-industry pairings): in effect, the proliferation of old work to new industries. We are especially interested to see this happening locally: a given class of worker may be already be employed in each industry somewhere in the U.S. (indeed, this is generally likely to be the case, given the size and dispersion of the U.S. population), but we are interested to know if local employers found new ways to employ young women in, e.g., secretarial work or waitressing work in sectors which did not previously employ these types of workers.

The data have some important descriptive clues. For example, secretarial and stenographic work was expanding widely in this era. Nationally, in 1910, the top five industries with secretaries/typists/stenographers accounted for 63% of their total. In 1940, the top-5 concentration ratio was down to 46%. The evidence for waitresses is different, but also has an unexpected twist. In every decade, "Eating and drinking places" and "Hotels and lodging places" account for >90% of all waitress work. But there was a category that was growing quickly: drug stores, where waiter/ress employment grew from 1 person in 1910 to >20K in 1940. The drug store lunch counter is now widely remembered as a feature of the early to mid-twentieth century.

We examine the relationship of local cutovers to mechanical switching to the share of young, white, American-born women's employment that is in a given occupation of interest and in local industries which, as of the given year, had not previously employed a worker in that occupation. Although this is a time-varying definition of "newness", we think it is preferred for two reasons: (i) it does not require choosing an arbitrary baseline to define the existing stock, but rather allows it to vary dynamically, and (ii) in doing so it reflects ongoing innovation.

Table E.7 shows what we find. We first focus on the second column, which shows that in cities with cutovers, young, white, American-born women took up more secretarial/typist/stenographic work in industries which had not previously employed this kind of labor. The size of the effect (0.41 p.p.) is large relative to the total increase in the share of this group working in this occupation (0.54 p.p.; Table VIII), and significant at the 5% level. Here we do not see significant growth in the share of this demographic working as waitresses in in locally-new industries, though we also note (per above) that this work was concentrated (with a top-2 industry concentration of >90% throughout the sample period), suggesting limited possibility of broadening.

Table E.7: Share of young, white, American-born women's employment which is in the given occupation and in locally-new industries

	Off. mach.	Typist/secr.	Office clerk	Sales clerk	Beautician	Waitress
Post-cutover	0.03	0.41***	0.07	-0.08	0.00	-0.06
	(0.02)	(0.15)	(0.06)	(0.06)	(0.01)	(0.04)
N	110372	110372	110372	110372	110372	110372
R^2	0.37	0.36	0.33	0.27	0.27	0.28
Cities	2845	2845	2845	2845	2845	2845
Cut over	261	261	261	261	261	261
Y Mean	0.11	3.40	1.17	0.60	0.02	0.48

Notes: Tables present staggered difference-in-difference estimates of the effects of dial cutovers on young, white, American-born women's employment shares in locally-new industries in select occupations, across successive cohorts, for cities with population ≤100k in 1920. From left-to-right, the 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.

Despite this, we think drug store soda fountains and lunch counters should get specific attention: this was by far the fastest growing segment of waitressing work in the period we study, with 20-30% year on year growth rates nationally between 1920 and 1940 (compared to ≈1% population growth rates). We thus took an interest in measuring the local emergence of this work. Though we do not see effects of cutovers on the share of young women employed as waitresses in drug stores per se, we explore other angles on the question. One result we do find, with consistency, is that cutovers increased the odds that a city subsequently had its first young, white, American-born women working at lunch counters. We think this is suggestive evidence that the growth of pharmacy lunch counter could in part (though in our view, not in full) have been an endogenous response to the contraction in demand for young women workers from the telephone sector, and the ensuing relative abundance of these workers, after the automation of telephone operation.

E.3.2 Mediators of task reinstatement

The foregoing evidence is consistent with endogenously-reinstating demand sustaining young women's employment rates after telephone operation was (mostly) automated. Yet this paper is one case—and it leaves us with questions around how general this example might be. Under what conditions is this adjustment mechanism likely to take effect, and when may it fail? For example, when aggregate demand slack, technological or business model innovation that creates demand in new sectors of local economies may be weakly incentivized, such that the displacement effects of automation may dominate, generating employment declines. Certain sectors of the economy may be more productive at task creation, especially for specific segments of the labor market (like young women in the early twentieth century). Complementary technology or innovation-led structural transformation may also facilitate endogenous task creation, as the returns to generating new tasks that fit with the modern technological environment increase, and the opportunity cost of replacing increasingly-obsolete tasks declines, in an evolving economy.

The long analysis window in this paper presents opportunities to study these questions, especially since our analysis intersects with the Great Depression and other changes in the U.S. economy. To do so, we collect additional data that we use to group cities and compare the effects of cutovers across them. We first examine the technological environment, focusing on two of the defining technologies of the first half of the twentieth century, both of which increased productivity and led to factor reallocation across space and across sectors: electricity, and automobiles. Though we lack city- or county-level measures of electricity or automobile diffusion, census data permit reasonable proxies in the per-capita number of electricians and auto mechanics. We next consider local economic diversity, conjecturing that cities with a more diverse employment base may be more resilient to automation shocks, as there are many sectors in which countervailing labor demand can endogenously emerge. We compute, for each city and year, a Herfindahl index of employment across economic sectors. We also use these data to measure cities' 1910 manufacturing share of employment, which provides a distinct view of local economic structure and reinstatement potential. Finally, we investigate heterogeneity across cities which were more versus less severely exposed to the Great Depression, using the Fishback et al. (2005) measures of county per-capita retail sales contractions from 1929 to 1933.[22] For each of these measures, we partition our sampled cities into quartiles and compare effects of automation on employment across them.

Table E.8 presents the results, with each column heading identifying the interacted measure. We do not find any differential effects of cutovers across cities by technological conditions. They do, however, relate to two economic factors: manufacturing intensity and Great Depression severity. Because manufacturing was a predominantly male worker industry, manufacturing-intensive cities may have been less conducive environments for endogenous task creation and demand reinstatement for young women. Consistent with the logic of Acemoglu and Restrepo (2019b), we also find that in cities with the most severe contractions during the Great Depression, cutovers were followed by a decline in employment of young women. This suggests that aggregate demand has a direct impact on whether, when, and to what degree labor demand can recover from large automation shocks, and which workers (or cohorts) are most exposed. That the estimated effect is monotonic in depression severity further reinforces this result.

[22] 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 characteristics; (ii) it abstracts away from cross-sectional variation in depression severity, which exceeds the time-series variation; (iii) in comparing cities automated in the 1929-1933 period against others, our tests would be underpowered; 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.

Table E.8: Changes in employment of young, white, American-born women around cutovers (diff-in-diff, by age, 16-25, interacted with potential sources of heterogeneity)

	Per capita: Electricians	Auto mechanics	Sector diversity	Manufacturing intensity	Depression severity
Post-cutover	0.19	0.53	0.10	2.08*	1.52*
	(1.39)	(0.86)	(1.14)	(1.08)	(0.91)
* 2nd quartile intensity	-0.26	-0.89	-0.25	-2.39*	-0.64
	(1.56)	(1.06)	(1.41)	(1.32)	(1.18)
* 3rd quartile intensity	-0.50	-1.24	-0.01	-2.53**	-2.09*
	(1.55)	(1.16)	(1.28)	(1.18)	(1.18)
* 4th quartile intensity	0.38	0.62	-0.44	-2.70**	-3.55***
	(1.55)	(1.18)	(1.27)	(1.27)	(1.19)
N	113752	113752	113712	113712	113752
R^2	0.83	0.83	0.83	0.84	0.83
Cities	2845	2845	2844	2844	2845
Cut over	261	261	261	261	261
Y Mean	40.35	40.35	40.35	40.35	40.35

Notes: Table presents staggered difference-in-difference estimates, by age, of the effects of dial cutovers on the percent of young, white, American-born women in successive cohorts who are working, for cities with population $\leq$100k in 1920. Columns (1) and (2) partition cities into quartiles by the number of electricians and auto mechanics per capita; Column (3), by industrial diversity; Column (4), by manufacturing intensity; and Column (5), by Great Depression severity. For the latter, cities are assigned their county's value, as Depression severity is only observed at the county level. 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.

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