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Does Census Hiring Stimulate Jobs Growth?

Abstract

Governments perform national, labor-intensive censuses on a regular schedule. Censuses represent many of the largest peacetime expansions and contractions in federal hiring. The predetermined occurrence and scale of the census offers an economic experiment in the effects of temporary government hiring. This paper describes the construction of a data series on census hiring in the United States since 1950 and also collects available data on census employment in England and Wales, Canada, Korea, and Japan. Regressing total employment changes on census hiring yields coefficients extremely close to 1, indicating that there is no spillover from census hiring to the rest of the economy. Using census hiring and occurrence as instruments for government hiring in the US, Canada, and Korea, I estimate the effect of federal hiring on overall employment. Different samples yield varying jobs multipliers, with point estimates varying from -0.01 to 1.48. Including Korean and Canadian data yields lower multipliers, while including pre-1990 US data yields higher multipliers. In no specification can I reject the hypothesis that the job multiplier equals 1. In all specifications, standard errors are large enough that I can reject neither Keynesian nor crowd-out effects.

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1 Introduction

The Constitution of the US requires the federal government to perform a census every 10 years. In the US and elsewhere, such regular censuses have been undertaken predictably decade after decade, regardless of economic conditions. Although most censuses use the postal system to distribute and collect survey forms, follow-up is typically carried out by an army of door-to-door enumerators. In the US, nondefense government spending grows rapidly (about 4%) in census quarters and falls in the quarter after each census (about −5%). The six largest 1-month increases in US federal civilian employment since 1948 all took place during census buildups; four of the six largest 1-month declines took place during census drawdowns. For economists interested in the effects of government employment programs, censuses provide a rare source of exogenous variation.

This paper presents newly compiled data on census employment in the US, Canada, Korea, the UK, and Japan, to varying degrees of detail. The data were gathered from archival sources and personal correspondence with census officials.

Temporary hiring of low-skilled individuals is often proposed as a policy to alleviate unemployment or boost economic activity through a Keynesian multiplier effect. Critics contend that government activity will crowd out private-sector activity. This paper uses data from Canada, Korea, and the United States to test the null hypothesis that temporary federal hiring has no effect on non-federal employment. In most specifications, standard errors (SEs) are wide enough that the null cannot be rejected; nor can we reject substantial Keynesian effects or perfect crowd out. The point estimates suggest mild crowding out of non-federal hiring.

2 Literature

This is the first effort of which I am aware to quantify and investigate the impact of temporary census hiring across a broad panel. However, the present work rests entirely on the shoulders of census administrators and historians who compiled the procedural reports on which the data below are based.

The literature on the effects of federal spending on economic outcomes, by contrast, is massive and has been a point of contention since Keynes. Keynesian theories emphasize the impact of government demand and its potential to expand or contract the broader economy, at least in the short run. "[T]he analysis of The General Theory is limited to a time period short enough that the changes in capital stock resulting from non-zero investment can be ignored" (Tobin, 1993). Competing neoclassical theories emphasize that the size of the economy is limited by the supply of productive factors.

There are many empirical estimates of the effect of a dollar of additional government expenditure or taxation on broader economic activity, usually gross domestic product (GDP) or a similar concept. It is useful to think of this fiscal multiplier as having a baseline of 1: if government spending changes nothing else in the economy whatsoever, GDP – by construction – rises dollar for dollar with government spending. If the multiplier is less than 1, government spending discourages private economic activity at the margin; if the multiplier is greater than 1, government spending increases private economic activity.

A recent surge in research on fiscal multipliers has substantially improved the precision of estimates. Ramey (2019) comes "to the surprising conclusion that the bulk of the estimates for

average spending ... multipliers lie in a fairly narrow range, 0.6 to 1". By contrast, in her 2011 review, Ramey (2011) noted that "most aggregate studies estimate a range of multipliers from around 0.6 to 1.8. Moreover, the range within studies is almost as wide as the range across studies, and the standard errors are always large". An earlier survey by Hemming, Kell, and Mahfouz (2002) found most multipliers distributed between 0.6 and 1.4.

Estimates of fiscal multipliers are regularly used in policy debates, and not always precisely. For example, Romer and Bernstein (2019) used a multiplier estimate associated with a permanent, debt-funded increase in government purchases in arguing for the American Recovery and Reinvestment Act (ARRA) although the ARRA was temporary and included tax breaks and transfers as well as government purchases.

Unlike the bulk of the literature, I estimated a jobs multiplier rather than a fiscal multiplier. In this I followed Moretti (2010) measuring the net employment effect of an additional, exogenous, temporary government job on total employment. Others, including Conley and Dupor (2011) and Wilson (2011), have estimated "spending-to-jobs multipliers", instrumenting for government spending and measuring the employment response.

An advantage of using employment rather than output measures is that the former are measured at a monthly frequency, which allows the researcher to follow the rapid expansion and contraction in hiring that accompanies the census. A disadvantage is that the competing Keynesian and neoclassical theories, and accompanying empirical work, are generally expressed in fiscal terms, but jobs multipliers cannot be readily converted into spending multipliers. The reverse, however, is also true, and the jobs multipliers discovered in the present work may be of more direct interest than GDP multipliers to policymakers whose overriding concern is unemployment.

A separate literature on active labor market programs (ALMPs) is marked by a "dichotomy in the empirical results with micro studies often finding no effect of labor market programs ... while macro studies conclude that these programs do reduce the unemployment rate" (Boone and van Ours, 2019, p. 295). In a widely cited macroeconomic analysis of labor policies, BW (2000) found that aggregate spending on ALMP decreases the unemployment increase following adverse macroeconomic shocks.

However, direct public hiring is relatively rare and rarely studied compared to other forms of ALMPs. YMS (2019) identified only three randomized control trials of hiring into "public works" among 102 studies they reviewed. Bonoli (2010) argued that direct public hiring was relatively popular in Europe in the 1970s and fell out of favor in the 1990s. Bown and Freund (2019) summarized evidence from European direct hiring programs and found that all were abandoned as failures.

The present paper performs a macroeconomic evaluation of direct public hiring. However, the census should not be categorized as an ALMP. Many of those hired as enumerators remain employed at a primary job throughout their census stint or come from – and return to – retirement or schooling. The impact of the census on unemployment will be muted because most of those hired, by all indications, do not come from unemployment.

3 **Data**

The following subsections present data for five developed countries for which I found records of employment in censuses. Most European countries moved away from regular censuses in the

last several decades, using continually updated registers or rolling surveys instead of exhaustive enumeration.

3.1 **United States**

In order to correctly apportion representatives, the US Constitution requires that "Enumeration shall be made... within every subsequent Term of ten Years" (US Constitution, Article I, Section 2). In keeping with this requirement, the United States has held a census every 10 years since 1790. As in other countries, the format has evolved steadily (Ruggles and Magnuson, 2020).

Detailed data on US decennial census employment, 1950–1970, are available in US Census Bureau publications, although interpolation is necessary. Procedural histories of the 1940 and 1980 censuses report total and/or peak employment but give no guidance on the time path of employment changes. In 1940, "[there] were approximately 120,000 enumerators employed" (Robert Jenkins, 1983, Procedural History of the 1940 Census of Population and Housing, Center for Demography and Ecology, University of Wisconsin, p. 25). The Bureau of Labor Statistics (BLS) published a separate series on temporary census employment for the 1990–2010 censuses. Sources are listed in Appendix A.

In order to use the data on training and employment of temporary employees in the 1950–1970 censuses, I normalize it to match the employment concept of the Current Employment Statistics (CES) survey. The CES intends to count all workers who were employed during the pay period that includes the 12th day of each month. In order to construct a census employment series that matches the CES, I interpolated day-by-day census employment from Census Bureau sources. Then, I matched the federal pay periods and used the highest employment day during the pay period containing the 12th of the month as monthly census employment. The census-year data are shown in Table 1. The resulting series yields month-over-month changes that are correlated with month-over-month changes in federal employment at 0.49 for 1948–1972. By comparison, changes in census employment almost perfectly match changes in federal employment (correlation = 0.97) from 1990 onward.

Table 1 US census employment since 1950

Month	1950	1960	1970	1990	2000	2010
January	2,161	0	524	4,000	32,000	24,000
February	2,949	39	7,549	15,000	59,000	39,000
March	11,382	10,792	20,042	68,000	154,000	87,000
April	143,789	177,706	160,585	153,000	181,000	154,000
May	65,716	66,490	130,669	335,000	530,000	564,000
June	12,967	18,529	65,353	251,000	305,000	339,000
July	8,015	11,511	24,316	179,000	232,000	196,000
August	8,336	10,887	11,662	81,000	146,000	82,000
September	8,662	10,297	8,347	44,000	24,000	6,000
October	8,931	9,905	6,517	27,000	11,000	1,000
November	9,240	9,578	5,088	19,000	6,000	0
December	9,446	9,089	3,973	13,000	1,000	0

Sources of census employment are listed in Appendix A. For 1950–1970, data are interpolated from available sources to match the federal pay period including the 12th day of each month. Only the calendar year of each census is shown here; the full data cover as many as 56 months for each census.

For 1950, the procedural history lists month-end enumerator employment during the key months and quarterly employment updates from 1948 to 1952. I used linear piecewise interpolation to fill in the daily numbers.

For 1960, a Census Bureau memorandum lists weekly (or better) enumerator employment from April 4 to July 11. I used piecewise interpolation during that period to create daily employment estimates. In order to code the buildup, I used training schedule data, which indicates that enumerators were trained en masse on March 27 and 28. A few thousand other employees were trained in advance. I added these other employees to the enumerators and patterned their drawdown after the duration of post-enumeration employment in 1950.

For 1970, I used the detailed training schedule to construct the buildup in employment. I found that trainees exceeded by 19% the peak employment number given in the Census Procedural History. In order to reconcile these figures, I included a daily attrition factor, calibrated to match peak employment. The calibrated rate amounted to 23% attrition per month. Attrition alone cannot match the speed of the post-peak drawdown in census employment, so I used the 1960 drawdown to guide estimates of the decline in employment and verify that the results are consistent with the office closure calendar given in the 1970 Procedural History.

The Procedural History of the 1980 census, which I did not use in the empirical estimation, noted that "460,000 persons were employed at one time or another in the census district offices, with 270,000 working at the peak of activities in April and May 1980" (1980 Census of Population and Housing: History, *U.S. Bureau of the Census*, pp. 1–14). Although this provides no evidence on the time path of employment, it is useful to note the size of the discrepancy between total and peak employment.

For 1990–2010, I made no adjustments to the BLS data. It is probable that some unrecorded temporary census employment occurred in 1989 and 1991, although none was published for those years by BLS. However, I made no attempt to estimate census employment in those years.

Comparing the extrapolated data to federal employment data, I found that significant discrepancies remain. The census creates a large and easily identifiable bump in federal employment. However, the biggest increase (or decline) in CES federal employment often occurs in a different month than it does in census employment in the series I constructed.

The 1960 census, shown in Table 2, exemplifies the discrepancy. Federal employment in the CES increases by 189,000 in March, but the Census Procedural History indicates that the enumerators were not trained until the 27th of March – well after the Federal pay period of record.

Table 2 Discrepancies in 1960 data

Month	Constructed change in Census employment	Change in CES Federal employment
February 1960	39	1,000
March 1960	10,753	189,000
April 1960	166,914	−12,000
May 1960	−111,216	−124,000
June 1960	−47,960	−34,000
July 1960	−7,018	−7,000

Sources of census employment are listed in Appendix A. Federal employment source is Current Employment Statistics, US Bureau of Labor Statistics. CES, Current Employment Statistics.

One possible explanation for the discrepancy is that census enumerators are not paid as regular federal staff. Thus, they may be counted using a different pay period. Another explanation is that the CES series may preserve anachronistic reporting of temporary Census Bureau employees. Until 2002, federal employment data were provided to the CES by the Office of Personnel Management and did not correspond to the usual CES definition. In 2002, the historical series of employment was rebuilt from employment records. However, for years prior to 1975, CES did not have the records with which to rebuild the series; instead, they relied on existing data and a benchmark.

In order to test for either of these possibilities, I constructed a second monthly series of census employment for 1948–1972, in which I used the entire month as the pay period. I find that this alternative census series better matches the CES federal employment series than my principal series. In Section 7.5, I showed that the results using the alternative series are not substantially different than the baseline results.

3.2 **Canada**

Canada has held a decennial census since 1871 and a quinquennial census since 1956. Table 3 gives a published estimate of employment for all but four censuses since 1871. The census shifted to "self-enumeration" in 1971, but the change did not lead to an immediate decrease

Table 3 Historic census employment, Canada

Month	Census commissioners	Crew leaders	Enumerators	Other positions	All
1871	206		2,789		
	205		3,183		
	241		4,324		
	351		8,800		
	264		9,703		
	241		11,425		
	254		13,886		
			15,308		
			16,850		
	263	526	15,011		
	1,300		30,000		
			35,000		
			41,208		
					No data
					No data
			44,042		
					No data
					38,000
					38,000
		1,800	20,000		
		5,000	30,000		
		4,000	25,000	6,000	

Sources are listed in Appendix A. Census handbooks and other sources on the 1976, 1981, and 1991 censuses did not contain employee counts. "Crew leaders" were called "field supervisors" in the 1956 census. Enumerators were called 'census representatives' in some censuses.

Table 4 Canada census employment, monthly

Month	2006	2011	2016
January		10	245
February		158	517
March		262	1,566
April	14,391	1,637	15,017
May	21,230	15,142	32,715
June	20,058	30,707	30,850
July	13,853	28,513	20,507
August	5,469	21,529	2,353
September	555	772	74
October	12	11	
November		17	
December		4	

Source: Jane Lavigne, Statistics Canada, personal communication.

in employment intensity. Unlike in the US, however, the temporary workforce has declined modestly in the 21st century.

Jane Lavigne of Statistics Canada provided monthly employment data for the Canadian censuses of 2006, 2011, and 2016 in personal communication (2012, 2016). These data are presented in Table 4 and show that the timing of census employment has shifted around among recent censuses. The data may not be perfectly comparable: the 2006 data are based on the week with the highest payroll within the month. For the record, those were the weeks of April 23, 2006; May 21, 2006; June 4, 2006; July 2, 2006; and August 6, 2006; the 2011 and 2016 data were transmitted as monthly counts of unique employees.

Unlike in the case of the US., there is not a single, clearly preferred survey of employment in Canada, nor is there a single series for federal employment. To analyze the potential impact of censuses, one must either mix and match data sources or restrict oneself to a shorter analytical period.

The most commonly used measure of broad employment is the Labour Force Survey (LFS), a household survey (which thus counts employed persons, not jobs). The LFS does not provide federal employment counts, but it has a long series.

The database CANSIM2 supplied data on the federal government workforce in Canada from 1981 to 2012, at which point the series was discontinued.

In addition, since 1991 Canada has conducted the Survey of Employment, Payrolls and Hours (SEPH). SEPH provides total and federal employment figures and measures of employees rather than persons employed. SEPH covers a smaller universe, counting 11% to 18% fewer jobs than the LFS count of people employed. SEPH counts 24% to 32% fewer federal jobs than CANSIM2. SEPH's methodology changed in 2001, and Statistics Canada has published many SEPH series beginning in 2001.

Although SEPH is a census, not a sample, Statistics Canada wrote, "This survey is a census with a cross-sectional design. Data are collected for all units of the target population; therefore, no sampling is done ... Responding to this survey is mandatory". It has flaws that may be responsible for its rare use in research. Google Scholar returns 25,100 results for the search *Canada "Labour Force Survey"* and only 313 for *Canada "Survey of Employment, Payrolls*

and Hours" (Google Scholar web search, performed by the author, October 4, 2018.). Stephen Gordon wrote, "Like almost everyone else, I've been treating the SEPH as almost an afterthought ... But now that I've taken a closer look, I'm going to start making a bigger deal of the SEPH numbers from now on" (Gordon-SEPH). It appears to be noisier than LFS, the reverse of the situation for the analogous surveys in the US. From January 1991 to March 2012, the broadest employment series (seasonally adjusted) of the US CES payroll survey has monthly first-difference standard deviation of 0.18%, while that of the US Current Population Survey is somewhat higher, at 0.25%. In Canada, however, the broadest employment series of SEPH has a first-difference standard deviation of 0.33% and that of LFS has 0.20%. The patterns persist in non-seasonally adjusted data. Of course, higher variance does not imply a lower signal-noise ratio, but lower variance is associated with the better US survey and with more recent data; older data in all surveys have higher variance.

The choice of broad employment series may significantly impact regression results. The dissonance between household survey and payroll survey is greater in Canada than in the US: monthly first differences in the American series are correlated at 0.48 over 1991–2012, while differences in the Canadian series have a correlation of only 0.33. However, the two series for federal employment are harmonious: monthly changes in CANSIM2 federal employment are correlated at 0.89 with those of the SEPH federal employment series even though the CANSIM2 covers a universe 40% larger.

In the subsequent empirics, I used LFS data for total employment. For federal employment, I relied on CANSIM2 data from 1981 to 2000 and SEPH data from 2001 to 2016.

3.3 **Korea**

The Republic of Korea has held a census of population and housing census every 5 years since 1970; prior to that date, there was some irregularity in the periodicity of the census. Statistics Korea has provided data on the number of enumerators and full-time government employees who were recruited to work on each census from 1990 to 2010 (personal communication, 2013, 2016). However, not all of those recruited in fact worked; some of them remained on a waiting list. The Comprehensive Plan of the 2015 Census reported that expected hiring was much lower than the earlier numbers (see Table 5).

The Korean census takes place in November. Statistics Korea indicates that the temporary workers are chosen and screened in September and some of them may work in October or

Table 5 Korea census hiring and reassignment

Year	Temporary employment	Reassignment of public servants
1990	116,000	55,000
1995	126,000	57,000
2000	190,000	16,000
2005	110,000	5,000
2010	113,000	6,000
2015	44,000	6,228

Note that 2015 figures are planned, not necessarily realized, and pre-2015 figures include waitlisted recruits. Sources: Statistics Korea, personal communication and 2015 Comprehensive Plan.

December (personal communication). The 2015 Census Comprehensive Plan offers a detailed schedule of the enumerators' training schedule. For the purposes of the Economically Active Population Survey (EAPS), they are only counted as employed if they are working as of the reference day, usually the 15th of a given month.

Employment data for Korea come from EAPS, which provides monthly data since 1963. However, data disaggregated to the sector of interest – "Public Administration and Defense; Compulsory Social Security" – are only available from 1992. As the Korean data are not seasonally adjusted, I used the X-12-ARIMA utility to adjust the data. Korea has used government hiring very aggressively in the major crises of 1998 and 2009, and those large shifts in public employment may bias nearby seasonal adjustment factors.

The Public Administration data show clear increases in November for the censuses of 2005, 2010, and 2015. However, for 1995 and 2000, the series is flat through the census months. This could be due to the timing of the census or due to a change in whether temporary census workers are counted as Public Administration employees. With insufficient knowledge of the earlier censuses, I elected to use Korean data only subsequent to the 2000 Census.

The 2015 census may prove to be the last traditional census in the Republic of Korea: in 2016, Statistics Korea introduced a register-based census, augmenting a 20% long-form field sample with "register" data from other public records for the rest of the population (Statistics Korea, 2018).

3.4 **England and Wales**

Data on census employment are available earlier in Britain than in any other country and provide an unparalleled view of the historical evolution of the labor intensity of enumeration. Thus, I included this section even though a lack of data granularity prevents me from using British data in the empirical part of this paper.

The 1841 census of England and Wales was the first to require "that the business of numbering the people should be completed 'in one day', in order to obviate the chance of inaccuracy from omissions or double entries to which the extension of the inquiry over a greater period might have given rise". In order to execute the census in a single day, it was necessary to appoint "no less than 35,000" enumerators (1841 Census of Great Britain, 1843).

British records, even in recent decades, do not give details of the hiring, training, and employment period for temporarily employed enumerators. The employment totals for every census of England and Wales from 1841 to the present, however, are recorded in Table 6. The number of enumerators remained roughly constant from 1841 to 1931, grew in the post-war period, and dropped to about 31,000 in 2011. Plans for the 2021 census anticipate that only 17,000 field staff will be hired, since most Britons will complete the survey online (HM Government, 2018, p. 76).

Without data on the timing of census employment, and with only quarterly data published on public employment in Britain, I did not use the census of England and Wales in regression analysis. Abstracts and general reports do give rich qualitative information about British censuses, although. The 1991 General Report, for example, discusses the interaction between census and other employment, noting both that "most field staff jobs could be carried out as a part-time activity by people already in regular full-time employment" and "strenuous,

Table 6 England and Wales Census employment

Year	Senior managers	Middle managers	Assistants	Enumerators
1841				35,000
1851	624	2,190		30,610
1861	634	2,194		30,329
1871	626	2,195		32,543
1881	630	2,175		34,711
1891	631	2,122		35,507
1901	635	2,064		38,200
1911	634	2,035		35,000
1921	637	1,913		38,563
1931		1,770		40,016
1951		1,225		49,318
1961	115	1,315		69,000
1971	100	1,997	6,127	96,741
1981	110	2,103	6,296	103,166
1991	135	2,539	7,724	117,696
2001	103	2,000	6,000	62,500
2011	157	2,194		31,436
2021 (anticipated)				17,000

Sources are listed in Appendix A. In early censuses, "superintendent registrars" and 'registrars' are the terms used for senior and middle managers, respectively. Data for 2021 are expected, not realized. Blank entries do not necessarily mean that no one was appointed to those roles.

but unsuccessful, attempts were made" to allow recruits on unemployment insurance to keep their unemployment benefits despite working temporarily for the census (Office for National Statistics, 1995, p. 49).

3.5 **Japan**

Japan has undertaken a population census every 5 years since 1920, including a post-war census in late 1945. Matsuda (1981) put the early population censuses in the context of a broader effort to create national and imperial statistical accounts. Japan's government employment data are not published at high frequencies, so Japan's censuses cannot be used in this paper's empirics.

However, the Statistics Bureau of Japan did note the number of enumerators employed in three recent censuses. In fact, Japan has the largest census labor force recorded: 830,000 enumerators conducted its 2000 and 2005 censuses. The labor force dipped to "about 700,000" for the 2010 census (Statistics Bureau of Japan, "Population Census," https://www.stat.go.jp/english/data/kokusei/index.html).

4 **Employment intensity**

Given the available data, I could not compare the labor intensity of censuses across time and countries. Methods of enumeration vary. Modern records make clear that census duties can be fulfilled during nights and weekends, but some "conventional" censuses focused a huge

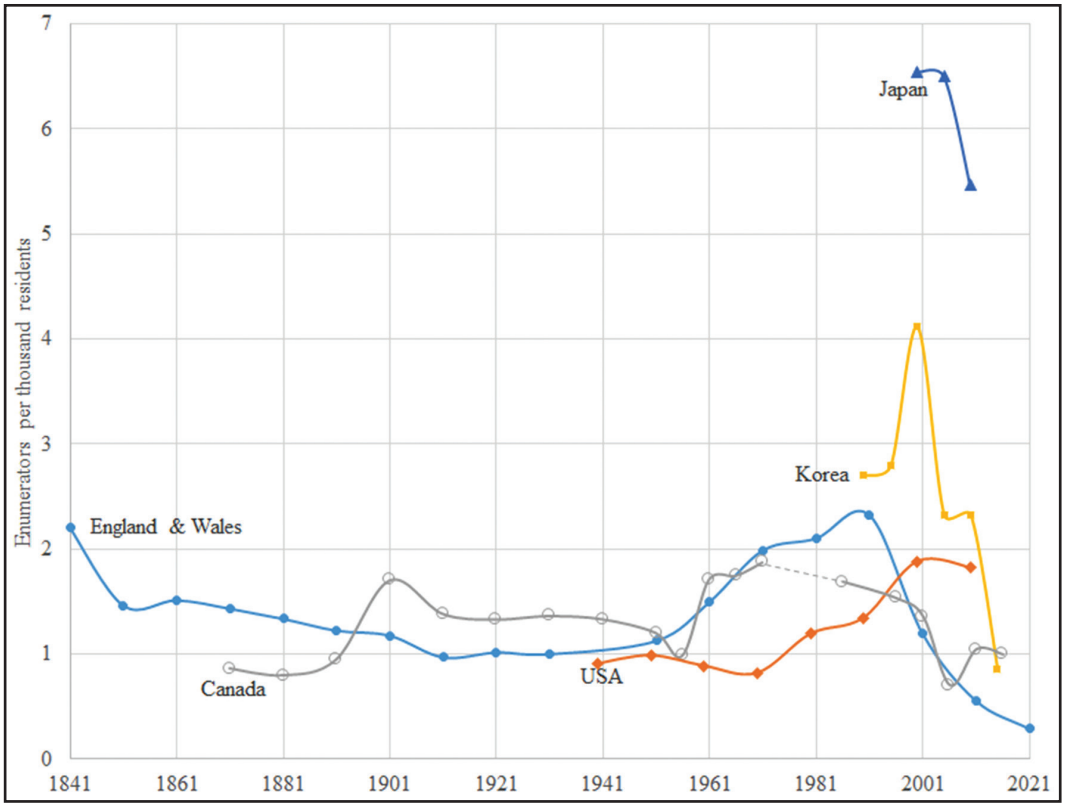
amount of effort on visiting every home on census day. In all four countries with data available, there is a clear trend toward a longer period during which census employment is elevated and in all but the US, a decline in peak employment as a share of the population. The overall trend in labor intensity is therefore unclear.

An exceptionally detailed procedural history of the 1950 US census records the cumulative man-hours worked each week, concluding at 6.6 million hours by July 7, 1950 (US Bureau of the Census, 1955, p. 202). With peak employment near 150,000, this implies that the average census worker totaled about 44 h, although the median worker likely put in far fewer hours.

What can be measured, however, is the approximate "employment intensity": the number of people hired as a share of the population enumerated. This measure is relevant because recruitment and training can be relatively large fixed costs for census employment. Figure 1 shows employment intensity (enumerators per thousand enumerated) for England and Wales, Canada, the US, Korea, and Japan across the timeframe for which data are available in each country. Sources can be found in Appendix A.

Japan's employment intensity far exceeds the others. Without Japan's data, it would be tempting to characterize employment intensity as relatively compact across time and countries. Instead, one can see that intensity varied by a factor of 6 for censuses undertaken around the year 2000. It is surprising that the gradual introduction of labor-saving methods (such as using the postal service to distribute and/or return surveys and offering an online option for Tokyo residents) did not further decrease Japan's employment intensity. By contrast, UK authorities expect the shift to a "digital-first" census in 2021 to cut the necessary field staff in half relative to 2011.

Figure 1 Employment intensity. See Appendix A for sources.



5 The employment effect of census hiring

Given that census employment growth accounts for most federal employment growth in its non-zero months and is plausibly exogenous, one may use ordinary least squares (OLS) directly to estimate the effect of census hiring on total jobs. The sample is limited to US data and the three most recent Canadian censuses. I estimated

$$\frac{\text{EMPLOY}_t - \text{EMPLOY}_{t-1}}{\text{EMPLOY}_{t-1}} = \beta_0 + \beta_1 \frac{\text{CENSUS}_t - \text{CENSUS}_{t-1}}{\text{EMPLOY}_{t-1}} + \epsilon_t \tag{1}$$

The "jobs multiplier" β_t is the effect on the growth of monthly total employment $EMPLOY_t$ of the growth in census employment, $CENSUS_t$. I expressed both growth rates as a share of total employment.

Regression cOLS1, shown in Table 7, shows that census hiring raises total employment almost exactly one for one. This result suggests that census hiring has almost no impact on the surrounding job market. Using US data only results in a very similar estimate. When using only high-quality US data (1990–2012), the multiplier estimate drops to 0.82. One cannot reject the hypothesis that the two estimates are equal. In unreported regressions, I replicated cOLS1 and cOLS3 using the alternative interpolation for the 1950–1970 US censuses and found extremely similar estimates.

The OLS results are unsurprising: even large-scale, rapid job creation is unlikely to have substantial ripple effects when the jobs created are temporary, low paid, and often part time. The subsequent sections use econometric methods to generalize the question at hand and expand the sample but ultimately fail to improve the precision or interpretation of results found by simple OLS.

6 Instrumental model

The previous section estimated the effect of census hiring on total employment. However with substantial uncertainty about the precision of census hiring statistics, one may wish to use the census data as an instrument for federal hiring. In addition, an instrumental approach allows me to include Korean and pre-2006 Canadian censuses for which no hiring data exist.

There are differences between the technically correct interpretations of the census OLS and the instrumental estimates: the former measure the effect of census hiring on total

Table 7 Total employment and census employment

Dependent variable	**dEMPLOY**		
Regression	**cOLS1**	**cOLS3**	**cOLS4**
US 1948–1972	Yes	Yes	
US 1990–2012	Yes	Yes	Yes
Canada	Yes		
Observations	239	208	88
dCENSUS	0.96 (0.42)	1.04 (0.46)	0.82 (0.23)
R^2	0.02	0.02	0.05
F-statistic	5.3	5.2	12.4

Note: Robust standard errors are in parentheses.

employment and the latter the effect of temporary changes in government employment on total employment. The fact that census jobs have lower monthly wages than the average job suggests that my estimates will have a bias toward 1 relative to the multiplier for median- or mean-wage temporary federal hiring.

Before turning to the instrumental setup, I estimate an employment multiplier in which government hiring may stimulate or crowd out private hiring. In the following equation, the "jobs multiplier" β is the effect on the growth of monthly total employment EMPLOY_t of the growth in government employment, GOV_t. I expressed both growth rates as a share of total employment.

$$\frac{\text{EMPLOY}_t - \text{EMPLOY}_{t-1}}{\text{EMPLOY}_{t-1}} = \beta_0 + \beta_1 \frac{\text{GOV}_t - \text{GOV}_{t-1}}{\text{EMPLOY}_{t-1}} + \epsilon_t \tag{2}$$

Owing to the simultaneity of government hiring decisions and macroeconomic conditions, OLS is a biased estimator. The OLS estimate of the equation above, in a panel of the US, Canada, and Korea using only census months, is $\beta_1 = 0.94$, with a robust country-clustered SE of 0.55. To maintain comparability with subsequent estimates, observations around the 1980 US Census are excluded. Remarkably, the point estimate is virtually identical when the same model is estimated on all months and years, including those between censuses, in Regression bOLS2.

A side note that deserves further study is that this unremarkable estimate masks divergence between the country-specific estimates. For the US alone, $\beta_1 = 1.60$ and SE = 0.59. For Canada, $\beta_1 = 0.09$ and SE = 0.26. For Korea, in an extremely small sample, $\beta_1 = 0.39$ and SE = 0.73. Throughout the paper, I omitted *P*-values and their conventionally associated asterisks from multiplier estimates. *P*-values are often unhelpful or distracting in the discussion of multipliers. Since federal jobs are mechanically included in total employment, a multiplier of one is a more natural null hypothesis than a multiplier of zero. These country-specific patterns persist when data from non-census years and months is included. The US series can be broken into a distinct high-coefficient period (1948–1977) and a low-coefficient period (1990–2012). In the earlier period, $\beta_1 = 2.00$ and SE = 0.93; in the more recent period, the coefficient falls to 0.79 and SE = 0.24. Biased OLS estimates are reported in Table 8.

Table 8 Biased OLS estimates

Dependent variable: dEMPLOY						
Regression	**bOLS1**	**bOLS2**	**bOLS3**	**bOLS4**	**bOLS5**	**bOLS6**
US 1948–1972	Yes	Yes	Yes			
US 1990–2012	Yes	Yes	Yes	Yes		
Canada	Yes	Yes			Yes	
Korea	Yes	Yes				Yes
Observations	271	1322	208	88	51	12
dGOV	0.94 (0.55)	0.95 (0.34)	1.61 (0.59)	0.77 (0.24)	0.09 (0.26)	0.69 (0.77)
Non-census years		Yes				
Controls		Yes				
R^2	0.05	0.08	0.08	0.05	0.00	0.09
F-statistic	3.0	.	7.5	11.0	0.1	0.8

Notes: Robust standard errors, clustered by country, are given in parentheses. Controls include country by month dummies, time trend, and log time trend. OLS, ordinary least squares.

7 Estimation

In order to control for simultaneity and autocorrelation of errors, I employed a two-step generalized method of moments (GMM) estimator with heteroskedasticity and autocorrelation correction and robust SEs. I used census employment as an instrument for federal employment wherever possible. In first-stage estimations, I found that census employment growth accurately predicts federal employment growth.

Where I lacked data on monthly census employment in Canada and Korea, I used indicator variables for the relevant census months. This approach to partially missing instruments is similar to the pooled instrumental variable (IV) estimation by ALS (2010). In addition to the pooled results, I reported GMM results by country. In order to isolate the effects of the bivariate interpolation used to create US data for 1950, 1960, and 1970. I also reported regression GMM4, which uses only US data from 1990 to 2012. I found that GMM4 is the most precisely estimated equation, with a robust SE smaller than those for estimates including less certain data.

For Canada's censuses prior to 2006, I used an indicator variable for the April, May, and June of census years. In the first stage of analogous IV regressions, the indicator coefficients suggest that federal employment rose by 0.06% points of national employment each census in April, rose by 0.16% points each census in May, and fell by 0.22% points each census in June. These are consistent with the records of peak census employment (see Table 3). Inspection of the federal employment series strongly suggests that census employment was much briefer prior to 2006, with a large drop occurring in every census in June from 1986 to 2001. From 2006, 2011, and 2016, the largest declines in both census and federal employment occur in August or September (see Table 4).

For Korea, I used indicators for October, November, December, and January around the 2005, 2010, and 2015 censuses. Since the recorded employment in the 2015 census was much lower than in the previous two, I scaled the indicator variables down by a factor of 0.5 in 2015. If the indicator variables for 2015 are left equal in magnitude, the first-stage coefficients are smaller and the SEs are about twice as large, indicating that the scaling improves the first-stage fit. The first-stage IV results imply a census-related increase of about 65,000 Public Administration employees in 2005 and 2010 and 35,000 workers in 2015. All the first-stage results are reported in Table 10.

My baseline regression is a pooled GMM estimation of all three countries. The instrumented and dependent variables are roughly equivalent across countries and time periods, but the instruments are temporally disjoint for Canada.

For additional controls, I used country-by-month fixed effects, country-specific time trends, and country-specific log time trends. These added relatively little explanatory power, and I reported regressions with and without the controls.

7.1 Results

The regression results are imprecisely estimated. In my most accurately estimated regression, the point estimate is 0.80, indicating mild crowding out. The 95% confidence intervals of various specifications can never reject 0.80. The confidence intervals are consistent with the recent multiplier literature summarized in Ramey (2019). Regressions GMM1 and GMM2, which use all the data available, point to mild crowding-out effects and cannot rule out mild Keynesian effects or severe crowding-out effects.

Table 9 Main GMM results

Dependent variable: dEMPLOY						
Regression	**GMM1**	**GMM2**	**GMM3**	**GMM4**	**GMM5**	**GMM6**
US 1948–1972	Yes	Yes	Yes			
US 1990–2012	Yes	Yes	Yes	Yes		
Canada	Yes	Yes			Yes	
Korea	Yes	Yes				Yes
Observations	271	1,322	208	88	51	12
dGOV	0.72 (0.66)	0.68 (0.44)	1.48 (1.11)	0.80 (0.35)	0.38 (0.70)	−0.01 (0.53)
Controls		Yes				
Non-census years		Yes				

Notes: Intercept terms are country specific. Robust standard errors are given in parentheses. Controls include month dummies, time trend, and log time trend, all interacted with country. GMM, generalized method of moments.

Table 10 First-stage instrumental results

Dependant variable: dGOV						
Regression	**IV1, first**	**IV2, first**	**IV3, first**	**IV4, first**	**IV5, first**	**IV6, first**
US 1948–1972	Yes	Yes	Yes			
US 1990–2012	Yes	Yes	Yes	Yes		
Canada	Yes	Yes			Yes	
Korea	Yes	Yes				Yes
Observations	271	1322	208	95	51	12
dCENSUS	0.69 (0.01)	0.71 (0.00)	0.7 (0.17)	1 (0.02)	0.63 (0.12)	
Canadian Census April	0.0006	0.0007			0.0006	
	(0.0000)	(0.0000)			(0.0005)	
Canadian Census May	0.0016	0.0017			0.0017	
	(0.0000)	(0.0000)			(0.0006)	
Canadian Census June	-0.0022	-0.0021			-0.0021	
	(0.0000)	(0.0000)			(0.0005)	
Korean Census October	0.0002	0.0004				-0.0002
	(0.0000)	(0.0000)				(0.0008)
Korean Census November	0.0026	0.0027				0.0022
	(0.0000)	(0.0000)				(0.0008)
Korean Census December	-0.0016	-0.0010				-0.0020
	(0.0000)	(0.0000)				(0.0008)
Korean Census January	-0.0011	-0.0006				-0.0014
	(0.0000)	(0.0000)				(0.0017)
Non-census years		Yes				
Controls		Yes				
R^2	0.51	0.26	0.35	0.98	0.61	0.69
F-statistic	1.6E+06	3.9E+15	17.6	2823.4	12.6	58.2

Notes: Robust standard errors, clustered by country, are given in parentheses. Controls include country by month dummies, time trend, and log time trend. IV, instrumental variable.

The regressions are reported in Table 9 and summarized in the following, and the first-stage IV results are reported in Table 10.

- Regression GMM1: All countries, including interpolated data, no controls. Multiplier estimate: 0.72. Confidence interval: [−0.58, 2.01].

- Regression GMM2: All countries, including interpolated data, including non-census months and years, country by month and time trend controls. Multiplier estimate: 0.68. Confidence interval: [−0.18, 1.53].
- Regression GMM3: US only, including interpolated data. Multiplier estimate: 1.48. Confidence interval: [−0.70, 3.67].
- Regression GMM4: US only, data after 1982. Multiplier estimate 0.80. Confidence interval: [0.11, 1.49].
- Regression GMM5: Canada only. Multiplier estimate: 0.38. Confidence interval: [−0.99, 1.76].
- Regression GMM6: Korea only. Multiplier estimate: −0.01. Confidence interval: [−1.05, 1.04].

The regressions suggest that Canadian and Korean data lead to a lower multiplier estimate, interpolated (pre-1973) US data lead to a higher estimate, and post-1989 US data give an intermediate result close to the overall estimate. Thus, whether one uses all the information available (Regressions GMM1 and GMM2) or only the most accurate information available (Regression GMM4), the point estimate is similar. It is of note that the biased OLS estimates exhibit relatively little bias relative to the instrumental GMM estimates, and the direction of bias is not uniform. In no case can the GMM results reject a multiplier of 1; in only Regression GMM4 can it reject a multiplier of 0. In all but one case, however, multipliers higher than 2.01 can be rejected.

In the following subsections, I have reported a variety of robustness checks.

7.2 Lagged employment growth

The effects of large-scale temporary employment may develop gradually. However, due to the rapidly mean-reverting nature of census employment, I could not estimate the 1-month-forward effect of government hiring without controlling for the contemporaneous effect.

The lagged results should be interpreted with caution: due to the temporary nature of census employment, measurement errors are not independent over time. When my data underreport the number of census workers added in a peak month, it will also underestimate the number of workers let go the following month.

I added lagged federal employment growth as an endogenous variable and lagged census employment growth as an instrumental variable to test the hypothesis that employment effects might emerge a month later. For Canada and Korea, the small and brief sample created interpretive and statistical problems; I thus reported only the following specifications in Table 11:

- Regression LGMM2: All countries, including interpolated data, including non-census months and years and time controls, includes lagged endogenous and instrumental variables
  - Contemporaneous multiplier estimate: 0.61, [−0.10, 1.32]
  - Lagged multiplier estimate: 0.47, [−0.70, 1.65]
- Regression LGMM3: US only, including interpolated data, includes lagged endogenous and instrumental variables
  - Contemporaneous multiplier estimate: 1.43, [−0.34, 3.21]
  - Lagged multiplier estimate: 0.07, [−1.21, 1.35]

Table 11 GMM with lags

Dependent variable	**dEMPLOY**		
Regression	**LGMM2**	**LGMM3**	**LGMM4**
US 1948–1972	Yes	Yes	
US 1990–2012	Yes	Yes	Yes
Canada	Yes		
Korea	Yes		
Observations	1317	212	92
dGOV	0.61 (0.36)	1.43 (0.91)	0.82 (0.22)
dGOV(*t*–1)	0.47 (0.60)	0.07 (0.65)	0.12 (0.16)
Non-census years	Yes		
Controls	Yes		

Notes: Intercept terms are country specific. Robust standard errors are given in parentheses. Controls include month dummies, time trend, and log time trend, all interacted with country. GMM, generalized method of moments.

- Regression LGMM4: US only, data after 1982, includes lagged endogenous and instrumental variables
  - Contemporaneous multiplier estimate: 0.82, [0.38, 1.25]
  - Lagged multiplier estimate: 0.12, [−0.19, 0.44]

In all three cases, the cumulative 2-month multiplier has a point estimate between 0.9 and 1.5, though very imprecisely estimated. The US data suggest that lagged effects are near zero. Unreported regressions with two lags showed that a negative multiplier was typically associated with the second lag, washing out some gains from the first lag. None of the lagged estimates is precise.

7.3 **Censuses during high unemployment**

Government spending may have larger net effects during periods of economic slack. This makes sense in the context of large-scale temporary hiring. To test whether high unemployment is associated with smaller multipliers, I identified "high unemployment" periods in both absolute and relative terms.

Four censuses took place with pre-census unemployment above 8% (US 2010; Canada 1986, 1991, 1996). Using only months in which unemployment exceeds 8%, the multiplier is 0.24 or 0.40, depending on the inclusion of time controls and non-census months. Values this low are not statistically rejected by broader specifications, but the point estimates are lower than baseline estimates, not higher as expected.

Alternately, five censuses took place with pre-census unemployment above the national average (US 1950, 2010; Canada 1986, 1991, 1996). In Korea, unemployment has been very steady and low throughout the short period I studied, and all three censuses have taken place at times of roughly average unemployment. I limited the sample to include months in which unemployment is above the national average in the US and Canada. In this specification, multipliers are 0.34 or 0.35, depending on whether time controls and non-census months are included. Again, the estimates are lower but not statistically different from the baseline specifications.

7.4 Asymmetric multipliers?

Another form of asymmetry in multipliers that could arise in the data is between upward and downward changes in federal hiring. The baseline regressions assume that the changes are symmetric, but that is not necessarily the case, especially for contemporaneous effects. Unfortunately, I found that allowing multipliers to differ between upward and downward movements in federal hiring leads to widely varying results under different specifications. In regressions that use all the available data, the multiplier on increasing federal hiring is much larger than the multiplier on decreasing federal hiring. However in the regression that uses only high-quality US data, the result is reversed: the multiplier is much larger for negative movements in federal hiring. I found that differences in either methodology or data inclusion can vastly change the results. In all cases, SEs are large.

7.5 Alternative interpolation

When I used calendar months as pay and reporting periods in my interpolation of the 1950–1970 US censuses, regressions have lower multipliers than when using the baseline series.

In first-stage regressions, I found that the alternative series closely matches changes in federal employment and GMM gives a coefficient of 1.00. The better first-stage fit leads to smaller SEs. However, due to shifts in census employment gains from 1 month to another, these regressions may be misspecified in the absence of lagged terms for federal and census employment growth.

Replicating regression LGMM3 with alternative data, I arrived at a contemporaneous multiplier estimate of 0.97 and a forward multiplier estimate of 0.45, the sum of which is almost equal to the estimates from regressions GMM3 and LGMM3. Thus, although the contemporaneous multipliers are lower than in regressions using the baseline data, the difference between the baseline and alternative estimates is accounted for by larger multipliers on lagged federal employment growth.

8 Conclusion

This study gathers data on national censuses in several countries. Censuses account for many of the largest peacetime buildups and drawdowns in federal government employment.

I constructed monthly U.S. census employment data from a variety of sources, interpolating where necessary, and did the same for Canadian census data where possible. For other Canadian censuses, as well as for censuses in Korea and Japan, I was unable to construct monthly census employment measures. My construction of US census employment for the 1950–1970 censuses is successful in the sense that it has strong predictive power on government employment changes during that time.

Among the potential uses of this data is estimating the employment multiplier on anticipated, budgeted government hiring. The data do not, however, yield precise estimates. The point estimates are most consistent with a null effect on the broader labor market or mild and temporary crowding out: the government can add net jobs by hiring people, but at a one-for-one rate or worse. The data are inconsistent, however, with strong Keynesian stimulus, since it does not appear that census hiring leads to the creation of new jobs outside of government or lasting job gains.

None of the point estimates I find are as large as the 1.57 permanent GDP multiplier used by Romer and Bernstein (2009) to justify the ARRA. Although comparing a temporary jobs

multiplier to a permanent GDP multiplier may be inappropriate, estimates based on temporary hiring might have been more appropriate for predicting the impact of the ARRA than a multiplier based on a model of permanent spending. The less precise estimates in this paper can neither reject the Romer and Bernstein (2019) multiplier nor reject a multiplier of zero.

These findings suggest that census and similar temporary hiring should not be pursued for the sake of broader employment effects. Our knowledge of the effects is highly uncertain, and expansive temporary hiring appears more likely to reduce employment in other sectors than to expand it. Temporary government hiring should be justified by the public goods that it creates, not spillovers to the labor market.

Declarations

Availability of data

See Appendix A for details of each source. The author's compilation of data from these sources is available on request and has been published on a personal website https://sites.google.com/site/salimfurth/home/research.

Competing interests

The author declares that he has no competing interests.

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Tobin, J. (1993): Price Flexibility and Output Stability: An Old Keynesian View. The Journal of Economic Perspectives 7(1), 45-65.

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Appendix A

A1 Data sources

A1.1 United States

No sources containing estimates of the temporary labor force were found for censuses before 1940.

1940. Robert Jenkins, "Procedural History of the 1940 Census of Population and Housing," Center for Demography and Ecology, University of Wisconsin - Madison, 1983, p. 25, https://www.census.gov/history/www/through_the_decades/overview/1940.html.

1950. The 1950 Censuses - How They Were Taken, *U.S. Bureau of the Census*, pp. 3, 4, and 202, http://www2.census.gov/prod2/decennial/documents/1950/proceduralHistory/1950proceduralhistory.zip

1960. 1960 Censuses of Population and Housing: Procedural History, *U.S. Bureau of the Census*, pp. 42, 46, and 360, http://www2.census.gov/prod2/decennial/documents/1960/proceduralHistory/1960proceduralhistory.zip. Detailed data came from a May 31, 1960 internal Census Bureau memorandum by Jack Silver to Robert B. Voight (Chief, Field Division), provided in personal communication by Michael Snow, U.S. Bureau of the Census.

1970. 1970 Census of Population and Housing: Procedural History, *U.S. Bureau of the Census*, pp. 1-1, 5-27, and 5-54, http://www2.census.gov/prod2/decennial/documents/03033103.zip.

1980. 1980 Census of Population and Housing: History, *U.S. Bureau of the Census*, pp. 1-14, http://www2.census.gov/prod2/decennial/documents/1980/proceduralHistory/1980proceduralhistory.zip.

1990–2012. Census 2010 temporary and intermittent workers and Federal government employment, *Bureau of Labor Statistics*, June 8, 2012. See also the regularly updated BLS web page with Census 2020 hiring: https://www.bls.gov/ces/cescensusworkers.htm.

A1.2 Canada

No sources containing estimates of the temporary labor force were found for censuses before 1871, nor for the censuses of 1966, 1976, 1981, and 1991.

1871–1921. Census of Canada web pages, "How the Census was Collected" within each year. Landing page: http://www.bac-lac.gc.ca/eng/census/Pages/census.aspx.

1931. Seventh Census of Canada, 1931, *Dominion Bureau of Statistics*, p. 52, http://publications.gc.ca/collections/collection_2017/statcan/CS98-1931-1-eng.pdf.

1941, 1951, 1971. General Review: Administrative report of the 1971 Census, *Statistics Canada*, p. 48, http://publications.gc.ca/collections/collection_2017/statcan/CS99-740-1971.pdf.

1956. Census of Canada, 1956, Volume III, *Dominion Bureau of Statistics*, pp. 10-32 and 10-39, http://publications.gc.ca/collections/collection_2017/statcan/CS98-1956-3.pdf.

1961. General Review: Administrative report of the 1961 Census, *Statistics Canada*, pp. 12-10, https://ia800604.us.archive.org/27/items/1961995371970engfra/1961995371970engfra.pdf

1986. Census Handbook, Census Canada 1986, *Statistics Canada*, pp. 71 and 75, http://publications.gc.ca/collections/collection_2013/statcan/rh-hc/CS99-104-1988-eng.pdf.

1996. 1996 Census Handbook, *Statistics Canada*, p. 22, https://www12.statcan.gc.ca/access_acces/archive.action-eng.cfm?/english/census01/info/96_Handbook-2002.pdf.

2001. Census of Canada (web page), *Statistics Canada*, http://www23.statcan.gc.ca/imdb/p2SV.pl?Function=getSurvey Id=30217.

2006. 2006 Census collection (web page), *Statistics Canada*, https://www12.statcan.gc.ca/census-recensement/2006/ref/about-apropos/coll-eng.cfm. Detailed timeline provided by Jane Lavigne, Statistics Canada, in personal communication.

2011. Archived – 2011 Census staffing 35,000 jobs across Canada (web page), *Statistics Canada*, https://www.statcan.gc.ca/eng/about/smr09/smr09_026. Detailed timeline provided by Jane Lavigne, Statistics Canada, in personal communication.

2016. Recruitment and the 2016 Census, *Statistics Canada*, https://www.brandonu.ca/careerplanning/files/2012/01/2016-Census-Information-Sheet-Jan.-15-1.pdf. Detailed timeline provided by Jane Lavigne, Statistics Canada, in personal communication.

A1.3 **Korea**

No sources containing estimates of the temporary labor force were found for censuses before 1990.

1990–2010 "Population and Housing Census Report" for each census year, *Korea National Statistical Office*, found via Hangul search at http://dl.nanet.go.kr/SearchList.do.

2015. 2015 Census Comprehensive Plan [종합시행계획], March 2015.

A1.4 **England and Wales**

No sources containing estimates of the temporary labor force were found for censuses before 1841. No census occurred in 1941.

1841. Enumeration Abstract, published by *Vision of Britain*, http://www.visionofbritain.org.uk/census/GB1841ABS_1/2.

1851. General Report, p. xi, published by *HathiTrust Digital Library*, https://babel.hathitrust.org/cgi/pt?id=chi.11273088 view=1up seq=15.

1861–1931. General Reports, published by *Vision of Britain*. Landing page: http://www.visionofbritain.org.uk/census/.

1951–2001. General Reports, provided by Isabel Trevenna (Census Customer Services, Office for National Statistics) in personal communication.

2011. General Report, p. 101 (Chapter 3), published by the *Office for National Statistics*, https://www.ons.gov.uk/census/2011census/howourcensusworks/howdidwedoin2011/2011censusgeneralreport.

2021. "Help Shape Our Future: The 2021 Census of Population and Housing in England and Wales", published by HM Government, Dec. 2018, p. 76, https://assets.publishing.service.gov.uk/government/uploads/system/uploads/attachment_data/file/765089/Census2021WhitePaper.pdf.

A1.5 **Japan**

No sources containing estimates of the temporary labor force were found for the 2015 census or for censuses before 2000.

2000–2010. Webpages entitled "Outline of the Survey" for each census, *Statistics Bureau of Japan*, landing page: https://www.stat.go.jp/english/data/kokusei/index.html.