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The Origins of Anti-Immigrant Sentiments: Evidence from the Heartland in the Age of Mass Migration

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Abstract

The Kansas Bureau of Labor and Industry surveyed attitudes towards immigration during the 1890s. The surveys reveal that individuals opposed immigration for cultural and economic reasons. Key correlates were the position in the labor market, the business cycle, and immigrant status. The magnitudes of the effects indicate that economic factors explain twice the variation in opinions across individuals than cultural factors explain. In addition, changes in economic conditions from 1880 to 1920 explain a substantial share of the rise in anti-immigrant sentiments at the end of the nineteenth and during the early twentieth centuries, but other factors, such as the rise of the eugenics movement, must have had at least as large a role.

KEYWORDS: Immigration, xenophobia, labor, politics, Kansas

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"The seeds of the immigration restriction of the 1920s were surely planted in the 1880s and 1890s"

Roger Daniels, 1997[1]

INTRODUCTION

The forty years between 1880 and 1920 were an age of mass migration. Falling transportation costs, an expanding economy, and an open-door policy encouraged twenty million men and women to cross the Atlantic. During the 1890s, opposition to immigration first became a formidable, national crusade. The movement crested during the winter of 1896-1897, when "only the stubbornness of Grover Cleveland prevented the enactment of a drastic test to stop the entry of illiterates."[2] Why opposition to immigration erupted during the 1890s has long been the subject of scholarly speculation.

Explanations fall into two classes. The first emphasizes bread and butter economic issues. Immigrants took jobs from American workers and reduced the wages of the native born. Foreigners willingly accepted wages too low to sustain American standards of living. Resentment rose during financial panics and nationwide economic downturns which were new and frightening phenomena. Depressions inflamed hostility over "unfair immigrant competition for jobs."[3] The second focuses on the changing composition of immigration and ensuing social and cultural conflicts.[4] Before 1870, most immigrants came from west of the Oder and north of the Alps. These traditional migrants were white, Anglo-Saxon, and Protestant. After 1870, ever increasing numbers came from Southern and Eastern Europe. The newcomers tended to be Catholic, Orthodox, and Jewish.

Documentary evidence supports both suppositions. Newspaper editorials, academic essays, popular novels, government reports, transcripts of congressional hearings, and similar sources demonstrate that cultural concerns influenced some individuals. Economic concerns influenced others. Which factor had more influence on the attitudes of ordinary individuals? Does evidence from individuals corroborate or contradict any of these conjectures? Data from opinion polls addresses this comparative question. The Kansas Bureau of Labor and Industry collected such statistics during the 1890s, allowing us to analyze the relationship between individuals' attitudes towards immigration and their economic, social, and demographic characteristics.

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1 Daniels 1997 pp. 45-46
2 Higham 1953 p. 77
3 Daniels 1991 p. 39
4 Fairchild 1917 and 1924, Parker 1924, Higham 1955, Daniels 1991

This essay's evidence, estimates, and conclusions appear in the three following sections. Section 1 outlines the principal theories of the origins of anti-immigrant sentiments. Section 2 describes data examined in this essay. An appendix describes the collection, collation, and construction of the data set. Section 3 describes the ordered probit statistical procedures with which we analyze the evidence and our regression results. An appendix discusses methodological issues and the robustness of the regressions. Section 4 discusses our essay's principal insights.

Both economic and cultural concerns influenced the opinions of ordinary individuals. The principal economic influences were a worker's position in the labor market and the recession of the 1890s. The principal non-economic factor was the number of generations that an individual's ancestors resided in the United States.

Economics, culture, and opinions interacted in ways unanticipated by the conventional academic wisdom. In the economic domain, individuals most opposed to immigration were workers such as mechanics and craftsmen with specialized skills and in trade unions. In the cultural domain, the ethnicity of individuals appears to have had little correlation with attitudes towards immigration. Immigrants from southern and eastern Europe did not arouse substantially more hostility than immigrants from northern and western Europe. American-born individuals of northwest European ancestry did not oppose immigration substantially more than American-born individuals of southwest European ancestry.

1: THEORIES ABOUT WHY NATIVES OPPOSED IMMIGRATION

A wide range of scholars has studied the origins of anti-immigrant sentiments, including historians, anthropologists, sociologists, political scientists, and economists. The theories that they have proposed fall into two broad classes. One focuses on economic issues. The other focuses on cultural and social concerns. Each category consists of several related conjectures.

Most conjectures in the economic-anxieties-aroused-opposition-to-immigration category begin with the observation that natives and immigrants competed in labor markets.[5] The most popular version of the conjecture presumes that immigration raised the number of low-skilled relative to the number of highly-skilled workers and thus lowered the wages of the former relative to the later. The decline in their relative wage made low-skilled, low-wage workers hostile towards immigrants. An alternate form of the conjecture observes that

[5] Daniels 1997, Higham 1955, Goldin 1993, Hatton and Williamson 1994, 1995, 1997, Timmer and Williamson 1998.

some workers, particularly highly-skilled and highly-paid individuals in the growing number of trade unions, earned economic rents due to their bargaining position in the labor market. Immigration weakened their bargaining position and threatened their level of income. This theory predicts that highly-paid workers opposed immigration more vigorously than their less fortunate counterparts and more than the owners and mangers of firms. Another economic conjecture presumes that personal economic circumstances inflamed anti-immigrant sentiments. This theory predicts that antipathy towards immigrants should be correlated with decreases in wages, days lost from work, reduced savings, and lack of wealth. A final economic conjecture presumes that general economic conditions such as depressions and the level of unemployment inflamed anti-immigrant sentiments. In this case, opposition to immigration should be greatest in regions where economic aggregates, such as employment, output, aggregate income, and the prices of land and buildings decreased.

A cohort of quantitative historians, including Goldin, Hatton, Timmer, and Williamson, uses aggregate data to argue that the first and the last of these economic conjectures, particularly relative wages and the rate of unemployment, explained most if not all of the variation across time and space in political support for policies that restricted immigration.[6] While they use different data sets, all conclude that during the age of mass migration, immigration depressed the wages of workers, the employment opportunities available to the native born, or both. These costs of open borders aroused opposition to immigration among the men and women that bore them, the working class. They base their estimates on political economy models which divide workers into two groups, the skilled and unskilled, and then find a relationship between this categorization and hostility to immigration.[7]

Most conjectures in the cultural-differences-engendered-opposition-to-immigration category begin with the observation that the culture, customs, or behavior of immigrants discomforted the native born. The clash-of-cultures version of this conjecture emphasizes the differences between peoples from northern and western Europe and southern and eastern Europe. It predicts that individuals with ancestry in northwestern Europe should oppose immigration more vehemently than individuals with ancestry in other places. Opposition to immigration should increase little in response to influxes of immigrants from northwestern Europe. Opposition to immigration should rise rapidly in response to influxes of immigrants from other places. The clash-of-generations version of this conjecture presumes that most individuals raised in the United States shared traits that most immigrants lacked. Thus, native born should oppose immigration

[6] Goldin 1993, Hatton and Williamson 1995, Williamson 1974 and 1996.
[7] Foremen-Peck 1992

more strongly than the foreign born. Native-born individuals with foreign-born parents (second generation immigrants) should oppose immigration more vigorously than individuals born abroad (first generation immigrants).

Social scientists frequently employ two additional theories when studying the formation of attitudes towards immigrants. One, marginality theory rests on the observation that "the experience of being oneself marginalized, oppressed, or outside the 'mainstream' breeds sympathy with marginalized or oppressed people in general, even if they do not belong to one's own group." [8] Marginality theory suggests that being an ethnic minority, a recent immigrant, the child of a recent immigrant, or a woman should reduce opposition to immigration. Two, contact theory suggests that interaction with immigrants in one's neighborhood or workplace can heighten or diminish opposition to immigration. As a prominent advocate of contact theory, Gordon Allport, notes "whether or not contact reduces prejudice seems to depend on the nature of the contact."[9] Productive, personal contact often decreases prejudice and opposition to immigration. Confrontational, casual contact often heightens tensions and opposition to immigration. The typical measure used for the extent of contact is the proportion of immigrants in a community such as a county. Such information appears in our database.

2: SOURCES OF DATA

The data comes from three sources. The Kansas Bureau of Labor and Industry's (KBLI) surveys of wage earners for 1895, 1896, and 1897 contain information about individuals' attitudes towards immigration.[10] The published tabulations of the United States census for 1890 and 1900 contain information about the localities in which the respondents to the KBLI surveys resided.[11] The Integrated Public Use Micro Samples (IPUMS) of the census for the years 1880 and 1900 contain information about the occupations in which the respondents worked.[12]

The Kansas Bureau of Labor and Industry took steps to ensure their surveys were accurate, random, and representative (a detailed description of their methods appears in an appendix available from the authors). Sampling statistics for the surveys appear in Table 1. Their methods generated a reasonably representative sample, as Table 2 shows. The data captured most of the variation in the national origins and much of the variation in occupations and experience of

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8 Fetzer 2000 p. 5. The predictions following come from Fetzer 2000 pp. 16-24.

9 Allport 1979 pp. 261-262

10 Carter, Ransom, and Sutch 1991; Carter, Ransom, Sutch, and Zhao 1993a, 1993b, 1993c; Kansas Bureau of Labor and Industry 1896a, 1896b; Kansas Bureau of Labor and Industrial Statistics 1898

11 United States, Office of the Census, 1890 and United States, Office of the Census, 1900.

12 Ruggles and Sobek 2003, www.ipums.org

the working population that existed in Kansas and the United States during 1890s. Coefficients derived from the data should be reasonably reliable indicators of attitudes in the nation as a whole.[13] Concerns exist, however, about clustering and stratification among the observations.

TABLE 1
KANSAS BUREAU OF LABOR AND INDUSTRY SURVEYS, SAMPLING STATISTICS

	Number of Questionnaires					Counties	
	Dispatched	Returned	Tabulated	Male	Female	Number [a]	Percent [b]
11th Annual Report, 1895	Unknown	Unknown	514	514	0	Unknown	Unknown
12th Annual Report, 1896	Unknown	Unknown	539	539	0	39	37.1
13th Annual Report, 1897	5,000	1,755	1,204	1,069	135	68	64.8
Total [c]			2,257	2,122	135	71	67.6

Notes: (a) This column indicates the number of counties for which a response to the survey was tabulated. County of respondent was not reported in the 11th Annual Report. So, the geographic distribution among counties is unnown. (b) The state of Kansas contained 106 counties in 1890 and 105 in 1900. The denominator used here is 105. (c) The total number of counties is the number of counties appearing in the pooled 1897 report plus the 3 counties in the 1896 report that do not reappear in 1897.

Sources: Kansas Bureau of Labor and Industry 1895, 1896, 1897

The Kansas surveys provide the dependent variable: preferences over immigration policies. The surveys asked each respondent: "do you favor restriction or suppression of foreign immigration?" The meaning of the question was clear from context and the tenor of the times. *Suppression* meant a complete

[13] Recent research reinforces this point. We recovered similar opinion surveys from Michigan. Initial analysis finds coefficients with magnitudes, signs, and significance levels similar to those in this essay.

cessation of working-class immigration, the erection of an impermeable barrier to

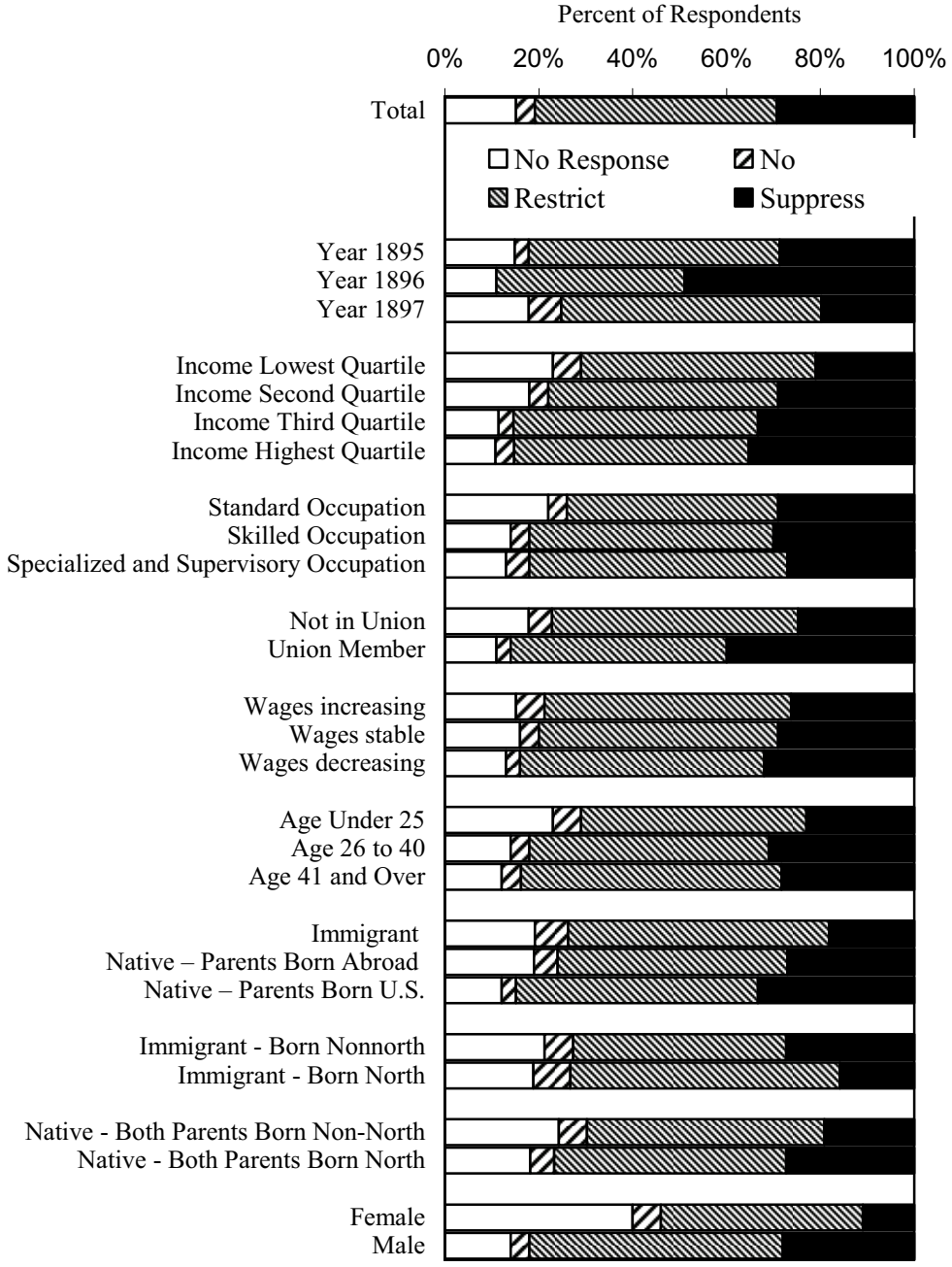
TABLE 2
SUMMARY STATISTICS FROM KANSAS BUREAU OF LABOR AND INDUSTRY SURVEYS COMPARED TO CENSUS DATA

	Kansas Surveys			Kansas			United States		
				Census		IPUMS	Census		PUMS
	1895 %	1896 %	1897 %	1890 % Pop	1900 % Pop	1900 % IE	1890 % Pop	1900 % Pop	1900 % IE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Born USA	83.5	85.2	85.3	89.6	91.4	89.6	85.2	86.3	83.7
1st Generation Immigrant	16.5	14.8	14.7	10.4	8.6	10.4	14.8	13.7	16.4
Born North	12.4	8.3	11.4	5.5	4.3	8.4	7.7	6.5	8.4
Born Non-North	4.1	6.5	3.3	4.9	4.3	2.0	7.1	7.2	8.0
2nd Generation Immigrant	23.0	25.6	26.1	26.8	27.4	18.9	33.0	34.3	20.1
Parents Born North	15.4	15.6	16.5	-	12.6	6.3	-	56.8	7.0
Parents Born Non-North	7.6	10	9.6	-	14.9	12.6	-	43.2	13.1
Occupation Standard	18.4	18.8	24.4	-	-	34.2	-	-	38.2
Occupation Skilled	77.4	75.3	65.7	-	-	40.1	-	-	37.1
Occupation Specialized and Supervisory	4.2	5.94	9.9	-	-	25.7	-	-	24.7
Under Age 25	18.4	19.5	25.8	59.8	-	31.8	57.8	-	28.7
Age 26 to 40	54.9	59.4	52.8	20.4	-	34.3	21.6	-	36.5
Age 41 and Over	26.8	21.2	21.3	19.7	-	33.9	20.4	-	34.9
Male	100	100	88.8	52.7	52.3	78.6	51.2	51.2	73.3
Female	0	0	11.2	47.3	47.7	21.4	48.8	48.8	26.6

Notes: A dash indicates information unavailable. Columns and rows may not sum to 100% due to rounding. Columns headed % Pop indicate percentage of the population. Columns headed % IE indicate percentage of industrial employment.

Sources: Integrated Public Use Sample of the United States Census; Kansas Bureau of Labor and Industry; United States Census, Report on Population of United States, 11th Census, 1890, Volume 1, Part 1 and Report on Population of United States, 12th Census, 1900, Volume 1, Part 1.

Figure 1
Do you favor restriction or suppression of foreign immigration?



the entry of laborers and strict restrictions on the entry of other classes in a manner reminiscent of the Chinese exclusion acts. *Restriction* meant reducing inflows of immigrants, probably by screening out those believed to be of lower quality, such as the ill, infirm, illiterate, impoverished, aged, uneducated, felonious, and anyone else for any reason thought undesirable. *No* alternative was specifically stated, although some individuals wrote in the answer "no," indicating that they favored neither policy. Others left the space blank.[14]

Figure 1 graphically cross-tabulates these replies. The cross-tabulations illuminate several significant patterns. Approximately one-quarter of the population desired the cessation of immigration. The preponderance of the population desired the regulation of immigration. This was true for individuals of all national and occupational origins. Opposition was strongest among the native born, particularly those whose parents were also born in the United States or whose parents came from northern Europe. Opposition was greatest among skilled workers with 10 to 25 years of work experience who were members of unions. Less skilled and experienced workers opposed immigration at lower, but still substantial, rates. Opposition to immigration peaked at the trough of the recession during 1896.

The Kansas surveys provide this study's individual level explanatory variables. These include occupation, income, wealth, employment status, age, gender, birthplace, and parents' birthplace (data appendices which are available from the authors describe how we standardized this information). Birthplaces reveal important information about individual's attitudes, culture, and linguistic skills. Immigrants came from two types of places. Those from northwestern Europe tended to be white, Protestant, and either Anglo-Saxon or Nordic. Their religion, customs, and complexion resembled those of the native born. We designate individuals from these regions with the label *North* (see appendix A for details). Immigrants from southern and eastern Europe were Slavs, Balts, Italians, and Iberians. They tended to be poor, illiterate, and unfamiliar with the English language. Few came from countries with democratic institutions. Most came from rural areas ruled by decaying empires and decadent aristocrats. Some were Orthodox or Jewish. Many, including the Irish, were Catholic. Religion, complexion, and culture distinguished them from the native born. We designate

[14] *No* and *non-response* have similar logical implications. Both imply lack of support for restriction and suppression, and since the form did not instruct individuals to specify negative responses, many individuals who disapproved of restriction and suppression probably left the answer blank. Non-response, of course, has an additional implication. The respondent may have declined, for some unstated reason, to answer the question. Our analysis considers both possibilities, and finds that the interpretation of non-response does not influence our conclusions.

Table 3
Descriptions and Summary Statistics of Principal Explanatory Variables

	Level	Units	Mean	SD	# Obs	Corr	Sig	Years Available			Source	Notes
								1895	1896	1897		
	(1)	(2)	(3)	(4)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Earnings and Wealth Variables												
Income (Annual)	Individual	100s of $	5.27	2.44	2003	0.16	0.00	x	x	x	KBLI	a
Wage changed (Decrease, stable, or increase since last year)	Individual	-1, 0, or 1	-0.02	0.52	2253	-0.04	0.04	x	x	x	KBLI	a
Investments (Cash value of financial assets and real estate)	Individual	100s of $	0.27	0.84	2253	0.11	0.00	x	x	x	KBLI	a
Occupational Variables												
Skill level												
Standard occupations	Individual	0 or 1	0.20	0.40	2253	0.05	0.02	x	x	x	KBLI	a
Skilled occupations	Individual	0 or 1	0.72	0.45	2253	0.04	0.04	x	x	x	KBLI	a
Specialized or supervisory occupations	Individual	0 or 1	0.08	0.27	2253	0.00	0.94	x	x	x	KBLI	a
Union member	Individual	0 or 1	0.36	0.48	2253	0.15	0.00	x	x	x	KBLI	
Hours worked per day (annual average)	Individual	Hours	10.28	1.73	2253	0.04	0.05	x	x	x	KBLI	i
Years in Occupation	Individual	Years	7.84	8.15	2253	0.05	0.02	x	x	x	KBLI	i
Percent of workers in occupation who were immigrants, 1900	Occupation	0 to 1	0.11	0.03	2253	0.06	0.00	x	x	x	IPUMS	a
Percent of workers in industry who were immigrants, 1900	Industry	0 to 1	0.11	0.04	2253	-0.01	0.59	x	x	x	IPUMS	a
Nativity Variables												
Born in United States, parents born in U.S.	Individual	0 or 1	0.53	0.50	2253	0.13	0.00	x	x	x	KBLI	a
Born in United States, parents born abroad	Individual	0 or 1	0.28	0.45	2253	-0.04	0.04	x	x	x	KBLI	a
Born in United States, both parents born north	Individual	0 or 1	0.14	0.35	2253	-0.03	0.18	x	x	x	KBLI	a
Born in United States, both parents born nonnorth	Individual	0 or 1	0.06	0.24	2253	-0.08	0.00	x	x	x	KBLI	a
Born abroad	Individual	0 or 1	0.18	0.39	2253	-0.12	0.00	x	x	x	KBLI	a
Born abroad north	Individual	0 or 1	0.14	0.35	2253	-0.12	0.00	x	x	x	KBLI	a
Born abroad non-north	Individual	0 or 1	0.04	0.21	2253	-0.03	0.20	x	x	x	KBLI	a
Immigration Variables												
Percent of county population born north, 1890	County	0 to 1	0.08	0.04	1739	-0.09	0.00		x	x	Census	
Change in % of county population born north, 1890 to 1900	County	0 to 1	-0.13	0.15	1739	-0.01	0.84		x	x	Census	
Percent of county population born nonnorth, 1890	County	0 to 1	0.02	0.03	1739	-0.05	0.06		x	x	Census	
Change in % of county pop. born nonnorth, 1890 to 1900	County	0 to 1	-0.11	0.30	1739	0.00	0.87		x	x	Census	
Change in % of males age 18 to 44 born abroad, 1890 to 1900	County	0 to 1	-0.04	0.05	1739	0.08	0.00		x	x	Census	

Table 3 (cont.)

Notes: Column (1) indicates the level at which variable measured. County level variables assigned to each individual residing in county. Occupation and industry level assigned to each individual working in a particular occupation or industry. Columns (3) through (5) report summary statistics for each variable including the standard deviation, denoted SD, and the # of observations reporting such information, denoted # Obs. Columns (7) and (8) report the coefficient of correlation with the variable indicating attitudes towards immigration, denoted Corr, and the significance level of that correlation, denoted Sig. Significance levels below 0.05 suggest the correlation exists for reasons other than measurement error or random chance. In Col (14), "a" indicates derivation described in appendix. "i" indicates missing observations imputed using all other available information.

Sources: See Table 2.

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individuals from these regions with the label *Nonnorth*.[15] *Standardized annual income* (later abbreviated as *income*) indicates annual remuneration in the form of wages and salary and reflects the extent to which workers competed in labor markets against immigrants. Immigrants tended to work jobs requiring few skills and paying low wages. Workers in high-paying vocations and managerial positions faced less labor-market competition.

The published tabulations of the United States census for the years 1890 and 1900 illuminate the characteristics of the counties in which respondents lived. Censuses for both decades indicate the percentage of the population born abroad, with ancestry from northern Europe, and with ancestry elsewhere, and the total value of agricultural and manufacturing output. The census for 1890 indicates the percentages of Protestants, Catholics, and other faiths in the population of each county.

The IPUMS for the years 1880 and 1900 contain information about hundreds of individuals from the state of Kansas. This information allows us to calculate the percentage of immigrants employed in each occupation and industry.

In sum, the Kansas surveys were collected judiciously. The data are amendable to modern statistical techniques. Combining data from the Kansas

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[15] The north / non-north bifurcation employed in this essay is robust to plausible alternatives. All regressions were run (i) using disaggregated data on birthplaces and (ii) grouping birthplaces into national areas (described in the appendix). In each case, the results resembled those in this essay.

surveys with data from other sources allows us to test the principal hypotheses concerning the origins of anti-immigrant sentiments. Table 3 summarizes the principal explanatory variables employed in these tests.

3: METHODOLOGY AND RESULTS

This section confronts the data with a series of increasingly stringent hypothesis tests. The tests yield coefficients and standard errors that allow us to calculate the relative importance of competing theories concerning the origins of anti-immigrant sentiments. A series of alternative specifications confirms the robustness of our results.

Since the dependent variable is qualitative and ranked, ordered probit regressions are the appropriate statistical methods. Three formulas comprise this essay's econometric model.

$$\Pr(Y_i = \text{No or Non response}) = \Phi(\varepsilon_i \leq -\beta' X_i)$$
$$\Pr(Y_i = \text{Restrict}) = \Phi(\varepsilon_i \leq \mu_1 - \beta' X_i) - \Phi(\varepsilon_i \leq -\beta' X_i)$$
$$\Pr(Y_i = \text{Suppress}) = 1 - \Phi(\varepsilon_i \leq \mu_1 - \beta' X_i)$$

where X is a matrix of explanatory variables. β is a vector of coefficients for those variables. i indicates the i^{th} individual. Φ is the cumulative normal distribution.[16] We estimate this ordered probit regression using the method of maximum likelihood via the Newton-Raphson algorithm. Our estimates contain controls for the economic and demographic characteristics of counties including the number of manufacturing firms in 1890, the average wage of workers in manufacturing, the number of farms in 1890, the total population in 1890, the colored population in 1890, the number of Catholics, Jews, and Mormons in 1890, and changes between 1890 and 1900 in all of those values except for the last, since religious information does not appear in the census of 1900. We report the coefficients on these controls only when they influence the interpretation of regression results. We do not report coefficients on constant terms, which are always positive and statistically significant.

Table 4 tests the economic hypotheses. Column (1), which pools data from

[16] We tested the robustness of this specification in four ways. We estimated it as a multinomial logit model with four choices {(non response), (no), (restrict), (suppress)}, as an ordered probit with three categories {(no), (restrict), (suppress)} where non responses were excluded from the regression, as a probit with (no, non response, restrict) grouped as one category and (suppress) as the other, and as a probit with (no, non response) grouped as one category and (restrict, suppress) as the other. In each case, the signs and significance levels resembled those presented in this essay.

Table 4
Basic Tests of Economic Hypotheses

Dependent Variable, Attitudes Towards Immigration 0 = neither restrict nor suppress, 1 = restrict, 2 = suppress	Coefficient Standard Error	
	(1)	(2)
Year 1896	**0.4781** 0.0738	
Year 1897	**-0.1416** 0.0689	**-0.6210** 0.0730
Income	**0.1388** 0.0377	**0.1452** 0.0468
Income squared	**-0.0084** 0.0027	**-0.0089** 0.0033
Unskilled occupation	0.0014 0.0670	-0.0262 0.0790
Specialized or supervisory occupation	0.0829 0.1035	-0.0185 0.1143
Wage changed since last year	-0.0334 0.0502	-0.0608 0.0568
Hours worked per day	0.0249 0.0154	0.0289 0.0180
Years in occupation	0.0013 0.0033	0.0010 0.0044
Male	**0.3783** 0.1167	**0.3636** 0.1209
Investments	**0.8360** 0.0341	**0.0941** 0.0414
Union member	**0.2060** 0.0615	**0.2910** 0.0812
% immigrants in occupation	-0.0407 1.0578	0.2696 1.2578
% immigrant in industry	-0.3914 0.7346	-0.5300 0.8999
Change in % foreign born among working males		**2.5034** 0.7761
Other county level variables (see text)		Yes
Observations	1999	1496

Bold faced type indicates significance at 5% level.

all three years, lacks county-level coefficients, because observations for 1895 lack information about the counties in which respondents resided. The coefficients on the chronological variables indicate that opposition to immigration peaked during 1896 (note that positive coefficients imply the variable in question aroused opposition to immigration), which was the trough of the recession in Kansas. Opposition to immigration fell during 1897, when bountiful harvests spurred Kansas's recovery from the depression of the 1890s, which was known as The Great Depression, until the depression of the 1930s proved to be even greater. The coefficient on the variable for *union membership* indicates that opposition to immigration was stronger among union than non-union workers. Union membership came primarily from the ranks of skilled manufacturing workers and the transportation industry, especially railroads. The coefficients on *skill* variables indicate that skilled laborers (the excluded category) on average opposed immigration more vociferously than workers in either tail of the skill distribution. The coefficients on skill-level variables are not statistically significant in the regressions presented in this paper, apparently because they are highly correlated with other explanatory variables including union membership, years in occupation, and annual income.

The coefficients on *income* (measured in units of $100 dollars per year) and *income squared* indicate that opposition to immigration increased as income increased until reaching a peak (in this case, at $826 dollars per year, or about 1 standard deviation above average income) after which opposition to immigration declined. This income peak lies within the range typically earned by union workers. Thus, the coefficients on income lend further support to the supposition that skilled, unionized workers opposed immigration more fervently than other individuals.

This interpretation is consistent with the typical interpretation in regressions of this type, which is based on the assumption that income is a proxy for the extent to which individuals compete against immigrants in the labor market. Money itself has an ambiguous effect. Individuals whose wages rose during the year in which they filled out the questionnaire viewed immigration more favorably. Individuals whose wages fell viewed immigration less sympathetically, although the coefficient on the variable *wage changed* is statistically significantly in only a few specifications of our regressions, probably because changes in wages were correlated with the business cycle, and the annual dummy variables capture much of the business cycle's impact. Individuals with savings in banks or investments in stocks, bonds, homes, or property, whose cash values appear in our variable named *investments*, viewed immigration less favorably than individuals without savings or investments. The larger an individual's investments, the less favorably they viewed immigration. Thus, it seems safe to accept the conventional interpretation of the coefficient on income –

that it reflects the extent to which individuals compete against immigrants in the labor market. Our data suggest, in fact, that this was the case. The percent of immigrants among the workers in an occupation is highly correlated with attitudes towards immigrants, and occupation is correlated with income. That multicollinearity is the reason that variable indicating the percentage of immigrants in an occupation is not significant in our regressions, despite its strong pairwise correlation with attitudes towards immigration (see its *Corr* and *Sig* in Table 3).

Column (2) pools data from the two years for which county-level information is available and adds county-level variables to the regression. The county-level coefficients are statistically insignificant with one exception: the change in the percentage of males of working age (18 to 44) who were born abroad. This coefficient suggests that the more the foreign-born share of the labor force in a county rose (or fell) during the 1890s the more (or less) the residents of the county opposed immigration. For the variables retained from the first column, the signs, standard errors, and magnitudes of the coefficients remain relatively unchanged. This similarity increases confidence in conclusions drawn from the first column.

Overall, the results in Table 4 corroborate some, but not all, of the principal economic theories, while rejecting others. The skill-composition-bias version of the labor-market hypothesis – that low-paid, low-skilled workers opposed immigration more than their better-paid, high-skilled counterparts – is not consistent with the evidence. No declining relationship existed between skill level, occupation, income, or other measure of one's position in the job market. A relationship existed, but in all cases, it had an inverted "u" shape. It rose initially, peaked somewhere near the range of skilled laborers, and fell thereafter. In addition, unionized workers opposed immigration more than other individuals. So did high-skilled and high-salaried laborers whose occupations and incomes resembled those of unionized workers but who did not belong to unions. These patterns are consistent with the battle-over-economic-rents version of the labor-market hypothesis.

The next economic theory – that personal economic circumstances influenced attitudes towards immigration – is consistent with the evidence. The hypotheses that individual's income or investments influenced their attitude towards immigration cannot be rejected at standard significance levels. The hypotheses that individuals' experience, occupation, or changes in their wage levels influenced their attitudes towards immigration appears at first glance to be rejected, but that rejection would be mistaken because multicollinearity exists among these variables. Each is correlated with attitudes towards immigration, and each is correlated with the others. By changing the specification of the regression, each can be shown to pass a "significance test." A Wald test on these variables

indicates that the group as a whole significantly influences the dependent variable.

The result for the final economic hypothesis – that the depression of the 1890s, which began in 1893 and continued through 1898, aroused opposition to immigration – is clear. Attitudes varied from year to year in the manner predicted by the theory. At the trough of the recession in Kansas, 16% more respondents advocated the suppression of immigration than during the preceding year. Such an effort compounded over the years of the depression could potentially explain the initial rise and subsequent decline of anti-immigrant sentiments during the decade and its symptoms such as Congressional bills banning immigration and the expansion of nativist organizations such as the American Protective Association.

Table 5 tests the cultural hypothesis. Column (1) presents the simplest possible specification. Dummy variables differentiate individuals born abroad from those born in the United States of foreign-and U.S.-born parents. Statistical tests reject the null hypothesis that the coefficients on these variables equal zero. Also rejected is the null hypothesis that the coefficients equal each other. The implications are clear. Nativity influenced attitudes towards immigration. Native-born individuals of U.S. parentage opposed immigration more than native-born individuals whose parents were born abroad who in turn opposed immigration more than immigrants did.

Column (2) reinforces that result and adds additional information by first dividing immigrants into groups who were born in northwestern Europe (denoted north) and those born in Ireland, southern and eastern Europe, and everywhere else (denoted nonnorth) and second by dividing the children of immigrants into groups depending on the location of their parents birth. Statistical tests at the 5 % level reject the null hypotheses that the coefficients of the nativity variables equal zero. Statistical tests at the 10% level reject the null hypothesis that the coefficients for native-born individuals of northern and nonnorthern heritage are equal (χ^2 statistic with 1 degree of freedom equals 6.97). But, statistical tests cannot reject the conjecture that the coefficients for individuals born in the areas labeled north and nonnorth are equal (χ^2 statistic equals 0.17).

The implications of these tests are clear. Opinions about immigration differed significantly between the native- and foreign-born. Opinions also varied among the children of immigrants. The immediate descendants of immigrants from northern Europe viewed immigration more favorably than the immediate descendants of immigrants from southern Europe. Opinions about immigration, however, varied little among immigrants themselves. The attitudes of immigrants born in northern Europe resembled those from southern Europe. The attitudes of immigrants from England resembled those from Eastern Europe. The attitudes of immigrants from Germany resembled those from Ireland. All of these observations remain true even if one examines the data by nation (rather than region) of origin.

Table 5
Basic Tests of Cultural Hypotheses

Dependent Variable, Attitudes Towards Immigration 0 = neither restrict nor suppress, 1 = restrict, 2 = suppress			Coefficient Standard Error
	(1)	(2)	(3)
Year 1896		**0.4862** 0.0714	
Year 1897		**-0.2879** 0.0596	**-0.7365** 0.0646
Born US, parent born abroad	**-0.2148** 0.0548		
Born US, both parents born north		**-0.2522** 0.0709	**-0.3311** 0.1106
Born US, both parents born non-north		**-0.5562** 0.1022	**-0.6430** 0.1425
Born abroad	**-0.4224** 0.0640		
Born abroad north		**-0.4835** 0.0720	**-0.4382** 0.1051
Born abroad non-north		**-0.4300** 0.1190	**-0.3842** 0.1834
% population born north in 1890 (county)			**-4.3870** 0.8671
% change in pop born north 1890 - 1900 (county)			-0.5307 0.3531
% population born non-north in 1890 (county)			0.1981 1.2183
% change in pop born non-north 1890 - 1900 (county)			0.0278 0.1561
Immigration/Nativity interactions			Yes
Other county level variables (see text)			Yes
Mixed heritage variables		Yes	Yes
Observations	2253	2253	1739

Bold faced type indicates significance at 5% level. County controls listed in text.

Column (3) adds information about the density of the foreign-born population within each county. This information measures the extent of the contact that the respondents to the Kansas surveys had with the immigrants living in the communities around them. The new variables include the percentage of the population living in each county in the year 1890 that was born in a northern nation, the change in the northern-born percentage of the population between 1890 and 1900, and comparable figures for nonorthern nations. Additional variables measure interactions among these figures for stocks and flows of immigrants and respondents' nativity.

The coefficients on these variables convey two messages. First, the ethnicity of immigrants had little or no influence on attitudes towards immigration. The null hypothesis that all of the coefficients on the immigration stock and flow variables jointly equaled zero cannot be rejected. None of the flow variables and only one of the stock variables were statistically significant. Second, that single significant variable indicates that individuals who lived in counties where a higher percentage of the population came from northern nations viewed immigration more favorably than individuals who lived in counties where a lower percentage of the population came from northern nations. In other words, either contact with northern immigrants engendered tolerance towards immigration, or migrants from northern nations moved to counties where residents tolerated immigrants.

Table 6 estimates our ordered probit model with all of the variables from the preceding regressions entered at the same time and after correcting for various potential econometric problems. We correct for clustering of observations using Rogers-Williams method of applying to clustered samples the sandwich-style estimators of robust standard errors devised by Hubert and White. We correct for excluded explanatory variables, such as aspects of individuals' neighborhoods, occupations, or industries that our data do not reflect, with the standard method. We add dummy variables for each county, industry, and occupation and interact those dummies with the original explanatory variables. We ensure that the procedures with which we cleaned, corrected, and standardized the data do not influence our results via a similar method. We include in the regression dummy variables indicating which procedures we applied to each observation and interact those dummies with the original explanatory variables. In almost all cases, the estimated signs and significance levels in Table 6 resemble the originals, and in most cases, the estimated coefficients and standard errors in Table 6 are statistically indistinguishable from those in Tables 4 and 5 (based on two-sample hypothesis tests).

Table 7 uses the nativity-immigration interaction terms from Columns (3) and (4) of Table 6 to conduct a strict test of the clash-of-cultures conjecture which maintains that attitudes towards immigration differed depending on the heritage of

Table 6
Joint Hypothesis Tests, Parameter Estimation, and Robustness

Dependent Variable, Attitudes Towards Immigration 0 = neither restrict nor suppress, 1 = restrict, 2 = suppress				Coefficient Standard Error
	(1)	(2)	(3)	(4)
Year 1896	**0.5082** 0.0742	**0.4869** 0.0767		
Year 1897	**-0.1341** 0.0671	0.2972 0.3368	**-0.6194** 0.0743	0.5653 0.4614
Income	**0.1338** 0.0380	**0.1214** 0.0338	**0.1464** 0.0475	**0.2910** 0.0806
Income squared	**-0.0081** 0.0027	**-0.0081** 0.0043	**-0.0090** 0.0034	**-0.0112** 0.0039
Wage changed	-0.0406 0.0504	-0.0426 0.0757	-0.0756 0.0575	-0.0827 0.0600
Union Member	**0.2470** 0.0621	**0.2449** 0.0672	**0.3171** 0.0825	**0.2779** 0.0828
Investments	**0.0713** 0.0341	**0.0637** 0.0303	**0.0781** 0.0419	**0.0642** 0.0346
Male	**0.3870** 0.1170	**0.4578** 0.1614	**0.3734** 0.1230	**0.4422** 0.1320
Born US, both parents born north	**-0.2764** 0.0754	**-0.2677** 0.0748	**-0.4139** 0.1228	**-0.4057** 0.0991
Born US, both parents born non-north	**-0.5284** 0.1116	**-0.5250** 0.1681	**-0.6554** 0.1551	**-0.6656** 0.1280
Born abroad north	**-0.5158** 0.0791	**-0.5181** 0.0510	**-0.4944** 0.1201	**-0.5047** 0.0998
Born abroad non-north	**-0.3684** 0.1255	**-0.3661** 0.1234	-0.3789 0.2037	-0.4105 0.2528
% population born north in 1890 (county)			**-0.3654** 1.6660	**-4.0431** 1.6642
% change in pop born north 1890 - 1900 (county)			0.1679 0.4459	0.2430 0.4510
% population born non-north in 1890 (county)			-0.0874 1.6100	-0.5429 1.6425
% change in pop born non-north 1890 - 1900 (county)			0.0026 0.2149	0.0266 0.2214
Immigration/Nativity interactions			Yes	Yes
Other county level variables (see text)			Yes	Yes
Mixed heritage variables		Yes	Yes	Yes
Robust standard errors and corrections for clustering		Yes		Yes
Method of payment dummies and interactions		Yes		Yes
Observations	1999	1999	1496	1496

Bold faced type indicates significance at 5% level.

Table 7
Stricter Tests of Cultural Hypothesis

Interaction Terms From Estimates in Table 6, Columns (3) and (4)		Coefficient Standard Error	
		(3)	(4)
Nativity of Respondent	Immigrants From		
Born US, Parents Born North	North	-0.7807 0.7380	-0.9409 0.6966
	Nonnorth	-0.0216 0.3997	0.0792 0.3959
Born US, Parents Born Nonnorth	North	-1.6592 1.0353	-1.6857 0.9628
	Nonnorth	0.7579 0.6257	0.7366 0.5426
Born North	North	-0.1573 0.7686	-0.1942 0.7249
	Nonnorth	-0.2143 0.4532	-0.2285 0.4864
Born Nonnorth	North	-1.7382 1.2310	-2.0086 1.5220
	Nonnorth	0.4045 0.4917	0.4300 0.5023
Wald Test Statistic (8 degress of freedom)		5.78	7.54

Bold faced type indicates significance at 5% level.

the opinion holder and homeland of the immigrants. The interaction terms indicate whether reactions to immigrant inflows differed depending upon those factors. Overall, Table 7 reveals no significant differences among the reactions of the various groups. We cannot reject the null hypothesis that each of the interaction coefficients equal zero. The Wald statistic for the joint hypothesis falls well below the critical threshold. The data, in other words, do not exhibit the kinds of correlations suggested by the clash-of-cultures conjecture.

The results in Tables 4 through 7 reveal which of the theories concerning the origins of anti-immigrant sentiments are consistent with the data. Some clearly are. Others clearly are not. Tables 8 and 9 perform the next task. They reveal the practical significance of the coefficients from our ordered probit estimates. The practical significance depends upon how much the explained variable in our model changes in response to changes the explanatory variables.[17] These marginal effects allow us to compare the explanatory power of various theories concerning the origins of anti-immigrant sentiments.

Economic factors clearly had substantial influence on attitudes towards immigration. The likelihood that a unionized worker preferred the suppression of immigration was roughly 8% higher than that of a non-union worker. A one standard deviation increase in investments (approximately $840) raised the likelihood of suppression by approximately 1.6%. A one standard deviation (approximately $500) increase in income increased that likelihood by an amount ranging from 3% (Table 8) to 13% (Table 9), although that likelihood peaks at an amount ranging from one (Table 8) to two (Table 9) standard deviations above the average income of roughly $527 per respondent and falls thereafter.

Nativity also influenced attitudes towards immigration. First and second generation immigrants were much more likely to prefer unrestricted immigration and much less likely to prefer restricting or suppressing immigration than were native-born individuals whose families resided in the United States for three or more generations. This pattern is consistent with the clash of generations version of the cultural conjecture. It is also consistent with marginality theory, which

[17] We report these magnitudes in the typical manner. For dummy variables, we report changes in average predicted probabilities for the three potential outcomes (suppress, restrict, and neither restrict nor suppress) as the value of the dummy variable changes from 0 to 1 for all observations, all else held constant. For numerical variables, we report changes in the average predicted probabilities as the value of the variable changes from one-half standard deviation below the value for each observation to one-half standard deviation above, all else held constant. For income, we also report changes from the baseline for a range of changes in the value of income. This spectrum illuminates the u-shaped influence of income on attitudes towards immigration. Thus, the third row of Table 8 indicates that a one standard deviation shift in income increases by 0.5% the likelihood of an preferring the restriction of immigration, increases by 3.1% the likelihood of an preferring the suppression of immigration, and reduces by 3.6% the likelihood of an preferring neither of the aforementioned alternatives.

Table 8
Magnitudes of Factors Influencing Attitudes Towards Immigration
1895 to 1897, Estimates Without County-Level Variables

Variable	Change	Change in Likelihood of Outcome (percent)		
		Neither	Restrict	Suppress
Year 1896	0 to 1	-11.6	-4.4	16.1
Year 1897	0 to 1	5.6	0.9	-6.5
Income				
minus $250	- 1.0 std. dev.	5.2	-1.0	-4.2
plus $250	+ 1.0 std. dev.	-2.1	0.3	1.7
plus $500	+ 2.0 std. dev.	-1.3	0.8	0.5
Wage Changed	-1 to 1	2.3	0.3	-2.6
Investment	1 std. dev.	-1.4	-0.2	1.6
Union Membership	0 to 1	-6.3	-1.3	7.6
Gender	0 to 1	-13.8	1.7	12.2
Born US - Parents Born North	0 to 1	7.5	0.1	-7.6
Born US - Parents Born Nonnorth	0 to 1	16.0	-2.3	-13.7
Born North	0 to 1	15.4	-1.5	-13.9
Born Nonnorth	0 to 1	10.8	-0.8	-10.0

Change in average of predicted probabilities from Table 6, Column (2). Indicator variables change from 0 to 1. Continuous variables change by 1 standard deviation centered at the mean.

Table 9
Magnitudes of Factors Influencing Attitudes Towards Immigration
1896 to 1897, Estimates With County-Level Variables

Variable	Change	Change in Likelihood of Outcome (percent)		
		Neither	Restrict	Suppress
Year 1897	0 to 1	-15.3	1.0	14.3
Income				
minus $250	- 1.0 std. dev.	16.9	-4.0	-12.9
plus $250	+ 1.0 std. dev.	-9.2	-3.9	13.1
plus $500	+ 2.0 std. dev.	-13.2	-9.4	22.6
Wage Changed	-1 to 1	4.2	0.7	-4.9
Investment	1 std. dev.	-1.4	-0.2	1.6
Union Membership	0 to 1	-6.9	-1.7	8.5
Gender	0 to 1	-12.8	1.1	11.8
Born US - Parents Born North	0 to 1	11.4	-0.3	-11.1
Born US - Parents Born Nonnorth	0 to 1	20.1	-3.6	-16.5
Born North	0 to 1	14.5	-1.1	-13.4
Born Nonnorth	0 to 1	11.8	-0.9	-10.9
% of population born north	1 std. dev.	4.0	0.6	-4.6
% change in % population born north	1 std. dev.	-0.9	-0.1	1.1
% of population born nonnorth	1 std. dev.	0.4	0.1	-0.4
% change in % population born nonnorth	1 std. dev.	-0.2	0.0	0.2

Change in average of predicted probabilities from Table 6, Column (4). Indicator variables change from 0 to 1. Continuous variables change by 1 standard deviation centered at the mean.

suggests that members of marginalized groups, such as minorities, immigrants, and women, sympathize with members of their own or other marginalized groups. Nativity and gender are, in fact, two of the most significant correlates with attitudes towards immigration.

Stocks and flows of immigrants had less influence on attitudes. One standard deviation changes in stocks or flows of immigration altered average predicted probabilities by less than one percent in most cases. Variables capturing interactions between nativity and immigration had similarly small effects. The only exception was the percent of the population that was born in northern nations. A one standard deviation increase in that percentage raised the likelihood of preferring no restriction by 4% and decreased the likelihood of suppression by 4.6%. These figures indicate either (a) that residents of counties receiving larger inflows of immigrants from northern nations learned to tolerate immigration, (b) that immigrants from northern nations moved to counties whose residents tolerated immigration, or (c) both of the above.

These findings do not mean that ethnic antagonisms did not dominate the mindsets of many individuals. Our statistical methods only reveal how the explanatory variables affected attitudes on average. It is possible that some individuals disliked the immigration of Italians into their communities, perhaps because they feared foreigners or despised Catholicism, but that others approved of Italian's arrival, perhaps because they enjoyed eating ethnic cuisine or coveted Romance customs. Antipathy and acceptance may even have been extreme among both types of individuals. But on average, the two extremes roughly balanced each other out so that no correlation concerning Italians (or other ethnicities) appears in our data.

The predicted probabilities concisely summarize our regression results. Immigration stocks and flows explain little of the variation across individuals in attitudes towards immigration. Gender, nativity, and the year (which serves as an indicator of the business cycle) have substantial impacts on individuals' attitudes towards immigration. Each could alter an individual's preferences for the extremes of the policy spectrum by 10% to 15%. Economic variables measured at the individual level have smaller individual impacts, although coming to a concrete conclusion concerning the impact of annual income (our measure of the extent to which a respondent competed against immigrants in the labor market) is difficult, since its magnitude doubled when we restricted the regression to the last two years of the sample and added county-level socio-economic variables. In either case, the total influence of the economic variables equals or exceeds that of the nativity and immigration variables. A union member earning twice the median income whose wages fell during a recession year might be 25% to 30% more likely to prefer suppressing immigration than a similar individual with a steady job paying an average wage during stable times. In addition, economic

characteristics varied across the population much more than the nativity and immigration variables. Thus, the economic variables as a group explain approximately twice as much of the variance in individuals' attitudes towards immigration than the nativity and immigration.

4: DISCUSSION

This essay employs survey data on attitudes towards immigration to test theories about the origins of anti-immigrant sentiments in the late nineteenth century. Ordered probit regressions corroborate some conjectures, but find little or no support for others. The regression results also reveal patterns that up till now have not been noted by historians and social scientists.

In the economic domain, individuals most opposed to immigration were workers such as mechanics and craftsmen with specialized skills and in trade unions, rather than the low-skilled workers and individuals on the bottom of the pay scale who, according to the conventional academic wisdom, competed in labor markets most directly with immigrants, and therefore, opposed immigration most vigorously. Several potential explanations for this pattern exist. First, the conventional competitive-labor-markets explanation may be correct. Immigration may have changed the skill composition of the labor force, and through competition, altered relative wages across skill-classes of workers. But, for this theory to remain valid, the conventional wisdom concerning the patterns of immigration in the late nineteenth century must be wrong. Rather than disproportionately receiving the poor, the unskilled, and the huddled masses, the United States must have received immigrants with the talents and training needed to compete in the segments of the labor market undergoing unionization. Second, a model with imperfectly competitive labor markets may be more appropriate. Workers with specialized skills and belonging to trade unions possessed privileged positions in the labor market. Their collective bargaining power allowed them to earn wage premia. Immigrants, who often served as scabs and strikebreakers, threatened these above-market returns. Thus, opposition to immigration may have been stemmed from battles over the distribution of labor-market rents.

This observation suggests that changes in economic conditions at the end of the 19th century – particularly the rise of a skilled and unionized labor force earning premium wages and the onset of depression in 1893 – contributed significantly to the surge in anti-immigrant sentiments during the 1890s. Changes in immigration patterns contributed little if at all. Immigration from southern and eastern Europe diminished when the economy slowed, while opposition to immigration rose rapidly. Congressional records support this supposition. The strongest arguments made by members of the House and Senate during debates

over immigration restriction focused on the effects of immigration on the wages working men and women (or on the benefits of immigration to the owners of capital).

Thirty years later, when Congress passed the National Origins Quota Act, the debate had a different tone. The ethnic origins of immigrants took center stage. Senators and representatives extolled the superiority of Anglo-Saxon and Scandinavian peoples while denigrating men and women from other places. Can the attitudes observed in this essay or changes in the economy and demography of the United States between the 1890s and 1920s explain the resurgence of the anti-immigrant movement during the 1920s and the changing nature of the debate?

Some of the resurgence but far from all of it is the answer. During the first two decades of the twentieth century, unemployment increased as more individuals moved off the farm and into the urban, cyclical, wage labor market. The fraction of the labor force employed in skilled, non-farm occupations and belonging to labor unions also increased. Sharp regional and nationwide downturns struck the nation before and after the First World War. Together, these forces may have accounted for something like a fifth or a quarter of the change in attitudes necessary to induce the majority of the population to prefer suppression.[18] Changes in patterns of migration had less of an effect. Between 1895 and 1925, immigrants as a percentage of the population increased by only a few percent as did the share of the population born in Southern and Eastern Europe. Together, these changes in the nature of immigration should have increased opposition to immigration slightly. Thus, changes in economic and demographic conditions cannot have been the sole cause of the resurgence of anti-immigrant sentiments. The relationships between those conditions and attitudes towards immigration must also have changed after the turn of the century.

Historiography highlights a potential explanation for such a shift. The eugenics movement, which spread during the early decades of the twentieth century, increased sensitivities to ethnic and racial differences and brought racial concerns to the forefront of the anti-immigrant debate. Eugenicists argued that immigration enabled inferior individuals from Southern and Eastern Europe to enter the nation, marry the native-born, pollute American's superior bloodlines with inferior genes, and degrade the Nation's descendants. By demonstrating that economics concerns predominated immediately before the dissemination of Galton's conception of scientific racism, the Kansas surveys suggest that the eugenics movement may have had a large impact on the politics of immigration during the first half of the twentieth century.

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[18] This share is calculated by multiplying the changes in the variables with the coefficient concerned and noting its influence on the average predicted probabilities of the various outcomes.

APPENDICES

This segment of the study contains two series of appendices. The first set describes the collection, collation, and construction of the data set. Topics covered include

A. Geographic and Birthplace Codes
B. Standardized Income Information
C. Occupations
D. Wage Changes

The second appendix discusses methodological issues and the robustness of the regressions.

DATA APPENDICES

The surveys of the Kansas Bureau of Labor and Industry (hereafter KBLI) present several challenges that must be overcome to put the data in useable form. First, the data is raw. The bureau published all of the returns that it received in useable form without double-checking the data for errors or inconsistencies. Second, the coding schemes imbedded in the survey do not coincide with those used by other agencies. Definitions are often unclear. Categories are often incompatible with those of the census bureau. Clarifying these issues allows information from other sources to be integrated with the Kansas surveys. The following appendices explain how to solve these problems and prepare the Kansas Surveys for statistical analysis. The also describe the structure of the surveys and features of the data that influence methods of analysis and regression results.

The Kansas Bureau of Labor and Industry collected information concerning attitudes towards immigration during its annual surveys of workers, wages, and the cost of living. The bureau collected this information by preparing blank forms with questions similar to those asked "by the leading labor bureaus of the country, with such modifications as were necessary to apply to conditions and affairs peculiar to our state."[19] Agents "scattered" these questionnaires throughout the state being "careful that no particular branch of labor should receive especial notice in distribution of the forms, but that every class of working men … should be given the opportunity of replying, setting forth their current condition, and expressing their views on current questions affecting their own welfare."[20] The method of distributing questionnaires evolved from year to year. In 1895, the bureau distributed most forms by mail. A stamped, self-addressed, return envelope accompanied each form. The bureau distributed others when its

[19] KBLI 1895 p. 96
[20] KBLI 1895 p. 96

Commissioner visited various businesses on errands unrelated to the survey itself. In 1896, the method was similar, with one addition. The bureau requested assistance from local unions to which they "sent bunches of self-addressed and stamped envelopes, to be distributed at regular meetings." In 1897, the bureau refined its methods. It conducted a stratified, random, mail-out/mail-back survey. Clerks obtained the names and addresses of thousands of wage earners from their employers and selected a subset covering most wage-earning occupations and counties in the state. The 4250 members of the subset received through the mail an introductory letter, a questionnaire, and a return envelope with postage attached. Another 750 questionnaires were distributed by the Commissioner during his travels for other purposes or "given to the employers of labor, who handed them direct to their employees."[21] The result was

> A greater number of persona and occupations than have heretofore been secured by this department in any one year. It gives expression to the voice of labor, and is representative, in a fair degree, of its conditions and desires, embodying the expressed will of the bone and sinew of our state, from the day laborer to the skilled workman of factory, farm, and shop.[22]

The Bureau published all replies deemed reasonably reliable and complete. Statisticians did this with "a rigid adherence to the laws of impartiality and disinterested personal opinion … [and] conscientious efforts to report exactly the language and figures given" in hopes of setting "forth, verbatim, the facts as given by the correspondents."[23]

These methods had benefits and costs. The obvious benefit was the data itself. Survey sampling was in its infancy. Modern methods did not exist. Budgets were tight. The bureau lacked money and manpower. It did the most that it could with the resources at hand. The mail-out/mail-back expedient maximized information and minimized costs. Cheaper alternatives could not generate comparable information. Superior alternatives were too expensive. Stratification also had a salutary effect. It reduced the number of observations needed to illuminate the correlations of interest, since more variation exists among explanatory variables in stratified samples than in purely random samples of the same size. This additional variance reduces the standard errors and increases the accuracy of parameter estimates.

Additional concerns exist. Clustering presents the biggest potential

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[21] KBLI 1897 p. 221
[22] KBLI 1897 p 237
[23] KBLI 1895 p. 96

problem. Few details of the sampling process survive, and those that do suggest clustering occurred at least on occasion. Sampling depended upon the availability of addresses, the efforts of clerks, and the whims of the Commissioner. Numerous observations from particular workplaces indicate that surveyors occasionally took shortcuts such as sampling groups of individuals employed by one firm. Such clusters do not contain observations drawn randomly and with replacement. Therefore, the number of independent observations trails the number of total observations. This difference confounds typical methods of calculating the precision of parameter estimates that use total observations as a proxy for the number of independent observations. Confidence intervals are too narrow. Test statistics are too large. Estimates appear more precise than they actually are. Stratification poses less of a problem. Bias occurs when strata vary along with the variation to be explained. Bias might occur in the Kansas data. It would exist if clerks established the strata with the dependent variable in mind. Extant documents, however, suggest clerks did not make that elementary mistake. Moreover, summary statistics reveal no troubling correlations. These facts suggest that stratification did not bias the Kansas surveys and that estimates drawn from them should be unbiased.

The mail-out/mail-back expedient also poses potential, but not practical, problems. The disadvantages of the method include inaccurate and incomplete responses. Individuals may have misinterpreted questions, answered untruthfully, or concealed embarrassing information. Some failed to respond. These shortcomings could complicate the interpretation of the evidence, but they present no insurmountable hurdles, because the Bureau of Labor and Industry took steps to mitigate their ill effects. "The Commissioner, in person, visited 119 towns and cities for the purpose of waiting upon those to whom our question blanks had previously been sent."[24] The introductory letter accompanying the questionnaire provided detailed instructions and attempted to elicit truthful, prompt replies. An appeal to civic consciousness asserted

> One of the objects and purposes of this Bureau is to present, through statistics, the true condition of the wage earner, that his interests may be the better protected and promoted through legislation. That this may be done fully and conscientiously, it is absolutely necessary that you contribute your part in this great work by filling out the attached blank, and return it to this Bureau at your earliest convenience.[25]

[24] KBLI 1896 pp. 10-11

[25] Kansas Bureau of Labor and Industrial Statistics 1898

A reference to state law warned that recipients of the survey who shall …

> Neglect or refuse to fully answer and return [the questionnaire] … shall be deemed guilty of a misdemeanor, and upon conviction thereof before a court of competent jurisdiction shall be fined in the sum not exceeding fifty dollars, or by imprisonment in the county jail not exceeding days, or both such fine and imprisonment.[26]

A pledge of confidentiality reassured those who replied that their identities were safe from prying eyes. A stamped, return-addressed envelope meant the cost of replying was the time needed to complete the questionnaire. In sum, the Bureau did everything it could to ensure that no harm would come to those who filed truthful responses while threatening to punish those who did not. These inducements seem to have had salutary effects. The response rate for the 19th century surveys compares favorably with those of modern times.

APPENDIX A: GEOGRAPHIC AND BIRTHPLACE CODES

Constructing codes for birthplaces was complicated by three facts. First, some individuals provided more detail such as the state or city of birth than others who indicated only the nation or continent. Second, the boundaries of European states shifted repeatedly and substantially during the latter half of the nineteenth century and the first half of the twentieth. Third, the three data sets used in this essay – the Kansas Bureau of Labor and Industry surveys (KBLI), the county-level census tabulations (CCT), and the Integrated Public Use Micro Samples (IPUMS) recorded birthplace in different ways. To standardize responses across individuals, years, and data sets, codes for birthplaces were assigned as follows.

USA, the indicator for the United States, includes everyone in the:

- KBLI listed as being born in the United States, a particular state of the United States, or America. In addition, one individual's mother, who is listed as a Negro but whose birthplace is not noted, is assumed to have been born in the United States, since during the post-bellum period, extremely few negroes migrated to the U.S.. One individual's father, who is listed as Indian (i.e. Native American) but whose birthplace is not noted, is also assumed to have been born in the United States.
- CCT listed as being native-born, born in the United States, or born in a particular state of the United States.
- IPUMS listed as being born in a particular state of the United States, an

[26] Kansas Bureau of Labor and Industrial Statistics 1898

unincorporated territory of the United States, or regions at the time outside of the United States that later merged with the United States.

CANADA, the indicator for our northern neighbor includes everyone in the:

- KBLI listed as being born in Canada and its internal subdivisions such as Ottawa, Ontario, and New Brunswick.
- CCT listed being born in Canada and Newfoundland

UK, the indicator for Great Britain, includes everyone in the:

- KBLI listed as being born in England, Scotland, and Wales. In addition, it includes several individuals born in Egypt and India who appear to have been of English ancestry but born in the outskirts of the empire before they immigrated to the United States.
- CCT listed as being born in England, Scotland, Wales, Australia, and India.

GERMANY, the indicator for regions of Europe where German languages predominated, includes all individuals in the

- KBLI born in Austria, Bavaria, Bohemia, Germany, Holland, Luxembourg, Netherlands, and Prussia.
- CCT born in Austria, Bohemia, Germany, Holland, Luxembourg, and Netherlands.

FRANCE, the indicator for regions of Europe where a significant portion of the population spoke French, includes all individuals in the

- KBLI born in France, Belgium, and Switzerland.
- CCT born in France, Belgium, and Switzerland.

SCANDIN, the indicator for Scandinavia, includes all individuals in the

- KBLI born in Denmark, Finland, Norway, and Sweden.
- CCT born in Denmark, Finland, Norway, and Sweden

NORTH, the indicator for Northern and Western Europe, includes all individuals born in CANADA, UK, GERMANY, FRANCE, and SCANDIN for all of the data sets.

EAST, the indicator for Eastern Europe, includes all individuals in the

- KBLI born in the Czechoslovakia, Rumania, Russian Empire, Poland, and Bulgaria.
- CCT born in Czechoslovakia, Rumania, Russia, Poland, and Hungary

IRELAND, the indicator for Ireland, includes all individuals in the

- KBLI born in Ireland, Eire, or Ulster.
- CCT born in Ireland

SOUTH, the indicator for Southern Europe and Hispania, includes all individuals in the

- KBLI born in Greece, Italy, Spain, Mexico, Portugal, Turkey, Central America, and South America.
- CCT born in Greece, Italy, Cuba, Portugal, Turkey, and the West Indies, Mexico, Central America, and South America.

OTHER indicates those individuals in the

- KBLI who listed their birthplaces as Africa and Europe but provided no details about where within those continents or in any other way not classified above.
- CCT who listed their place of birth as "other" or "at sea" or in any other way not classified above.

NONNORTH, the indicator for all individuals born outside the Protestant regions of Northwestern Europe, includes all individuals born in EAST, IRELAND, OTHER, SOUTH.

APPENDIX B: STANDARDIZING INCOME INFORMATION

Income comes from several sources. Workers earn wages, which are returns to labor. Savings earn interest, which are returns to capital. Multiple wage earners may live in a household. A worker may have more than one job. All of these phenomena complicate my calculations, as do idiosyncrasies within the surveys of the Kansas Bureau of Labor and Industry (hereafter KBLI). In the surveys, some sources of income were measured poorly. Others were imprecisely defined. A few definitions varied from year to year. Many individuals reported a wage rate, in terms like dollars per day or dollars per week, but not the number of days or weeks that they worked. This appendix describes my procedures for overcoming these difficulties for the surveys of 1895, 1896, and 1897.

In the 1895 survey, individuals reported earnings (either wage or salary) per day, week, month, or year. I use this information to create a measure of income that can be compared across individuals. I construct the variable "Standardized Annual Earnings" with the following algorithm. Its rules are applied sequentially. If line n returns a value for standardized annual earnings, then the figure is established, and lines $m > n$ of the algorithm are not applied.

	Kansas Survey			Our Variable
1.	If earnings per day > 0, then	earnings per day * δ	=	standardized annual earnings
2.	If earnings per week > 0, then	earnings per week * λ	=	standardized annual earnings
3.	If earnings per month > 0, then	earnings per month * 12	=	standardized annual earnings
4.	If earnings per year > 0, then	earnings per year	=	standardized annual earnings
5.	None of the above		=	missing value

The symbol δ converts earnings per day into earnings per year. A lower bound estimate of δ is the median number of days worked by individuals in the 1897 KBLI survey for whom the figure could be calculated. That figure is 240. I use the 1897 figure, rather than an 1895 figure, because insufficient information exists to estimate an amount for 1895 amount. An upper bound estimate is the average days in operation for a manufacturing firm according to Rees (1961, Table 10, page 33). The 1895 value is 284. The symbol λ converts earnings per week into earnings per year. The low estimate is the median number of weeks worked by individuals in the 1897 Kansas Bureau of Labor and Industry survey for whom the figure could be calculated. That figure is 48. The high estimate is the average weeks in operation for a manufacturing firm according to Rees (1961, Table 10, page 33). That value comes from dividing 284 by 5.5. The value is 51.6. The order of the sequence matters in only one case. Individual # 314 reported earnings of $50 per month and earnings per year of $72. In this case, the monthly value seems appropriate. The individual was a policeman with 4½ years of experience. Similar individuals earned $50 per month. The individual supported a wife and four children. That would be difficult and perhaps impossible on $72 per year.

Corrections must be made to several other observations. Some observations contain no information about income. These must be dropped from the data set. They include # 77, 83, 88, 138, 146, 150, 230, 231, 248, 251, 257, 333, 360, and 390. Other observations contain typos. Observations # 27, 29, 30, 31, and 32 appear as ¢60, ¢60, ¢60, ¢60, and ¢30 per month respectively. Those values could not be correct. No worker could have survived on such a small sum. No one working in the fire department or the police department made such a minimal amount. The typesetter must have displaced the decimal point. The correct figures should be $60, $60, $60, $60, and $30 respectively. Observations # 161 and 162 appear as $15 and $30 per week respectively. Those amounts were unheard of for a 26-year-old clerk with 2 years of experience and a 30-year-old

laborer with 4 months experience. Those amounts are also inconsistent with reported annual earnings of $180 and $385 respectively. The annual amounts indicate the "weekly" earnings appear in the wrong column. They should be $15 and $30 per month respectively. I identify these typos like I identify all others. I seek observations with three characteristics. One is an obvious internal inconsistency such as wage rates incompatible with annual income. Two is an obvious external inconsistency such as wages much higher or lower than those earned by other individuals in the same occupation. Three is a series of consecutive observations with the same problem. If a single, simple typesetting error, such as neglecting to place a zero after a decimal point, could have created all of these errors and seems to be the most parsimonious explanation of them, then I presume it to be the source of the problem and correct it.

The 1895 survey tracks two additional sources of income. The first is earnings of other members of the family. In some families, both parents work. In other families, the children work as well. In a few families, additional adults – perhaps relatives or grown children – also work for wages. The KBLI reports this income in the variable *TOTEAR*. The initials stand for "earnings of all members of the family." Few respondents answered that question, perhaps because the directions were incomplete and confusing. Idiosyncrasies among the answers may also reflect that fact. Some answers appear to be in terms of annual amounts. Others appear to be on a time scale, such as earnings per week or month, similar to the earnings reported by the head of the household. Some answers appear to include the earnings of the household head. Others appear to exclude that amount. Appearances, of course, can be deceiving. Because intelligently interpreting this variable is impossible, I exclude it from the analysis, except for using it as one of the consistency checks described in the preceding paragraph. A different variable allows us to determine whether multiple members of a household earned income. *FAMWK* reports the number of members of a family working for wages. If $FAMWK > 1$, then I assign the value of one to the categorical variable for a multi-earner household. If $FAMWK \leq 1$, then I assign the value of zero. The second source of income is labeled "all other." It includes income from renting rooms, interest on investments, and similar sources. I code this information in a straightforward manner. If the KBLI variable *OTHINC* is greater than zero, then I assign that value to the variable *Other Income*. In all other cases, the variable *Other Income* is set to zero.

The 1896 survey reported incomes almost identically to the 1895 survey. So, the procedures that I use to calculate potential annual earnings for 1896 are almost identical to those that I used for 1895. The algorithm again applies the following rules sequentially. If line n returns a value for annual wage, then annual wage is established, and lines $m > n$ of the algorithm are not applied.

	Kansas Survey			Our Variable
1.	If earnings per day > 0, then	earnings per day * δ	=	standardized annual earnings
2.	If earnings per week > 0, then	earnings per week * λ	=	standardized annual earnings
3.	If earnings per month > 0, then	earnings per month * 12	=	standardized annual earnings
4.	If earnings per year > 0, then	earnings per year	=	standardized annual earnings
5.	None of the above		=	missing value

The order of the sequence matters in only one case. Individual # 353 reported earnings of $1.50 per day and earnings $15 per week. In this case, either figure seems appropriate. He states his occupation as printer. The earnings of printers in the data set ranged from $1 to $3 per day, which is $5 to $15 per week. So, either value could be correct. I go with the lower figure, to remain consistent with my actions in the other data sets, while noting that changing the decision would not influence the results of the analysis. The symbol, δ, converts earnings per day into earnings per year. It is the average days in operation for a manufacturing firm according to Rees (1961, Table 10, page 33). The 1896 value is 274. Dividing 274 by the average number of days worked per week, 5.5, and rounding down, gives us the value of λ, which converts earnings per week into earnings per year. The 1896 value is 49.8. Observations dropped from the data set because they contained no income information include # 9, 22, 60, 64, 67, 87, 100, 102, 108, 116, 156, 165, 179, 218, 228, 278, 279, 288, 354, 359, 419, 425, 465, 468, 494, 496, 497, 515, 517, 519, 521, 526, 529, and 535. Observations dropped because they contained obviously erroneous information include # 86 and 351. These observations contain weekly wage figures, $50 and $60 are unheard of for their occupations, butcher and printer. Once again, they appear to be monthly wage figures accidentally placed into the weekly wage column.

The 1897 survey reported incomes in a manner similar to the 1895 and 1896 survey. So, the procedures that I use to calculate potential annual earnings for the earlier surveys are similar to those that I used for 1897. The algorithm again applies the following rules sequentially. If line *n* returns a value for annual wage, then annual wage is established, and lines *m* > *n* of the algorithm are not applied.

	Kansas Survey			Our Variable
1.	If wage per hour > 0, then	wage per hour * θ	=	standardized annual

				earnings
2.	If wage per day > 0, then	wage per day * δ	=	standardized annual earnings
3.	If wage per week > 0, then	wage per week * λ	=	standardized annual earnings
4.	If wage per month > 0, then	wage per month * 12	=	standardized annual earnings
5.	If wage per year > 0, then	wage per year	=	standardized annual earnings
6.	None of the above		=	missing value

The order of the first four rows matters in only one case. Individual # 244 reported wages of $0.15 per hour, employment on average 7 hours per day, wages $45 per month, and wages of $30 per year. All of these claims cannot be true. The monthly figure is inconsistent with the former and latter pieces of information. The figures for hourly wage and employment indicate a daily wage of $1.05, which can never amount to $45 per month. The sequence imposes a parsimonious solution. The monthly figure is assumed to be erroneous and ignored. The remaining figures are consistent with each other and all else know about individual #244. The symbol, δ, converts earnings per day into earnings per year. It is the average days in operation for a manufacturing firm according to Rees (1961, Table 10, page 33). The 1897 value is 285 Dividing 285 by the average number of days worked per week and rounding down, yields the value of λ, which converts earnings per week into earnings per year. The 1897 value is 50. Multiplying 285 by the average number of hours worked per day, approximately 10, yields the value of θ, which converts wages per hour into wages per year. The value is 2850. The average number of hours worked per day is calculated from the data set by averaging the value for all workers paid hourly wages during 1897. Observations dropped from the data set because they contained no income information include # 214, 261, 264, 265, 267, 271, 273, 480, 481, 482, 758, 879, and 1146. Observations dropped because they contained information only on wages per mile or per ton, which I could not standardize, include # 36, 40, 41, 44, 46, 53, 54, 58, 60, 63, 68, 83, 74, 92, 94 to 97, 109, 113, 115, 116, 118, 124, 128, 133, 135, 136, 148, and 761.

The 1897 survey contains an additional complication. Some individuals received part of their pay in scrip. These dollars-denominated coupons had to be used at the company store, where prices exceeded those in local markets. To adjust for this difference in prices, I use the following formula.

Corrected wage = *wage* (1-*PCTSTORE*) + *wage* (*PCTSTORE*) / (1+*COMINC*)

Where *wage* indicates wage information from the Kansas survey. *PCTSTORE* is the variable in the Kansas survey that indicates the percentage of an individual's wage paid in scrip. *COMINC* is the variable in the Kansas survey that indicates the percentage of "increased cost of merchandise when paid in scrip." Observations effected by this transformation include # 761 through 808 excluding 784, 795, 800, and 807.

APPENDIX C: OCCUPATIONS

Categorizing occupations is complicated. No standard classification system existed at the end of the nineteenth century. Job titles varied across space and time. Jobs differed along innumerable dimensions. Nineteenth-century occupational classifications focused more on work settings and economic sectors than on a worker's specific technical function. To overcome these hurdles, I have adopted a classification scheme devised by the U.S. Census Bureau and the Integrated Public Use Micro Sample Project at the University of Minnesota.[27] Their methods allow us to translate the idiosyncratic array of occupational titles that appear in the Kansas surveys into a useful set of occupational and industrial variables.

We converted the original replies into useful data in the following manner. First, I noted the occupation attributed to an individual in the Kansas Survey. Second, I found the corresponding entry in the IPUMS occupational dictionary, a compendium of all occupational entries encountered by the IPUMS research staff and the Bureau of the Census.[28] Third, I noted the OCC1950 and IND1950 codes associated with that entry. I used those codes to categorize the individual's job tasks, because they contained more useful information than the census bureau classification schemes for 1890 and 1900 censuses. In those decades, the Census Bureau grouped information about jobs into industrial groupings, such as agricultural pursuits, domestic and personal service, manufacturing and mechanical trades, trade and transportation, and non-occupational activities, such as student, retiree, and housewife. OCC1950 and IND1950 recode those groupings into the 1950 Census Bureau occupational and industrial classification scheme. This allows us to distinguish the tasks that individuals performed on the job and the industry in which that individual worked. Finally, I converted the

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[27] Steven Ruggles and Matthew Sobek et. al. *Integrated Public Use Microdata Series: Version 2.0*
Minneapolis: Historical Census Projects, University of Minnesota, 1997.

[28] The composition of the occupation categories is described in detail in U.S. Bureau of the Census, *Alphabetic Index of Occupations and Industries: 1950* (Washington D. C., 1950).

OCC1950 codes into trinary variable. Codes from 1 to 299 and code 523, which correspond to managerial, official, professional, proprietary, and technical positions, were assigned the label *specialized and supervisory workers*. Codes 304, 325, 335, 340, 490, 700 to 751, 753, 760, 761, 763 to 770, 780, and 783 to 970, which correspond to clerical, sales, and service sector personnel, household servants, and laborers, were assigned the label *standard workers*. All other codes, which corresponded to experienced operatives and craftsmen, were assigned the label *skilled workers*.

APPENDIX D: WAGE CHANGES

This appendix explains how to generate the variables *wage changed* and *percent by which wages changed*, below denoted *PWgChg*, from the Kansas surveys' information on those issues. The former answers the question "did wages rise or decline relative to the previous year or did they remain steady?" The later answers the question did "by what percent did wages change from the previous year?" The initial table describes how to construct the codes for the years 1895 and 1896.

Our Variable		Kansas Survey			
Wage changed =	if	WGINC		WGDEC	
		defn	code	defn	code
0		same	-8	same	-8
0		no response	-9	no response	-9
1		yes	-7	no response	-9
1		# %	#	no response	-9
missing		yes	-7	~ no response	
missing		# %	#	~ no response	
-1		no response	-9	yes	-7
-1		no response	-9	# %	#
missing		~ no response		yes	-7
missing		~ no response		# %	#
PWgChg =	if	WGINC		WGDEC	
		defn	code	defn	code
0		same	-8	same	-8
0		no response	-9	no response	-9

#	+ # %	#	no response	-9
median (+#%)	yes	-7	no response	-9
missing	yes	-7	~ no response	
missing	# %	#	~ no response	
- #	no response	-9	-# %	#
median (-#%)	no response	-9	yes	-7
missing	~ no response		yes	-7
missing	~ no response		# %	#

Where the symbol "~" implies not what follows. Median (+#%) means the median of all percentage rises in wages. Median (-#%) means the median of all percentage declines in wages.

The next table describes how to construct the codes for 1897.

Our Variable		Kansas Survey	
Wage changed =	if	IFCHGWG	
		defn	code
-1		Decreased	2
0		No response	-9
0		No	1
1		Increased	3

PWgChg =	if	IFCHGWG		and	CHWAGE	
		defn	code		defn	code
#		increased	3		# %	#
median (+#%)		increased	3		no response	-9
- #		decreased	2		-# %	#
median (-#%)		decreased	2		no response	-9
- 10 / 410		decreased	2		by $10/year	-8
0		All other responses				

Where median (+#%) means the median of all percentage rises in wages. Median (-#%) means the median of all percentage declines in wages.

METHODOLOGICAL APPENDIX – ROBUSTNESS OF REGRESSIONS

Checking the robustness of the estimates is important, because the Kansas Bureau of Labor and Industry collected the data before statisticians perfected modern methods of survey sampling and because surviving documents only suggest, but do not prove, that the methods which the Kansas Bureau used were as random and representative as modern methods. If the data were not collected with methods equivalent to modern, then the standard statistical assumptions about the distribution of the error term on which the accuracy of the estimates depend might be violated. The estimates of coefficients and standard errors might be biased. The hypothesis tests might lead us astray. The conclusions that I draw from them could be incorrect. This appendix demonstrates, however, that the estimates are accurate.

Three issues are of obvious importance. The first is clustering. Clustering occurs when the error terms of observations within clusters are not independent. The standard errors of estimated coefficients calculated via conventional formulas tend to be understated. I correct for clustering using Rogers/Williams methods of applying to clustered samples the sandwich-style estimators of robust standard errors devised by Hubert and White.[29] This corrects the standard errors for the correlation of error terms within clusters. It also corrects the standard errors for problems caused by model misspecification, which is a possibility that should concern us, since the century-old data almost certainly lacks information about some factors that influenced peoples' attitudes towards immigration.

The second issue is related to that concern. There may be aspects of individuals' neighborhoods, occupations, or industries that the data do not reflect. These excluded explanatory variables could bias both the coefficients that I estimate and the standard errors that form the foundations of the hypothesis tests. To account for this possibility, I took the standard steps to correct for county, occupation, and industry fixed effects. I created dummy variables for each county, industry, and occupation, then added these dummy variables to the matrix of explanatory variables, and then reran the regressions. I also interacted the dummies for counties, industries, and occupations with the key explanatory variables and reran the regressions again. The signs and significance levels of the key coefficients in these regressions, which I do not report to save space, correspond closely to those in the regressions that I do report, which increases the confidence in the correctness of the conclusions.

The third concern comes from the construction of the variable income. As Appendix A explains, the Kansas Bureau of Labor and Industry collected

[29] Hubert 1967, Rogers 1993, White 1980, Williams 2000

information about income in a variety of formats. Some individuals reported their wages per hour. Others reported wages per day, week, month, or year. Some reported total earnings for various time periods. Others reported earnings of family members as well as their own. Some of the observations contained obvious typos. Others contained contradictory information. The appendix describes the procedures with which I cleaned, corrected, and standardized that data to create the variable *income*. If done poorly, of course, such standardization could influence the regression results. It could, in fact, entirely determine the results. In that case, the correlations on which I base my conclusions would be nothing more than an artifact of the methods with which I cleaned the data. To allay the fears about this concern, I employ two different methods. One resembles the procedure with which I controlled for fixed effects. I construct dummy variables indicating the manner in which individuals reported their incomes. If an individual reported receiving a certain wage per hour, for example, I assign them the value of 1 for the dummy variable indicating wage per hour and the value 0 for all other method of payment dummy variables. Then, I interact all of these method of payment dummy variables with the variables for *income* and *income squared.* Finally, I include these dummy variable and interactions in the regressions, and report the results in Table 6, columns (2) and (4). The addition of these dummy variables and interactions allows the coefficients of *income* and *income squared* and the cut points of the ordered probit to vary along with the method in which individuals reported payments and thus, along with the method with which I standardized the data. This frees the coefficients from the effects of the standardization procedure. The dummy and interaction variables capture the patterns unique to each method of payment. The variable income, as intended, captures the patterns common across the various indicators of income.

The other method involves dropping the variables *income* and *income squared* from the regressions, and in their place, adding a series of variables containing information about income in the form originally reported and the dummy variables indicating the method of payment. In this case, if an individual reported receiving a certain wage per hour, I assign that wage to them in the variable *wage per hour* and assign the value zero to them in all other variables for the various methods of payment. Then, I rerun the regressions including all of the other explanatory variables. I do not report the results of this method, because it yields the same result as the previous method, but requires the reader to examine the coefficients on 33 different variables (the values, values squared, and intercepts for11 different measures of income) rather than the two coefficients on *income* and *income squared* that I report in Table 6.

Table 6 reports the robust regressions in Columns (2) and (4). Column (2) is the robust version of the regression in Column (1). Column (4) is the robust version of (3). Comparing the robust regressions to their original versions proves

an important point. Correcting for the obvious potential econometric problems changes no important results. The signs and significance levels of the key variables remain unchanged. In most cases, even the magnitudes of the coefficients remain the same (in the sense that two sample t-tests cannot reject the hypothesis that the original coefficient equals its robust counterpart). The coefficient on the year 1897 is the only exception. The similarity of the original and robust results indicates that the methods reveals real correlations within the data rather than spurious correlations generated by departures from classical statistical assumptions or the calculations required to prepared the data for analysis.

A series of additional tests for robustness, which I do not report, corroborate that conclusion. Several dozen observations contain imputed values for the variables *years in occupation* and *hours worked per day*. This raises an obvious concern. The imputation process could create correlations not otherwise observed. To ensure that this does not occur, I rerun all regressions without the observations containing imputed values. The results do not change, which indicates imputation does not produce spurious correlations. Similarly, several hundred observations contain non-responses to the question, "do you favor restriction or suppression of foreign immigration?," from which I derived the dependent variable. The regressions printed in this paper treat the non-response as the equivalent of no. This treatment could also generate spurious correlations. I ensure that it did not in four ways. First, I estimate all regressions after discarding all of the non-responses. The qualitative results remain the same. Second, I ask, what percentage of the *no/non responses* would I have to recode as *suppress* in order to reverse the regression results. Iteratively estimating the regressions while assigning larger shares of the *no/non responses* to the *suppress* category show that nearly all of the former must be assigned to the latter before confuting the conclusions. That assignment is implausible, indicating that the handling of the no/non responses did not distort the results. Third, I use data from 1895 and 1897, the years that distinguish the answer *no* from *non-response*, to estimate the probability of non-response. I found that non-respondents resembled individuals who answered no. Then, I use that information to adjust the ordered probit models for non-response by estimating the answers of the non-respondents and via a nested logit. Once again, these adjustments have little influence on the results. Finally, I reestimate all of the regressions using only the observations of individuals responding *restrict* or *suppress*, for whom the dependent variable is unambiguous. These probit regressions yielded results with the same qualitative characteristics as the ordered probit regressions on the entire data set, which once again, allayed concerns over potential confusion of *no* and *non* response. Estimating this probit model and a similar probit differentiating between no/non responses and restrict/suppress responses alleviates a more general concern about

the efficacy of ordered probit models, which are more sensitive than probits to misspecification and departures from classical statistical assumptions.

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