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

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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 new categories of work. Using a new genealogy-based census-linking method, we show that incumbent telephone operators were most affected, and a decade later more likely to be in lower-paying occupations or no longer working. *JEL codes:* E24, J21, J24, J62, M51, M54, N32, O33, O40.

I. Introduction

Automation anxiety has recently surged in the United States and other developed economies (Autor 2015), fueled by warnings of a sweeping wave of automation (Brynjolfsson and McAfee 2014). Yet the degree to which automation reduces employment,

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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 article, we study the effects 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 United States 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 that are increasingly being automated today (U.S. Bureau of Labor Statistics 2022b).

We document the effects of mechanizing telephone operation on incumbent workers and future generations. To do so, we construct a data set 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 fell by 50% to 80%. These jobs made up 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 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 reequilibrated 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 that 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—that is, the emergence of new work (Autor et al. 2024). 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 article, 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 1% of the nonfarm U.S. workforce, and by far the largest employer of women. The growing network 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 forthcoming). 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 United States 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 article, we focus on the 2,846 cities with ⩽ 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 produces a broader, more representative linked sample of women than existing methods, and is among the contributions of this article.[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 next decade. Relative to non-operator women in the same city and telephone operators in untreated cities, we find that treated operators were significantly less likely to be working as operators 10 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

1. Prior approaches to building linked samples of women restrict to women whose marital status does not change across censuses (Marchingiglio and Poyker 2019; Price et al. 2021), or use marriage records to identify maiden and married names (Eriksson et al. 2019; Withrow 2020). In our case, linking always-single or already-married telephone operators to their census record 10 years later would condition the sample on an endogenous outcome or restrict our analysis to a small, nonmodal population of operators, and the uneven coverage of marriage certificates across states would preclude a nationally representative sample.

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 similar-skill secretarial work and lower-skill 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 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 reequilibrated 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 that had not previously employed these kinds of workers, which we interpret as evidence of the emergence of new work (Autor et al. 2024). 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.

This article 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 (Bessen et al. 2019), but varies for which workers and with what net effects on employment (Chiacchio, Petropoulos, and Pichler 2018; Dauth et al. 2018; Graetz and Michaels 2018; Acemoglu and Restrepo 2020; Adachi, Kawaguchi, and Saito forthcoming). The empirical literature studying the forces that 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 intrafirm labor demand through q-complementarity or scale effects (Aghion et al. 2020; 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 Acemoglu and Restrepo 2019b; Autor et al. 2024). 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 article 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 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

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 (Dillender and Forsythe 2019).

technology to the specific task it performed and the workers who would have otherwise performed it.[3]

The historical setting of this study, 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 with 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 that have (or are likely to have) heterogeneous effects on different kinds of work, and research is often unclear on whether these technologies represent automation or capital-augmenting technological progress, mechanical call switching was explicitly, specifically labor-replacing. Second, we think there are few specific cases of these technologies today that would come to bear on a population as large as telephone operators. Third, local operators were eliminated more abruptly than most occupations that may be at risk

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, by 1940 it was close to current levels. Educational attainment was also rapidly growing across cohorts. Cohort differences, however, will be accounted for by fixed effects.

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 II 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 III introduces our data on telephone operators, local labor markets, and mechanical switching. Section IV describes characteristics of telephone operators and cities with cutovers. In Section V, we show that cutovers significantly reduced the number of telephone operators in cities where service was automated. In Section VI we examine the effects of cutovers on incumbent telephone operators. In Section VII, 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 VIII concludes with lessons and remaining questions.

II. Historical Background

II.A. 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 United States for most of the twentieth century. Bell Telephone (AT&T's predecessor) was founded in 1877, a year after Alexander Graham Bell's 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 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 (Online Appendix Table A.1). AT&T served around half of telephones in the early 1900s, after which it began acquiring independents across the nation 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.

II.B. 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, 2) summarized the job requirements as follows:

> An applicant was expected to be a high school graduate, at least 18 but not much older, in good physical condition, and living at home or with close relatives. Good eyesight and good hearing... are 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

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).

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 article.[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% a year (O'Connor 1930).

In 1920, telephone operators were roughly 2% of the U.S. female workforce and 4% of nearly 3 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 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.

II.C. 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

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

7. Population statistics throughout the article 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 were telephone operators 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 10 years, 400 unique women might be employed.

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 forthcoming).

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 two to three years of preparation—for example, to get regulatory approval, prepare the mechanical equipment, distribute dial telephone sets, and draw up new telephone numbering plans and directories. Operationally, cutovers from manual to dial (when the wires were cut from the manual switchboards and connected to the mechanical equipment) were discrete events that 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 80% (Gherardi 1917). 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.

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

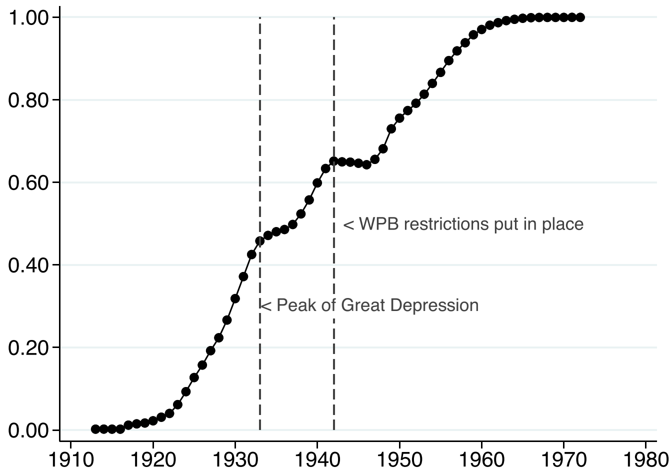

FIGURE I

Percent of Bell System on Dial, 1913–1972

The figure shows the fraction of Bell system telephones with mechanical operation (i.e., dial) over time. Data are 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.

through the entire network, by which time AT&T had already begun adopting digital switching. Our focus for this study is the 1910–1940 period.[9] In Section III we document cross-sectional variation, and in Section IV we return to discussing the drivers of automation in more detail.

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.

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).

II.D. 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 nonfarm 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, 5), 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 to its current level (U.S. Bureau of Labor Statistics 2022a).

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 constitute a control group. Moreover, if marriage bars or increasing school attendance were coincident with the time and place of

10. Though most common among public school teachers, Goldin (1988) finds marriage bars present in some clerical employment. Green (2001) recounts that 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).

mechanization, we would expect to see even larger declines in post-cutover employment rates of young women—in contrast to the muted effects we find.

III. Data and Geographic Coverage

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

III.A. 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 (Sheldon Hochheiser, personal communication, 2017). However, we located in the AT&T corporate archives a single document from 1937 that lists the earliest cutover and percent of subscribers on dial service for 164 U.S. cities (and 7 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)—which we manually extend to 1940.

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 impacts on 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. Online 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.

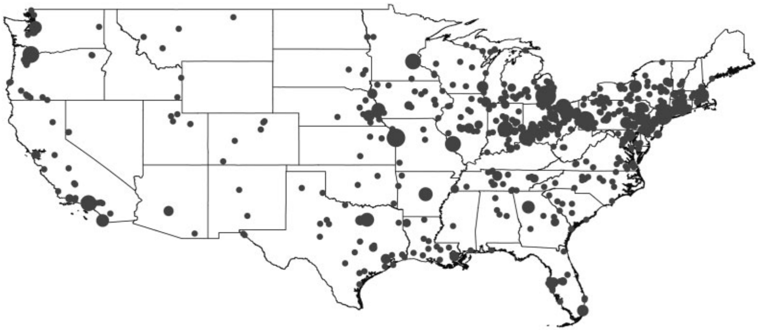

FIGURE II

Cities in Data with Cutovers by 1940

The figure maps the cities with a dial cutover in the AT&T and newspaper data through 1940. Bubble sizes are proportional to the number of reported cutovers through 1940.

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%).

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 Online Appendix Figure B.5 shows, for cities in both the AT&T and newspaper data sets, 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). Online Appendix Figure B.2 provides evidence that cities of under 100,000 people in 1920 typically had one cutover in which the entire city was converted to dial, whereas larger cities were converted in a piecemeal fashion—motivating our empirical focus on smaller cities.

III.B. Data on Local Outcomes

We use IPUMS complete count U.S. census data (Ruggles et al. 2020) to measure local outcomes between 1910 and 1940. Throughout this article, we restrict attention to the adult (16+) nonfarm population in the continental United States 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 size of a city's operating force and identify workers exposed to cutovers. For each subgroup, 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 Online Appendix B). We 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 $\leqslant$ 500 people in any year, 56 cities with anomalous reporting of occupation (see Online 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]

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

12. We drop the handful of cities with a population $\leqslant$ 500 in 1910 to eliminate those where inference is made difficult by small samples—though this is immaterial to our analysis, which is 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 (Online 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

III.C. Linked Sample of Female Telephone Operators

To understand the long-run effects of telephone cutovers on operators, we have to follow 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 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, 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 (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 woman 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

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.

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 $\leqslant$ 100,000 in 1920, which is our core sample throughout the article. 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.

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.

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 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 which were already cutover to dial by 1930. 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 supplement these data by identifying, for each operator, a matched comparison set of women from the same census enumeration district (akin to a neighborhood of roughly 1,000 residents) who were also working and of the same age (± 5 years), sex, race, nativity (U.S. versus foreign-born), parental nativity, marital status, and with or without children, and we apply the same linking procedure to track them from a base year to the next census. This effort produces matched controls for about three-quarters of operators in 1920 and 1930, with an average of 4.7 control women per operator. With this expanded sample we can add operator-specific fixed effects to condition comparisons to between treated operators and their matched controls.

An example can clarify why linking women is difficult, and why 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 (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. If another woman named Daisy born in Kentucky around 1902 marries and takes a surname of Fay, we may falsely match two distinct people. 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 (Online 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 Online 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.[15]

15. 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 FamilySearch sample vis-à-vis descendants or genealogists—especially because FamilySearch is affiliated with the Church of Jesus Christ of Latter-Day Saints.

IV. Characteristics of Telephone Operators and Cutover Cities

IV.A. 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- 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—was substantially larger.

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 ($\leqslant 25$) 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 characteristics of operators in the telephone industry. Telephone operation thus went from being a young woman's job to an older woman's job over this period, as local service was automated.

IV.B. 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 forthcoming), we study what propelled

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 (1,000s)	2,427.6	2,690.9	3,618.4	4,043.3	2,295.9	2,769.8	3,509.4	4,148.5
Working population (1,000s)	1,032.3	1,215.6	1,409.0	1,143.0	865.8	1,124.9	1,556.9	1,873.5
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	1.9	3.1	3.8	3.5	2.0
Population 5–10k	3.7	4.5	3.5	1.8	2.8	3.4	3.3	1.8
Population 10–20k	3.2	4.2	3.7	1.6	2.3	3.0	3.1	1.7
Population 20–50k	2.8	3.8	3.9	1.5	2.0	2.7	3.0	1.7
Population 50–100k	2.6	4.0	3.4	1.1	1.6	2.6	2.8	1.3
Population 100–200k	3.0	4.8	4.0	0.9	1.9	3.1	3.2	1.0
Population > 200k	3.3	4.9	4.8	1.0	2.2	3.7	3.7	1.3

Notes. The 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 Online Appendix B).

TABLE II
CHARACTERISTICS OF TELEPHONE OPERATORS, 1910–1940

	Telephone industry				Other industries			
	1910	1920	1930	1940	1910	1920	1930	1940
Population (1,000s)	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-Hispanic	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. The 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.

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 became system bottlenecks, service quality fell, and operator requirements 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)–(3) is an indicator for whether a city had a cutover pre-1940, and in columns (4)–(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.[16] Across all columns, population is the

16. 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.

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	2,992	2,986	2,986	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	1,929.08	1,929.08	1,929.08
State FEs	Yes	Yes	Yes	Yes	Yes	Yes

Notes. The table presents horserace regressions of (i) an indicator for whether a city had its first cutover by 1940 (columns (1)–(3)) and (ii) the timing of that first cutover (measured in decimal years; columns (4)–(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 16–25). *, **, *** represent significance at the .1, .05, and .01 levels, respectively. Heteroskedasticity-robust standard errors are in parentheses.

main determinant of cutovers, explaining more variation than any other variable—including state fixed effects, which are included in all columns.[17] In a full horse race regression, we find

17. 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.

that cities with cutovers before 1940 were larger, richer, and more likely to have unionized telephone operators (column (3)), though population has nearly 10 times the explanatory power of other variables.[18] Conditional on having a pre-1940 cutover, larger cities had earlier cutovers (column (6)).[19]

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. 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 article we 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 effects, this could confound our results. We are reassured by two observations. First, cutovers would be shaped by expected labor demand, but their effects 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 made up the Bell System. Operating company managers made decisions over mechanization, and sometimes,

18. 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 article, 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.

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

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 midsize 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 (Online Appendix A).

V. Effects of Automation on Demand for Telephone Operators

Our primary goal in this study 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 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.

V.A. *Empirical Approach*

We take two empirical approaches to studying the effects of dial. Here and in Section VII, 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 VI, 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 is on cities with population $\leqslant$ 100,000 in 1920, where automation was typically a discrete event (Online Appendix Figure B.2).[20]

20. For this analysis, we pare our sample to the 2,845 cities with 1920 population $\leqslant$ 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 Online 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—where we find similar results to our TWFE strategy.

Our first set of results estimate the following TWFE 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 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 10 years (and will thus be included in every fifth bin).

In nearly all specifications we restrict attention to a single subpopulation (e.g., white, American-born women age 16–25, 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 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 others 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.[21]

21. 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.

These controls are important because population is closely related to cutovers (Section IV), and may also correlate with outcomes. For example, high school completion rates among 16- to 18-year-olds were rising throughout this period, 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 local population controls eliminate differential pretrends 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 replace event studies with a staggered DID specification for other outcomes (that is, TWFE with a binary treatment indicator, replacing D^s_{it} in equation (1) with $D_{it} = \mathbb{1}(\text{Post-Cutover})_{it}$, which indicates whether city i in year t is post-cutover). This yields the following specification:

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

V.B. *Effects of Dial on Operator Jobs*

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

In Figure IV we shift our focus from the number of operators to the fraction of young women's jobs that were automated

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

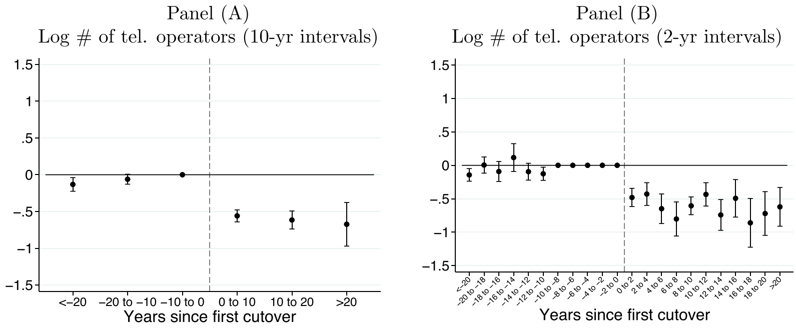

FIGURE III

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-year Intervals)

The 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, for the small-city sample (population $\leqslant$ 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 standard errors clustered at the city level.

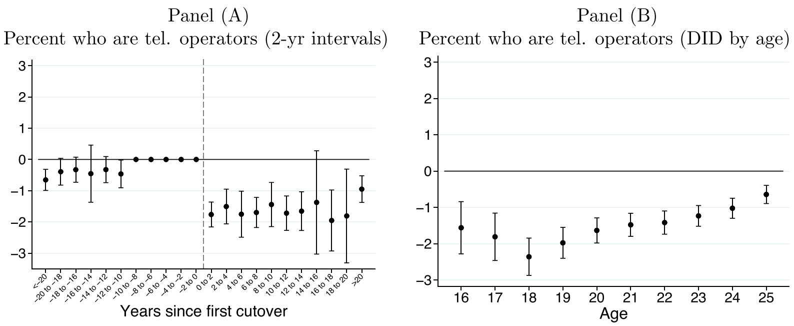

FIGURE IV

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)

The 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 $\leqslant$ 100k in 1920). Error bars represent 95% confidence intervals, computed from standard errors clustered at the city level.

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 throughout much of the rest of the article. 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.

In Online 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 Online 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 intuition and evidence, we use OLS for the remainder of the article.

We present three other robustness checks in the Online Appendix. First, although we are estimating these effects in all cities in the continental United States meeting our sampling criteria, our measurement of cutovers partly depends 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 that 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 (Online 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 (Online 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 V that older incumbent operators experienced more adverse effects. Because telephone operation made up a smaller share of older (26+) women's employment, the effects on future cohorts are smaller but directionally similar to those on younger women (Online Appendix D.2).

VI. 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 Online 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 reemployed by the telephone company (including at exchanges in other cities), and 33 in other industries—10 in retail, 8 in clerical jobs, 7 as private branch exchange 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, 6).

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.

VI.A. 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 $\leqslant$ 100,000 in 1920, we estimate the effects of a cutover in the intervening decade on individual operators' outcomes 10 years later:

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

where Y_{ict}^{t+10} represents an outcome in year $t+10$ for a woman i who lived in city c in year t, $\mathbb{1}(\text{Post-Cutover})_{ct}$ 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.[23] In our most demanding specification, we replace the city-year fixed effects with operator-and-control-worker pair fixed effects, which conditions comparisons to be 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).

VI.B. 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-

23. 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.

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

TABLE IV
CONTINUED

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

TABLE IV
CONTINUED

	All Ages		16–20		21–25		26+	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
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. The table reports the 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. The sample is restricted to women in the small-city sample (population $\leqslant$ 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 .1, .05, and .01 levels, respectively. Standard errors clustered by city are in parentheses.

level controls. Odd-numbered columns add city-year fixed effects, and even-numbered columns operator-year fixed effects.

Echoing our results from Section IV, 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 percentage points less likely to be operators 10 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 noncutover 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 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 percentage points less likely to be working after a cutover, relative to peers in untreated cities—accounting for more than half of the displacement of operators in this age group. However, cutovers had smaller and less statistically precise effects on younger women's employment (those under 25 in the base year).

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 and household work may have been an alternative to formal employment 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

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

TABLE V
CONTINUED

	All ages		16–20		21–25		26+	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel B: Got married? (conditional 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

TABLE V
CONTINUED

	All ages		16–20		21–25		26+	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel C: Had children? (conditional 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. The table reports the 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. The sample is restricted to women in the small-city sample (population $\leqslant$ 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 .1, .05, and .01 levels, respectively. Standard errors clustered by city are in parentheses.

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 percentage points (or nearly 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.[24] 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.

In Online Appendix Tables D.2 and D.3, we 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 away or whether they were more likely to be living in a different city, local labor market, or state than they were 10 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 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

24. We study whether operators in year t were in higher- versus lower-paying jobs 10 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.

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? (conditional 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

TABLE VI

CONTINUED

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

TABLE VI
CONTINUED

	All ages		16–20		21–25		26+	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
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. The table reports the 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. The sample is restricted to women in the small-city sample (population $\leqslant$ 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 .1, .05, and .01 levels, respectively. Standard errors clustered by city are in parentheses.

is more surprising. The 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.

VII. Effects on Future Cohorts of Young Women

The evidence in Sections IV and V 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 V, and evaluate how telephone automation affected local labor markets.

VII.A. 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 (16–25, 16–20, and 21–25). 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 effect on fertility for the youngest women in our sample.[25] 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.

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 Online Appendix C.6). To do so, we construct shift-share instruments to project local employment growth from local industry

25. Follow-up analysis on the marginal fertility result for the youngest age group 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.

TABLE VII

CHANGES IN WORK, EDUCATION, MARRIAGE, AND FERTILITY PATTERNS AROUND CUTOVERS

	Percent of the group that is:				
	Tel. oper.	Working	In school	Married	Has children
Panel A: White, American-born women ages 16 to 25					
Post-cutover	−0.66***	0.03	0.12	0.08	0.22
	(0.05)	(0.43)	(0.25)	(0.30)	(0.21)
N	113,752	113,752	113,752	113,752	113,752
R^2	0.42	0.83	0.95	0.95	0.93
Cities	2,845	2,845	2,845	2,845	2,845
Cutover	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					
Post-cutover	−0.75***	−0.01	0.25	−0.04	0.24*
	(0.08)	(0.57)	(0.46)	(0.25)	(0.12)
N	56,884	56,884	56,884	56,884	56,884
R^2	0.45	0.86	0.92	0.90	0.85
Cities	2,845	2,845	2,845	2,845	2,845
Cutover	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					
Post-cutover	−0.57***	0.08	−0.01	0.20	0.21
	(0.05)	(0.46)	(0.13)	(0.38)	(0.31)
N	56,868	56,868	56,868	56,868	56,868
R^2	0.36	0.74	0.75	0.82	0.79
Cities	2,845	2,845	2,845	2,845	2,845
Cutover	261	261	261	261	261
Y mean	1.09	43.66	3.85	53.55	32.60

Notes. The 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 in the labor force, in school, married, and have children, for cities with population $\leqslant$ 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 × year controls. *, **, *** represent significance at the .1, .05, and .01 levels, respectively. Standard errors clustered by city are in parentheses.

employment shares in each census year and next-decade, leave-one-out national industry growth rates.[26] We control for this variable in levels and in percentiles (which compresses outliers). In both cases, our results are unchanged. We also control for cities'

26. 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).

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. Online 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 to dial for at least another decade. We find no systematic differences in employment rate changes in the run-up to cutovers. In Online 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 $\geqslant$ 20 years prior to cutovers and are unlikely to be directly related. We also undertake DID due diligence in Online 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 VI), then local employment rates might have been sustained by selective outmigration. 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 National Industrial Conference Board (1926), and data on the most common occupations for young women from the complete count data itself (Online 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 (Online 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

TABLE VIII

CHANGES IN EMPLOYMENT SHARES IN SELECT OCCUPATIONS AROUND CUTOVERS

	Conditional on working, percent employed as or in							
	Tel. oper.	Off. mach.	Typist/secr.	Office clerk	Sales clerk	Beautician	Waitress	Ln(Occscore)
Panel A: White, American-born women ages 16 to 25								
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	111,485	111,485	111,485	111,485	111,485	111,485	111,485	110,671
R^2	0.37	0.42	0.61	0.53	0.41	0.46	0.49	0.76
Cities	2,845	2,845	2,845	2,845	2,845	2,845	2,845	2,845
Cutover	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								
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	54,997	54,997	54,997	54,997	54,997	54,997	54,997	54,421
R^2	0.38	0.37	0.61	0.51	0.42	0.41	0.46	0.72
Cities	2,845	2,845	2,845	2,845	2,845	2,845	2,845	2,845
Cutover	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

TABLE VIII
CONTINUED

	Conditional on working, percent employed as or in							
	Tel. oper.	Off. mach.	Typist/secr.	Office clerk	Sales clerk	Beautician	Waitress	Ln(Occscore)
Panel C: White, American-born women ages 21 to 25								
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	56,488	56,488	56,488	56,488	56,488	56,488	56,488	56,250
R^2	0.32	0.47	0.58	0.55	0.38	0.48	0.55	0.58
Cities	2,845	2,845	2,845	2,845	2,845	2,845	2,845	2,845
Cutover	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. The table presents 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 $\leqslant$ 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: office machine operators; typists, stenographers, and secretaries; other office clerks; sales clerks; beauty parlor workers; and restaurant workers. The final column estimates effects on (log) average occupation scores. All regressions include city and year fixed effects, and log city size $\times$ year controls. *, **, *** represent significance at the .1, .05, and .01 levels, respectively. Standard errors clustered by city are in parentheses.

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.[27] 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 industry jobs, like restaurant work.[28] 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.

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

VII.B. Why Was Employment So Stable?

Why did local labor markets adjust so smoothly to the automation of telephone operation? We explore six possibilities. We consider whether our results can be explained by inelastic supply, examining whether (i) the labor market reequilibrated at lower wages, or (ii) the influx of would-be operators into substitute occupations displaced other demographic groups

27. The effect of cutovers on telephone operation employment in column (1) of 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.

28. 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 $1.52 \times \frac{11.61}{100-1.52} = 0.179$ percentage points (versus the estimated 0.54 percentage points), and the share in restaurant work to increase by $1.52 \times \frac{4.15}{100-1.52} = 0.064$ percentage points (versus 0.83 percentage points). 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.

from these jobs. We 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 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 (Acemoglu and Restrepo 2019a).

1. *Supply-Based Explanations.*

i. *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.[29] We first calculate individuals' weekly wage, as census-reported 1939 wage income over 1939 weeks worked. Then we 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 versus older white, American-born women in cities with and without cutovers (exploiting their differential exposure to the telephone industry's automation); and (iii) young, white, American-born women versus men.

29. 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.

Across these tests, we do not find evidence of systematic wage declines or differences in relation to cutovers (Online Appendix E.1.1). These results should be interpreted with some caution, given standard errors (we cannot statistically rule out wage changes of $\pm 10\%$), which may be due to heterogeneous effects, noise in the wage data, or more limited power afforded by a smaller set of cutovers (72 versus 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 that 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.

ii. *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 effect 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 (Online Appendix E.1.2).

2. *Demand-Driven Explanations.*

i. *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 nonautomated 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

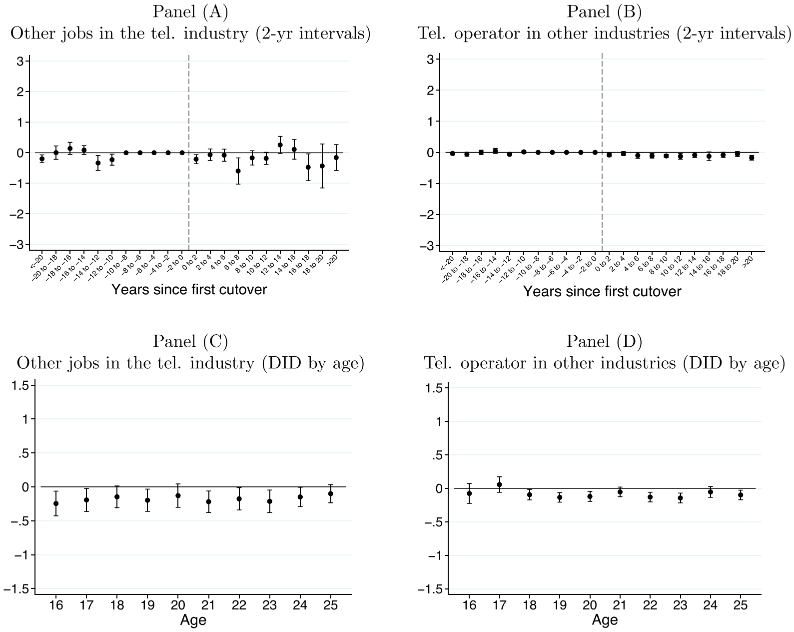

FIGURE V

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

Panels A and B 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 (A) and who are telephone operators in other industries (B), for the small-city sample (population $\leqslant$ 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). Panels C and D show the associated staggered difference-in-differences estimates by age. We plot the estimates on the same scale (-3 to 3 percentage points) as the previous figures to ease comparison. Error bars represent 95% confidence intervals, computed from standard errors clustered at the city level.

telephone industry or telephone operator jobs in non-telephone industries.[30]

30. 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.

ii. *Productivity growth and scale effects*. We also consider whether mechanical switching may have had wider productivity effects (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, but rather used the capital investment as a rationale for requesting local regulatory approval of higher telephone rates; see Online Appendix Figure A.4 for examples from newspapers). The technical efficiency savings of dial service also appear to be minuscule: in Online 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 that productivity gains from cheaper or improved telephone service, and any resulting expansion in output and increase in labor demand, can explain our results.

iii. *Contemporaneous technological change*. We consider whether other technologies might have coincided with cutovers and offset their effects. We focus on electricity and motor vehicles, each of which diffused rapidly between 1900 and 1940 and had significant effects on the organization of production. To evaluate whether these changes coincided locally with telephone industry automation, in Online 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, for example, 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.

iv. *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 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, 206). 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.[31]

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 that 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 (notetaking) 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 concurrent paper, Autor et al. (2024) study the emergence of new work by measuring new titles listed in government occupational dictionaries, many of which arise as specialized variants of existing titles. Motivated by their work and by the medical stenographer example (and others like it), we use our data to study the emergence of new occupation-industry pairings:

31. 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.

the proliferation of specific types of work to new industries.[32] 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 reflects invention or diffusion (both are consistent with theory).

The data offer some descriptive clues. For example, secretarial work was broadening in this era: in 1910, the top five 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% in every decade), but were growing quickly in drug stores, a new setting.

In Online 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 that 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 $\times$ 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.[33]

32. 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. (2024)'s use of occupational dictionaries. We believe occupation-industry pairings are more cleanly measured and provide similar information.

33. 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 above and in Online Appendix E.1.1 suggests against this possibility, data challenges limit strong conclusions. Moreover, reinstating demand growth and wage declines could each be present in different occupations.

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 become obsolescent. 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 Online 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 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.

VII.C. Connecting the Results for Incumbents and Future Cohorts

Taken together, the results of Sections VI and VII suggest that 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, 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 effects 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.

VIII. 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 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 upward 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 in 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—that is, 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 that 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.

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 twentieth 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 effects of automation in our setting—sharpening or attenuating its effects. The racial and class dynamics in the United States 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 effect 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), employment 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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Supplementary Material

An Online Appendix for this article can be found at the *Quarterly Journal of Economics* online.

Data Availability

The data underlying this article are available in the Harvard Dataverse, https://doi.org/10.7910/DVN/SM89VW (Feigenbaum and Gross 2024).

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