

PASSENGER SHIPPING CARTELS AND THEIR EFFECT ON TRANS-ATLANTIC MIGRATION¹

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Abstract

We investigate the impact of passenger shipping cartels on trans-Atlantic migration during the early twentieth century. We assemble from primary sources a detailed database of passenger flows and cartel operations and show that cartel operation reduced migratory flows by approximately 20% to 25%. Further, we show that there was no strong intertemporal substitution in migration to North America (at least in the short-run) and, therefore, that the effects of cartel operation were not “undone” by later migration. Lastly, we find that cartel operation had no appreciable effect on the variability of migration flows, providing evidence against the notion that unfettered competition was destabilizing to turn-of-the-century transportation markets.

JEL Codes: N70, L13, L43, L92, F22.

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1. INTRODUCTION.

Can mass migrations, typically attributed to “macro” factors such as economic conditions, social upheavals and technological change, also be influenced to a large extent by “micro” factors, such as the structure of the transportation industry that carries the migrants? The overwhelming portion of the literature attributes migratory flows almost exclusively to macro phenomena and ignores the impact of micro phenomena. In this paper we demonstrate that micro phenomena, such as the cartelization of the trans-Atlantic passenger shipping industry, can have large effects on migration, with potentially long lasting demographic consequences.

The immense trans-Atlantic movement of the 19th and early 20th centuries is perhaps the historical migration that has received the most scholarly attention. Some of the literature has focused on long-run measures of migration, using for example decadal data. Such studies have found strong evidence supporting the importance of labor market indicators, demography, lagged migration, and economic structure. Other studies have utilized annual data, and have confirmed the importance of short-run labor market fluctuations.² Past work has recognized the desirability of including a measure of transport costs directly in econometric specifications, but there is no comprehensive set of passenger fares. Some scholars have reasoned, based on the limited data that exist, that transport costs after 1860 did not exhibit any long-run trend and therefore were unlikely to have been an important explanatory factor in the variation in migrant flows after that date. The prevalence of emigrants traveling on tickets prepaid by relatives in North America, is also pointed to as a reason that the cost of transport might not have been important. Further, the salient feature of transport for many scholars has been the transition from sail to steam, which some scholars believe aided migration through an improvement in quality of transport, rather

than the much less well known phenomenon of shipping cartels.³ Prior work on migrant transport during this period has discussed passenger shipping cartels, but there is no consensus on how effective they were and no quantitative examination of their effects (Hyde, 1975, Keeling, 1999a and 2005, Moltmann, 1989). This may be due in great part to the fact that, in contrast to freight rates, there do not exist sufficiently rich data on passenger fares or (prior to this paper) a comprehensive data-base of flows at the route level and a systematic classification of the cartelization of routes over time.

In this paper we investigate the degree to which widespread collusion in trans-Atlantic passenger shipping had an impact on migration flows. In order to accomplish this, we first obtain detailed information on the passenger cartels and use it to identify the cartels with organizational structure and internal incentive schemes that resulted in highly effective collusion. Our sources of detailed information on these cartels consist of the evidence and briefs presented in an American antitrust suit, *U.S. v. Hamburg-American Co., et al.* (239 U.S. 466, 1916), the account of a high-ranking executive from one of the key firms published in 1922, a congressional inquiry, as well as a variety of secondary sources. We also identify cartels that were looser and less effective and periods/routes with no cartels. We next assemble from primary sources a unique new data set that consists of the number of steerage passengers carried on each individual trans-Atlantic voyage during the years 1899-1911. These data were collected by the cartel members themselves in an effort to monitor and enforce their agreements, and were eventually subpoenaed and offered into evidence in the aforementioned antitrust suit.

We then combine our classification of the passenger cartels with our passenger flows dataset to analyze whether market structure - in particular, the existence of well-organized cartels -

affected migration flows. We test the hypothesis that these cartels restricted migration flows below what would occur in a more competitive environment. This hypothesis is consistent with the “consensus” view of cartel operation. We also test the validity of the defense maintained by the cartel members which, in arguments presented in the U.S. antitrust case, claimed that the cartels, by achieving greater price stability, actually improved the immigrant trade and increased the quality of transport. Efficiencies were achieved, they argued, by insuring more stable steerage travel patterns.⁴ We test this hypothesis by investigating whether effective cartels lead to reduced variability of passenger flows. Our analysis shows that there is evidence in support of only the first hypothesis: Effective cartel operations resulted in lower passenger volumes. The magnitude of the effect is not only statistically significant (and robustly so); it is also large in an economic sense with effective cartel operation reducing migration levels by approximately one-fifth.⁵ We verify our findings using somewhat more aggregated immigration data in Ferenczi and Willcox (1929). Moreover, the results are robust across specifications including (i) a non-parametric treatment of time effects through the use of dummy variables, (ii) allowing for heterogeneity in the cartel response across routes, (iii) the use of alternative controls for economic activity, and (iv) whether a finer categorization of cartels is used. Finally, we show that there was no inter-temporal substitution of migration from periods of cartel operation (during which prices were high) to periods with no cartel operation (during which prices were lower). Therefore, a reduction in migration flows due to cartel operation was not offset by later increases in migration when the cartels were no longer operating. In light of changes in the geographical pattern and scale of migration flows after World War I, this suggests that the operation of the turn of the century passenger shipping cartels has had a measurable effect in shaping the North

American demographic profile.

This paper is organized as follows. Section 2 describes our classification of the cartel agreements, while section 3 summarizes the rest of the data, including the key features of trans-Atlantic data flows. The econometric methodology is developed in Section 4 while the results are presented in Sections 5 and 6. The paper ends with a few concluding remarks.

2. THE PASSENGER SHIPPING CARTEL AGREEMENTS AND THE ANTI-TRUST CASE.

In this section we present qualitative information that permits us to categorize each route according to the effectiveness of collusion during the period of our analysis.⁶ Different passenger shipping cartels served different regions. We designate routes according to the regions corresponding to cartel agreements and to how ports of origin are listed in the voyage data. Our data list every voyage that carried steerage passengers between Europe and North America between 1899 and 1911 and the number of steerage passengers transported. A port or ports of origin are listed for each voyage, as well as a port or ports of destination. It is clear from the data that the Mediterranean, for example, comprised a single route, as many vessels stopped in numerous ports in several countries in the region on their journeys (and indeed, often a voyage will list as a “port” of origin a region, e.g., Adriatic or Spain). Similarly, we designated Great Britain, Northern France, the Rhine and Scandinavia as routes (see section 3 for the ports included in each region). When there are direct trips from a region of origin to both Canada and the US, we distinguish two routes, one for each country of destination. All European ports from which emigrants embarked and all North American ports receiving them are included in our data.

At any given point in time, a cartel may or may not have been in existence on a particular

route. Furthermore, if a cartel did exist, it may or may not have been effective in preventing competition among participants or entry from outside lines. Thus, our task was to assemble information about both the existence and the effectiveness of cartels along each route.

Determining the existence of cartels was by far the simpler of the two. The voluminous record of the *U.S. v. Hamburg-American Co., et al.*, includes copies of every passenger cartel agreement in existence at any time from 1899-1911, and testimony from shipping company executives detailing the histories of the different cartels. The record makes clear when particular agreements were reached, and when they lapsed. We checked the legal record against authoritative contemporary sources, including Erich Murken's monumental *Die grossen transatlantischen Linienreederei-Verbande, Pools und Interessengemeinschaften*, articles from *Fairplay* (London), the *Journal of Commerce* (New York), the *Times* of London, the *New York Times*, diplomatic consular reports, investigations by the U.S. House of Representatives and the British Royal Commission on Shipping Rings, and the secondary literature.⁷

Our measurement of the effectiveness of cartels is binary - we designate them as either "tight" or "loose." Our criteria include signals of the likelihood of significant discord or cheating inside the cartel, as well as outside competition. Signals of internal cohesion are a well-defined incentive structure supporting collusion. These include the performance bonds posted by members upon admission, revenue pooling, an independent monitoring agency, and a strict division of exclusive territories. These criteria were chosen on the basis of what contemporary scholars and industry participants deemed to be valuable, and are consistent with the current theoretical literature on cartel stability.⁸ We identified the cartels that had these characteristics, and then corroborated their implementation through examination of correspondence and

testimony contained in the legal record. For example, the early Mediterranean agreement dated from 1896 but was in force only during the first two years of our sample. A high-ranking official of North German Lloyd steamship company described the agreement as a “general understanding” with respect to fares. Erich Murken says that the agreement was not always followed, especially in times of low demand, and that there were repeated unsuccessful attempts to organize a revenue pool. Based on this information, we classify the cartel as “loose” until it lapsed entirely.⁹ By contrast, other cartels of the same period (when present) were much more sophisticated in their operations.

Signals of outside competition were measured as whether there were significant inroads made in either attracting emigrants from a cartel’s route, or in affecting fares offered. For example, when the Cunard Line began service from the Hungarian port of Fiume in 1903, this began to significantly affect the flow of emigrants from the Rhine. An outright rate war spread across all trans-Atlantic routes and that lasted about one year. An uneasy peace resumed in northern European routes in late 1904, but efforts to maintain effective collusion were described as “desultory” by a shipping executive intimately involved in the negotiations, and evidence of the continued rivalries between lines is abundant in the correspondence. Based on the evidence, we are able to categorize the cartels as “loose” from the fourth quarter of 1904 until the third quarter of 1907, when hostilities between the lines again took the form of a trans-Atlantic rate war. The various agreements that were reached in 1908 and 1909 were the culmination of twenty-five years of experience, and resolved a number of earlier problems with respect to the division of territories, and disagreements over quotas, differential rates and pooling. The testimony and documents strongly support the contention that cooperation after 1908 was more effective and far-reaching than in the previous five years.¹⁰ Table 1 summarizes the above discussion on the

classification of the cartels into tight, loose and non-existent. Darkly-shaded boxes correspond to effective cartel quarter-route observations, lightly-shaded boxes correspond to loose cartels, and unshaded boxes correspond to no cartels or price wars.

The U.S. Department of Justice brought formal charges against cartel participants in 1911 under sections 1 and 2 of the Sherman Act, accusing the firms of restraining trade and attempting to monopolize North Atlantic passenger carriage through their cartel agreement and practices (exclusive contracts with ticket agents and “fighting ships”).¹¹ The defendants countered that the collusion was actually beneficial to the trade, and had “greatly increased and developed trade and intercourse with Europe” (Brief for North German Lloyd, II-III). They further maintained that rates were reasonable and that their costs could be “more economically distributed” under the pooling agreement because of greater stability of volume in the trade (Brief for North German Lloyd, IV). In 1914 the U.S. District Court, citing the landmark Standard Oil and American Tobacco cases that established a “rule of reason” for the application of the Sherman Act, decided to dismiss the government’s case.¹² Circuit Judge LaCombe found that the restraint on trade was “reasonable” because “the testimony...fails to satisfy us that the defendants, or any of them, have charged excessive or exorbitant rates for the transportation of passengers of any class.” The case was argued before the Supreme Court in late 1915, but the court never issued a verdict on the merits, instead dismissing the suit on the grounds that it was moot due to the start of World War I.¹³

3. DATA.

In this section we describe the data used in this analysis and summarize some of its key features. The first component of our dataset is information on trans-Atlantic voyages. We collected these data on each firm's carriage of passengers from the evidence and briefs presented in the abovementioned American antitrust suits, *U.S v. Hamburg-American Line, et al* (1914, 1916).¹⁴ These data were at the voyage level, which is a level of detail that was too fine for the purposes of our analysis. We proceeded to aggregate the number of passengers by calendar quarter, and geographic region. Therefore, our unit of observation is the number of passengers ferried from a particular origin region to a particular destination region in North America during a specific quarter. We distinguish between 5 origin regions (Britain, Mediterranean, North France, Rhine/Russia, and Scandinavia),¹⁵ and 2 destination regions (Canada and the US).¹⁶ Our division into these regions represents the minimal aggregation that is compatible with the pattern of regional coverage of cartel agreements and the pattern of sailings. This gives us a total of 10 possible markets (the Scandinavia-Canada pair has no passenger flows).

Our analysis focuses on third class (steerage) passengers, as we are interested in migration flows. The overwhelming majority of steerage passengers were migrants, rather than US citizens returning from a trip to Europe (Keeling, 1999b). In contrast, first and second class passengers, a small minority of the traveling public, are not likely (for the most part) to be moving to the US for the purpose of finding employment. Furthermore, if some of the first and second class passengers are traveling for the purpose of migration, they are not likely to be influenced much by the price of the fare, as they could downgrade to third class. Figure 1 Panel 1 shows the number of third class passengers ferried to North America in each quarter (the x-axis tick for the

year corresponds to the first quarter of that year). Three features are apparent: First, there is a strong seasonal pattern, with migratory flows peaking in the second quarter and being lowest in the first quarter. Second, there is a general secular increase in the flows. Third, passenger flows essentially “collapsed” during 1908, which reflects the very poor state of the US economy at that time. These three features play a role in shaping our econometric model. Panels 2, 3, and 4 of Figure 1 give the flows by region of origin for the three most important origin areas.

The number of separate westbound trips that carried third class passengers and the number of quarters with at least one westbound trip is given in Table 2. Notice that only six of the routes had sailings for every one of the 52 quarters in our sample. A seventh route had positive flows on almost all quarters, while two routes had zero flows for every or almost every quarter. As we discuss in greater detail below, this data structure has implications for our empirical strategy. We corroborate our analysis based on these passenger data through the use of more aggregated official immigration statistics, as obtained from Ferenczi and Willcox (1929).

We supplement our data on flows with data on labor market conditions, widely perceived to be the dominant factor for trans-Atlantic migration.¹⁷ We used the data in Williamson (1995) to obtain real wage series for the US, Canada, and eleven European countries (Britain, Ireland,¹⁸ Italy, Portugal, Spain, France, Germany, the Netherlands, Belgium, Sweden and Norway). These wage series have been used to construct wage series for the destination and origin location regions in our data set.¹⁹ We also obtained the NBER series of US factory employment and the unemployment series for the United States from Lebergott (1964). These wage and employment data are plotted in Figures 2 and 3. The US employment series exhibits three features: a secular rise until 1907, a rapid decline followed by a recovery in 1908 and 1909, and stability thereafter.

This pattern is mirrored in the unemployment series. Some of the wage series exhibit increasing trends, others are fluctuating around a constant level. These features of the data will shape, to some extent, our empirical strategy. We complete our economic series with GDP data for Canada, the US, Austria-Hungary, Britain, Italy, France, Germany, Belgium, Sweden and Norway, from which we constructed the GDP series for the destination and origin regions.²⁰ These data were also obtained from Williamson (1995). We do not include any variables to account for regulatory changes in immigration, because in both Canada and the United States immigration during this period was relatively unhindered and not subject to major new restrictions until after World War I.²¹

Finally, we classify the cartel status for each of the routes on any particular quarter as either tight/effective, or loose/ineffective, or absent/price war. This classification is described in the preceding section and also summarized in Table 1. There is substantial cross-section variation in cartel status prior to 1909, but all routes are under effective cartel agreements afterwards. We next turn to the development of the empirical model.

4. THE EMPIRICAL MODEL

As discussed above, the history of passenger cartels permits us to identify periods when there were effective agreements on given routes, periods when there were weak agreements, and periods with no agreements at all. By examining the level and variability of immigration flows during the periods of cartel effectiveness, we can analyze to what degree the cartels acted to restrict migration and to what extent they acted to “stabilize” the market, if at all. Because of the temporal correlation of effective agreements, it is necessary to control for other factors that

influence demand. We adopt a reduced form framework because there does not exist a reliable series of passenger fares (in section 6 we discuss the implications of some limited price data that are available).²²

We model log flows on any route pair as a trend-stationary process with deviations from trend being driven by a list of stationary regressors that include economic conditions in the destination and origin countries and the cartel status of the route. The log transformation underlies our assumption that the cartel form and the economic conditions are likely to have multiplicative effects on passenger flows. To control for the fact that routes differ in the “intrinsic” demand for travel, we include a comprehensive set of dummy variables for every route. A cursory look at Figure 1 reveals very strong seasonal effects on migratory flows. To account for those we include a full set of seasonal dummy variables. Also, as it can readily be seen from Figures 2 and 3, the wages, employment, and GDP for the various economies contain trends. We use as regressors the departures of these variables from the corresponding linear trend, i.e, the residual from the regression of the corresponding variable on a linear trend, and incorporate a linear trend in the regression to account for the trend in the dependent variable arising from all sources.²³ The U.S. unemployment rate, being a percentage, has no trend and is used as an explanatory variable without any transformation. Finally, we observe that the emergence of strong cartels in most routes coincides with a recession in the U.S.. Thus, to account for the possibility of non-linearities in the response of migration to economic conditions, we have also included (in most specifications) a dummy for the year 1908. Since almost all routes are strong cartels during 1908, the inclusion of a dummy for that year effectively removes the impact of that year in the calculation of the cartel effect (in a variation, we add year dummies

for every year). To maintain consistency with the flow regressions, we model the log variance of flows as a linear function of the same variables that are included in the mean regression.

Therefore, our “base” regressions are given by

$$\begin{aligned}
\log(Flow_{it}) = & \alpha_i + \beta_q Quarter_t \\
& + \beta_{spring} Spring_t + \beta_{summer} Summer_t + \beta_{fall} Fall_t + \beta_{1908} Year1908_t \\
& + \beta_{tight} Tight_{it} + \beta_{loose} Loose_{it} + \beta_{un} Unempl_t \\
& + \beta_{wo} WageOrigin_{it} + \beta_{wd} WageDestination_{it} + \epsilon_{it}
\end{aligned} \tag{1}$$

and

$$\begin{aligned}
\log(\sigma_{it}) = & \gamma_i + \delta_q Quarter_t \\
& + \delta_{spring} Spring_t + \delta_{summer} Summer_t + \delta_{fall} Fall_t + \delta_{1908} Year1908_t \\
& + \delta_{tight} Tight_{it} + \delta_{loose} Loose_{it} + \delta_{un} Unempl_t \\
& + \delta_{wo} WageOrigin_{it} + \delta_{wd} WageDestination_{it} + u_{it}
\end{aligned} \tag{2}$$

where i signifies the route, t the period, $Flow_{it}$ is the number of third-class passengers, $Quarter_t$ is a linear time trend, $Spring_t$, $Summer_t$, and $Fall_t$ are season dummies, $Tight_{it}$ takes the value of 1 if route i is under an effective (i.e., “tight”) cartel in period t , $Unempl_t$ is the Lebergott US unemployment rate, $WageOrigin_{it}$ and $WageDestination_{it}$ are the deviations from trend of the real wage of the origin and destination regions, respectively, $Year1908_t$ is an indicator variable for observations that correspond to 1908, and σ^2 is the variance of the disturbance term ϵ_{it} .²⁴ Notice that the regression intercept varies across the routes. In a variation, we replace the US unemployment rate by $USlabor_t$, the deviation from trend of the U.S. factory employment index. We do not have data on Canadian unemployment or industrial employment; for some specifications, we use the U.S. series as a proxy for the Canadian series as the industrial

cycle of the two countries is likely to be strongly correlated, while for other specifications we set the value of unemployment and employment index to zero for the routes that terminate in Canada (the results are essentially identical, and we report the later set). In addition to the above model, we also estimate variations in which (i) the Loose cartel dummy is excluded, (ii) cartel effects are allowed to vary by routes, (iii) the GDP (deviation from trend) of the destination and the origin regions replaces the corresponding wage rates, (iv) the economic variables and/or the 1908 dummy are excluded, and (v) the Tight and Loose cartel dummy is combined into a single indicator variable (the last two variations do not yield meaningful differences and are not reported). Furthermore, a number of similar models have been estimated to test for intertemporal substitution effects of migratory travel, cartel effects on eastbound flows, the presence of similar cartel effects when using annual immigration data from official sources, and the possibility of delayed cartels effects on passenger flows. The most common of the above variations is the exclusion of the Loose cartel dummy for two important reasons. First, it is estimated from a relatively small number of observations and reduces the number of observations in the omitted category, thus decreasing the precision with which the effects of the tight cartel are estimated; this is particularly important when interactions with routes are included or intertemporal substitution effects are to be estimated. Second, in terms of effects on passenger flows, loose cartels appear to be more similar to the absence of a cartel than to tight, well organized cartels.

Note that the regressions represented by equation (2) are effectively a test for heteroskedasticity of equation (1). One might argue that the hypothesis that shipping cartels stabilized passenger flows is more consistent with the reduction of the *unconditional* variance of flows, or at the very least, the variance of route flows conditional on only a trend and seasonal dummies. In other words, the cartels may have been dampening some of the effects arising from

changes in economic conditions. This suggests that the dependent variable in equation (2) may be best obtained not from the residuals of equation (1), but rather from the residuals of a more parsimonious regression that has only trend, seasonal dummies, and route fixed effects as independent variables. We have re-estimated equation (2) using the residuals of a more parsimonious versions of equation (1). For robustness, we have also estimated equation (2) itself using more parsimonious regressor sets than those used for regression (1). These variants of equation (2) yield estimates of δ_{Tight} that are comparable to those obtained from regressions in which the regressor sets of (1) and (2) are the same. For this reason, in what follows, we only report regression results in which the two equations have the same set of regressors.

The flows regression (equation (1)) implies a zero probability of zero flows. However, actual flows in some quarter-route pairs *are* zero, partly due to the discreteness embodied in sailings. One of these routes never has positive flows and a second has positive flows in only 3 quarters; given our use of route fixed effects, no useful information can be extracted from them. Two other routes have positive flows in 24 and 42 quarters, respectively, and can provide useful information. Therefore, we estimated the above models for three different sets of data: one dataset consists of the six markets for which flows are positive in every quarter; the second consists of the full dataset; the third consists of the six markets for which flows are positive in every quarter plus the Rhine/Russia to Canada route for which flows are positive in 42 out of 52 quarters (see Table 2). Observations with zero flows are dropped from the regression since the log of zero is not defined (standard censored regression models are incompatible with our log-linear regression framework). We focus most of our discussion on the last of the three datasets. The results are not sensitive to the sample used.

Finally, all cartel and economic explanatory variables are lagged by two quarters. There are

two reasons for this. First, passenger data are for arrival in the US. With trips from most origin cities taking two months (including ground transport) and tickets being often purchased in advance or with installments, a change in the cartel status or in economic conditions could not possibly affect arrivals within the same quarter. Second, migration decisions are not taken in the spur of the moment, but rather require some planning. Contemporary observers (see Hourwich, 1911) note that even during the economic crisis of 1908, immigration stayed at normal levels for a quarter, with its full impact being felt in the next fiscal year (7 to 8 months later).

5. THE IMPACT OF CARTEL AGREEMENTS ON MIGRATION.

The “benchmark” results of the GLS regressions, which are based on the observations from the seven biggest markets in terms of number of westward voyages, are shown in Table 3. Model 1 focuses on the effect of tight cartels only, while Model 2 corresponds to the specification in equation (1).²⁵ The results confirm that flows are increasing over time, are highest during the spring, and lowest during the winter. Origin and destination labor wages have the expected sign, but origin wages are not significant. US unemployment is negative (as expected) and significant. The 1908 recession dummy is strongly negative significant: it appears that a collapse in labor market conditions indeed has a bigger effect on migration than that predicted by a linear extrapolation from the effect of small fluctuations in labor market conditions.²⁶ In sum, during this period the “pull” of higher wages seems to be a more important determinant of immigration flow than the “push” of lower wages. There are at least four possible reasons for the lack of significance of origin wages. The first is the fact the origin wage is a weighted average of the wages in a broader region, rather than the wage in the country of origin (which is not observed).

The second is the high frequency nature of our data. Given that origin wages are so much lower than destination wages, short run fluctuations are not likely to have a large impact on migration. In contrast, destination wages are likely to be correlated with the ability of immigrants to find jobs and easily integrate economically, and thus could affect migration decisions. A third possibility follows arguments put forth in Hatton and Williamson (2005): since migration is costly, a potential migrant needs a minimum level of funds to finance a trip; an increase in wage of an origin country from a very low level will simultaneously blunt the incentive to migrate and increase the ability to do so, potentially leading to a zero net effect on migration flows. A fourth possibility is that would-be migrants are more likely to be working for wages upon arrival than they would be in their home countries (where they may have been working on a farm). Thus, wages are more relevant for the destination than the origin country. The effect of a tight cartel operation on passenger flows relative to the loose cartels or no cartels (Model 1) is statistically significant and approximately equal to a reduction of 22% ($1 - \exp(-0.247) = 0.219$). Relative to no cartels (Model 2), the effect of a tight cartel is statistically significant and approximately equal to a reduction of 28%, while a loose cartel results in a flow reduction of 13%.

The variability, $\log(\sigma)$, regressions show that flow variance is higher during adverse economic conditions in North America (all three related variables have positive and statistically significant coefficients) and for routes towards Canada. However, the cartels have no bearing on flow variance (except surprisingly for the loose cartels). Thus, there appears to be no flow stabilization effect arising from the operation of tight cartels. However, the systematic relationship of flow variance with economic conditions and routes suggests that observations should not be equally weighted, and that GLS is the appropriate estimation approach for the flow

regressions. Nonetheless, the OLS estimates of the flow regressions, also reported in Table 3, are similar but generally with higher standard errors. US unemployment is now significant only at the 10% level, and the estimated cartel effects lower (essentially zero for the loose cartels).²⁷

Table 4 shows some of the results of variants of the base specifications. Models 3 and 4 replace wages by GDP as a measure of economic activity, and unemployment by the US employment index. The estimated cartel effects are actually stronger than those in Models 1 and 2. Positive deviations from trend of the US employment index and destination GDP are associated with increased flows, while origin GDP has no discernible effect. In Model 5, we estimate the effect of a tight cartel for each region of origin. The cartel effect is strongest for routes originating in Scandinavia (the omitted region) and weakest for Mediterranean routes, for which it is not statistically significant (it is statistically significant for all other routes, but only at the 10% level for the Rhine and Northern France). The counterparts of all of the above regressions when using all 9 routes, rather than the 7 routes with complete or almost complete series, are essentially identical to those reported here.

A possible concern is that the negative coefficient on effective cartels is not causal but rather it picks up the effect of unobserved negative shocks to demand for passage to North America. In other words, the cartel indicator variable may be endogenous, with firms colluding when demand for their product falls in an attempt to shore up their finances, and engaging in price wars when demand is strong. Rotemberg and Saloner (1986) provide a theoretical model that is consistent with this possibility.²⁸ Even though we control for economic conditions by including employment, wages, and the 1908 dummy as regressors, unobserved factors that reduce demand to North American might be important enough to result in a negative bias on the cartel indicator

variable. We investigated this possibility by (i) running a regression with year fixed effects, (ii) examining the written record on cartel formation and (iii) by using evidence from reverse/eastward flows. The year fixed effects capture variation in economic conditions and any other factors that influence demand for travel to North America (given that economic data is reported at the annual level, their inclusion in these regressions is not meaningful and they are omitted). The inclusion of year fixed effects increases the standard error of the cartel coefficients (see Model 6; similar results are obtained if the Loose cartel dummy is dropped). This is not surprising given the temporal pattern of the cartel agreements. The point estimates for the tight cartel are reduced by about a third, but that for the simple price agreements goes up. All cartel effects remain statistically significant.

Let us now present the narrative evidence. Factors that are unrelated to economic characteristics have often been cited as catalysts for cartel formation and dissolution. For example, a German executive who was at the forefront of negotiations to form shipping conferences, attributed the difficulties in establishing cartels in France and Italy to political considerations (Murken, 1922, pp. 52, 385-98). The literature places primary blame for the breakdown of cooperation among continental and British lines in late 1903 on two main factors (1) the disruptive impact of the International Mercantile Marine merger on pre-existing quota arrangements; and (2) the efforts of Cunard to gain a foothold on the emigrant traffic from central Europe which led to a proliferation of rate wars, as rival firms retaliated in numerous other markets due to the extensive multi-market contact among firms (Murken, 1922, Keeling, 1999a, Brief for the United States, 1915). Neither of these factors can be traced to fluctuations in demand.

Finally, we examine the cartel impact on the (much smaller) eastward flows. If the estimated cartel effects on the westward flows were due to some unaccounted for correlation with demand for travel towards North America, then the cartel effects would have been absent when we consider the eastward flows (unless unobserved demand for travel to North America is strongly correlated with unobserved demand for travel to Europe). The results of the Eastward versions of Models 1 and 2 (Models 1E and 2E of Table 5) show that cartel operation reduced Eastward flows substantially.²⁹

6. DISCUSSION AND FURTHER RESULTS.

6.1 Discussion of Prices and Price Elasticities

The estimated effects of cartel operation on westward flows are very substantial. Is it really possible that industry structure had such a strong impact? In this section we present evidence that shows that our results are indeed plausible. Let us first examine the effect that cartel operation had on prices. Even though there does not exist a complete price series, there is anecdotal evidence that cartel operation increased prices considerably. Keeling (1999b) calculated the effective prices of Cunard Lines between London and New York or Boston using information on revenues and number of passengers from the Cunard archives. The average price for a third-class westward trip during the years of effective cartel operation is 32% higher than the price during the years of ineffective cartel operation.³⁰ The effect of cartels on the fares from Antwerp to North America was even more dramatic: prices dropped by 70% between 1901-1903 and the price war of 1904. A few quotes that we have obtained from the Havre to New York route and the Italy to New York routes, suggest that cartels raised prices by 20% and 50%, respectively.

These figures, if representative of other lines and routes, indicate that an elasticity of demand of only 0.7 would be sufficient to generate the 20%-25% reduction in travel that we estimate.³¹

How reasonable is then a demand elasticity of 0.7? According to Hatton and Williamson (1998), the average income for a foreign born male in the U.S. at the end of our sample period (1911) was \$455 (for a family it was \$843). European incomes ranged from a third to half of those amounts. With third class rates at that time at about \$35 per person (\$140 for a four member family) a North European family would have to spend a third of its annual income to purchase its passage to North America; a South European family would have to spend half its annual income for such a trip. These figures do not include the forgone income from ceasing work for a substantial amount of time. With families of that era not being typically able to save a substantial portion of their earnings, a trip across the Atlantic represented a substantial expenditure. It is not surprising, then, that an approximately 32% increase in fares leads to a 20% decline in volume. True, the gains from migrating to North America, in terms of additional income, greatly exceed the cost of passage, but European families of that era may not have been able to obtain ready financing for their trip against their future income.³² The plausibility of an elasticity of -0.7 is also corroborated by a comparison of the effect of US wage increases on passenger flows with the effect of cartel operation on these flows. An increase in the present discounted value of lifetime income at current wages by one dollar increases $\log(Flow_{it})$ by 0.0010.³³ An increase in freight costs by one dollar reduces $\log(Flow_{it})$ by 0.0055. [Cartel operation increased the cost of passage from the UK for a family of 4 by \$45.] This 5-to-1 difference is reasonable because changes in current wages (from trend) are expected to be partially reversed in later years, because liquidity constraints are likely to cause a dollar increase

in costs today to have a bigger effect than a dollar reduction in the present discounted value of future income, and because immigrants' wages are likely to be lower than the US average. Therefore, the anecdotal evidence and "back-of-the-envelope" calculations suggest that the estimated effects of cartel operation on trans-Atlantic flows of that era are reasonable.³⁴

6.2 Impact on Migration Quality

A reduction in migration flows due to higher fares is not likely to impact all types of immigrants equally. Rather, immigrants with lower income (and possibly skills and educational level) should be disproportionately affected. In fact, there is direct documentary evidence that the firms understood that the higher rates not only discouraged passengers from traveling but that they also had an impact on the composition of those who did travel. For instance, Sidney Lister, a passenger manager for the Cunard Line, stated in his testimony (U.S. v. Hamburg-American Co., et al, pp. 1557-62) that higher rates discourage lower income ("undesirable") potential British migrants from traveling to the United States. One can only expect that the impact on lower income continental migrants would have been even greater. Conversely, price wars provided the opportunity for lower income strata to immigrate to the United States. Referring to the effect of the 1904 price war, the U.S. Consul in Bremen reported that "The competition between steamship lines is very pronounced. It is believed that each competing company will use every means to secure the largest possible share of the traffic, and it is also supposed that there is good reason to believe that European communities make the most of the opportunity of getting rid of their undesirable population."³⁵ Evidence of a decline in immigrant quality also abounded in the press. The front page of the June 14, 1904, *New York Times* reports that the quality of immigrants deteriorated to such an extent that the proportion of those detained on arrival at Ellis

Island tripled from 13% to over 32%. The Cunard Line's general agent in New York is quoted as stating that “The rate is sufficiently low to attract the scum of Europe, and it is very evident that a large proportion of those who take advantage of it will have to be deported.” Thus, there is evidence that the perception of contemporaneous observers was that price wars led to a reduction in the quality of immigrants while cartel operations led to an improvement in the quality of the immigrants.³⁶

6.3 Relation of Passenger Flows and Immigration Records

What is the relationship between the passenger flows included in our dataset and the official U.S. immigration records? Formally, our analysis shows that cartel operation reduced passenger flows to North America from Europe; thus, the inference that it reduced immigration flows is indirect. The use of passenger data is necessitated by the fact that cartels operate at the route level (rather than at the nationality of passengers level) and published U.S. immigration data indicate the country of origin of an immigrant (rather than the route he/she took). Furthermore, the available Immigration and Naturalization Service data are of annual frequency, with fiscal years ending June 30. But could it be that the link from passenger to immigration flows (for that period) is weak? To verify that the effects of cartel operation are reflected in official immigration statistics, we obtained annual immigration data from Ferenczi and Willcox (1929), allocated countries of origin to routes using the same partition we utilized to calculate regional wages and GDP series, and converted all other series to annual frequency by using their average value over the fiscal year. We then run the annualized versions of Models 1 and 2 using the immigration data, omitting the season dummies (which cannot be identified from data of annual frequency) and the Canadian route dummies (since we use the US immigration series). The results, reported

on Table 5 (Models 1M and 2M) show that the cartel effects are of the same magnitude as those using the passenger data series. Other parameters also have the same sign and significance, except that US unemployment and the Year 1908 dummy have weaker effects on migration (the latter almost surely because of the June 30 fiscal year end).

6.4 Effect on Cumulative Migration: Inter-temporal Substitution in Migration Travel

Did cartel operation merely redistribute migration flows from the periods of cartel operation to the periods of cartel non-operation? If cartels were not to have operated, would cumulative migration to North America have been substantially higher? To address the first question, we investigate whether there is any evidence of (short-run) inter-temporal substitution in travel, i.e., any evidence that high prices in any particular year “pushed” migration forward to years at which prices were lower. To determine the size of such an effect, we examine whether there is a surge in flows (over and above the non-cartel steady state) one or two years after the collapse of a cartel agreement. This surge would arise from people who would not have traveled at the high prices, but who would wait and travel at the low non-collusive prices, in a similar way that a firm’s sales in a period of temporary low prices are higher than they would have been if prices were permanently lower. Similarly, one would check whether there is a decrease in flows (relative to the cartel steady state) during the first or second years after the formation of a cartel agreement. The idea here is that as the cartel persists, people would have been able to eventually accumulate savings and travel to North America. Therefore, the short-run impact of the cartel would be stronger than the long run-impact of the cartel.

To estimate the existence of inter-temporal substitution we add to the regression model four dummy variables, $EndYear1_{it}$, $EndYear2_{it}$, $StartYear1_{it}$, and $StartYear2_{it}$. The first variable

takes the value of 1 for the four quarters following the demise of a tight cartel in route i ; otherwise it takes the value of 0. The second variable takes the value of 1 for the fifth through eighth quarter following the demise of tight cartel in route i takes the value of 0 otherwise (neither of these variables take the value of 1 when a cartel is under operation because tight cartel breakdowns last for at least two years). Similarly, the remaining two variables take the value of 1 for the first 4 (or 5 to 8) quarters of a tight cartel's operation. Otherwise, they take the value of 0 (these variables are not defined for cartel spells that are left censored). On the basis of the discussion in the preceding paragraph, we would expect (if there was inter-temporal substitution in travel) the coefficients of $EndYear1_{it}$ and $EndYear2_{it}$ to be positive (and decreasing), and the coefficients of $StartYear1_{it}$ and $StartYear2_{it}$ to be negative (and decreasing in absolute value). Re-estimating Model 1 with the addition of these variables shows that the effect of the cartel coefficient is unaffected and the "Start/End" parameters not statistically significant, and often of the "wrong" sign (see Table 5, Model 7).³⁷ These findings suggest no measurable short-run inter-temporal substitution.

What about long-run inter-temporal substitution? If there were no cartels, would the total sum of migration to the U.S. be different? This question we cannot answer. Stronger migration flows in the pre-war period might have triggered earlier restrictions on migration, leaving total cumulative migration unchanged. In other words, there are many other endogenous policy responses that affect migration which might differ had cartels never operated. What we can say, though, is that by the time of the Great War there were noticeably fewer European migrants in North America than there would have been in the absence of the North Atlantic cartel agreements.

7. CONCLUDING REMARKS.

In this study we assemble a unique data-base of passenger flows to North America from European ports at the turn of the century, and combine it with detailed information about the operation of shipping cartels to investigate the impact of effective collusion among liner firms on trans-Atlantic migration. We find that the operation of shipping cartels led to a substantially lower level of migration, as neoclassical models of collusion predict, but did not, as the cartel members have argued at the time, stabilize the migration flow. These results challenge the widely held belief that immigration flows to North America from Europe were determined primarily only by “macro” factors, such as economic conditions, social upheaval, and technical change in the transportation and manufacturing industries. Furthermore, the cartels reduced the pre-war flows of Western and Northern Europeans, whose place was taken by post-war flows of Southern and Eastern Europeans and non-Europeans. Immigration to the U.S. was severely restricted beginning in the 1920s until the 1970s, and recent immigration consists primarily of flows from Latin America and East Asia rather than from Europe. It would appear, then, that the cartel operation did not merely “postpone” the growth in the U.S. population, but possibly affected its demographic characteristics.

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Footnotes:

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2. See Easterlin (1961), Hatton and Williamson (1998), chapters 3 and 4, Hatton (1995), Jerome (1926), Thomas (1973), and Wilkinson (1970). Important surveys and discussion are contained in Baines (1994), Chiswick and Hatton (2003) and Gould (1979).

3. For the discussion of transport costs on the trans-Atlantic migration, see Baines (1994), Cohn (2005), Gould (1979, pp. 613-615), Hatton and Williamson (1998, p. 14), Hvidt (1971), Keeling

(1999a), O'Rourke and Williamson (1999, p. 134) and Thomas (1973).

4. Theoretical models consistent with this prevention of "destructive competition" view of cartels are given in Sjoström (1989) and Pirrong (1992).

5. Multiple contemporary accounts claim that the operation of the cartels did not affect all immigrants equally. Rather, cartel operation acted as a positive selection on immigrants, with higher prices disproportionately reducing the flow of lower income (lower "quality") immigrants. For a long term view of positive and negative selection on immigration and associated policy issues see Hatton and Williamson (2004).

6. A more general discussion of the origins of both freight and passenger steamship cartels is available in the secondary literature (Gottheil, 1914, Bowen, 1930, Deakin, 1973, Hyde, 1975). The transport requirements of large numbers of European migrants contributed to a substantial expansion of merchant shipping in the late 19th and early 20th centuries. In the 1840s, some trans-Atlantic passenger shipping companies began to convert from sail to steam, and by end of the 1860s nearly all passenger liners had adopted steam technology. The transition to steamships implied a much higher level of fixed costs for the carriers, and this in turn made the firms especially vulnerable to periods when the immigrant tide ebbed. As one potential solution, the firms began to collude. Firms also colluded on freight traffic, but these cartels were separate entities with distinct histories. See Deltas, Serfes and Sicotte (1999) and Murken (1922), chapter 7.

7. From the secondary literature, especially valuable were Aldcroft (1968), Cecil (1967), Hyde (1975), and Keeling (1999a).

8. Ellison (1994), Genesove and Mullin (2001), Gottheil (1914), Sickel (1914), Scott-Morton (1997), and Stevens (1914).

9. U.S. v. Hamburg-American Co., et al., Testimony of Hermann Winter, p. 1171. Murken (1922),

pp. 64-72.

10. U.S. v. Hamburg-American Co., et al., testimony of Arthur Cauty (International Mercantile Marine), pp. 1092-93, testimony of Hermann Winter, pp. 1175-1183, Brief for the United States (1915), U.S. House of Representatives Committee on Merchant Marine and Fisheries (1914), Hyde (1975), and Murken (1922), chapters 10-12. A detailed description of our categorization of cartels is available in our working paper.

11. Evidence shows that these lines virtually monopolized passenger traffic on the North Atlantic (see Murken, 1922, chapter 7, and Brief of the United States, 1914, pages 22-29. For a description of the cartels' exclusionary practices see the decision of the District Court (S.D. New York) United States v. Hamburg-American S.S. Line et al., 216 Fed. 971 (1914).

12. U.S. v. Hamburg-American S.S. Line et al., 216 Fed. 971 (1914). The court did render a small verdict for the government by prohibiting the use of so-called "fighting ships," the predatory pricing technique utilized by the colluding firms against potential entrants.

13. U.S. v. Hamburg-American Line, et al. 239 US 466 (1916).

14. These data are found in Petitioner's exhibits 154-158 (1906-1911) and 209-215 (1899-1905) in the above cited anti-trust case. The data were subpoenaed from the office of the Conferences in New York. The evidence included data for both conference and non conference lines.

15. "Britain" includes voyages indicated as originating from Bristol, Britain, Glasgow, Hull, Liverpool, London, Manchester, Southampton, or combinations thereof. "Mediterranean" encompasses all Mediterranean ports in Europe, including those of Southern France and Portugal (voyages indicated as originating from Adriatic, Algeria, Azores, Black Sea, Greece, Italy, Levante, Lisbon, Marseilles, Mediterranean and Continental Mediterranean, Naples, Palermo, Spain). "North

France” includes voyages originating from Bordeaux, Cherbourg, Dunkirk, and Havre and combinations thereof. “Rhine/Russia,” often referred to simply as “Rhine,” includes all German, Dutch, Belgian and Russian ports (voyages indicated as originating from Amsterdam, Antwerp, Bremen, Hamburg, Libau, Rotterdam, or combinations thereof). “Scandinavia” includes voyages indicated as originating from Christiania or from Scandinavian ports.

16. In previous versions of the paper, we distinguished seven European and three North American regions (Austria, Russia, and the US Gulf were separated out from the Mediterranean, the Rhine/Russia, and the US, respectively). None of the conclusions of the paper are sensitive to this finer partition, but the number of zero flow routes is much smaller in the current coarser partition.

17. See, for instance, Hatton and Williamson (1998) chapter 3, O’Rourke and Williamson (1999) chapter 7, and references therein. The economic analysis of contemporary immigration also confirms the sensitivity of migration flows to labor market variables (see Borjas, 1999).

18. Ireland was not yet an independent country, but Williamson (1995) provides a separate series for it.

19. Some regions consist of more than one country. For these regions, we used a weighted average of the wage series of the constituent countries. The weights correspond to the average migration flows from these countries to the US during our sample period. In results that we do not report, we have used an alternative set of weights based on our assessment of the potential for immigration. These are: (i) for the UK, Britain 0.8 and Ireland 0.2, (ii) for the Mediterranean, Italy 0.7, Portugal 0.1, and Spain 0.2, (iii) for the Rhine, Germany 0.6, the Netherlands 0.2, and Belgium 0.2, and (iv) for Scandinavia, Sweden 0.5 and Norway 0.5. Relative to immigration weights, this alternative set over-weighs Portugal, Spain, and Britain to reflect their relatively high population weights, and

Netherlands and Belgium to “diversify” the Rhine series. Our results are not sensitive to the choice of weights.

20. We aggregated the country GDP series to region GDP series using immigration weights. In results not reported here, we also used an alternative set of weights. In this alternative set, the Mediterranean consists of the Italian series, the series for the Rhine consists of 0.8 times the GDP of Germany plus 0.2 times the GDP of Belgium, while the series for Scandinavia consists of 0.5 times the GDP of Sweden plus 0.5 times the GDP of Norway. We have no GDP data for Russia, while the UK series is not decomposed to a series for Britain and a series for Ireland. Our results are not sensitive to the choice of weights.

21. In the time period and routes that we study, the only changes in American immigration policy were the 1907 increase in the American head tax to \$4 from \$2, and the establishment of a financial test - \$25 per individual and \$50 per family (Timmer and Williamson, 1998). Canada enacted an Immigration Act in 1910 which set out more specific terms for admission (Green, 1995).

22. A fully structural model would require modeling and solving the cartel game in which the liner firms were engaged. The usefulness of such an exercise would be questionable (even if detailed price series was available) given the strenuous assumptions that would be have to imposed to account for the absence of detailed firm-level capacity and cost data.

23. Results are essentially identical if the log flows are also de-trended (and adjusted for differences in the mean route flows) prior to the regression, except, of course, for the coefficients on the trend and route fixed effects which become essentially zero (as expected).

24. σ_i is estimated by the absolute value of the residual from equation (1). All equations are estimated by GLS, though for completeness we also report and discuss OLS results for the base

specifications.

25. The effect of cartels is stronger if one omits the economic variables.

26. The results are not sensitive to “shifting” the 1908 dummy variable backwards into the last quarter of 1907 or forward into the first quarter of 1909.

27. This pattern of GLS versus OLS estimates is present for most other specifications reported in this paper.

28. In contrast, Abreu, Pearce and Stacchetti (1986) and Green and Porter (1984) provide theoretical models that predict the opposite, i.e., that price wars and cartel breakdown will take place in periods of low demand. The most crucial difference between these two models and Rotemberg and Saloner (1986) is that the latter assumes the state of demand is observable to all parties, while the former models assume that cartel participants can observe the residual demand for their products, but not the actions of their competitors or the state of the market demand. Ellison (1994) and Levenstein (1997) provide empirical evidence in support of Abreu, Pearce and Stacchetti (1986) and Green and Porter (1984).

29. The sign of the economic regressors was often reversed, with weak employment markets and economic growth in the U.S. increasing the eastward flow of passengers. Overall, the expected effect of economic conditions on eastward flows is less clear than their effect on westward flows. A strong U.S. labor market may reduce the number of people returning economic reasons, but would increase the number of people who would return temporarily for non-economic reasons (and its effect on repatriating retirees is indeterminate).

30. Incidentally, eastbound rates are somewhat *higher* than westbound rates, despite the fact that ships had excess capacity during the eastward flows. This price discrimination, driven by the fact that U.S.

residents had higher income than European residents, could only be sustained if shipping firms did, in fact, have market power as we maintain here.

31. This elasticity corresponds to price changes in the Liverpool to New York route, for which our price series is most detailed. A demand elasticity of 0.7 is not necessarily inconsistent with a cartel equilibrium. The cartel agreement did most likely not perfectly replicate monopoly behavior (perhaps due to incentive compatibility and organizational constraints). Further, the estimate of 0.7 reflects an arc elasticity over a discrete price change; the point elasticity at the equilibrium prices could actually be greater than 1.

32. However, some families may have been able to secure funds for their passage from other members of their family already in North America (O'Rourke and Williamson, p. 131).

33. The coefficient on Destination Wage has been converted to present discounted income using a discount rate of 15%, assuming that it takes one year from when passage is paid to when work in North America commences, and observing that one point of the US wage index (the regressor) corresponds to \$5 of family income at 1910 wages. A parameter estimate of 0.27 for the US wage index is used, as obtained from Model 1 *excluding* the dummy for 1908 to ensure that the Destination Wage coefficient also reflects the effect of the 1908 recession (sign and significance of other variables not affected; complete results not reported for brevity).

34. A recent counterpart to the cartel induced price increases in terms of raising the cost of (illegal) migration is the level of resources channeled into border enforcement. In principle, one could with the appropriate data infer the elasticity of illegal migration to such "price increases" (see Hanson and Spilimbergo, 1999, Hanson, Robertson, and Spilimbergo, 2002, for some discussion based on indirect evidence).

35. See the US Monthly Consular Reports, April 1905, no. 295, 58th Congress, 3rd Session, House Document 331. “Emigration to the United States via Bremen in 1904.”

36. There was a secular shift in migration towards countries of lower “quality” immigrants to which cartel operation did not contribute. Indeed, cartel operation had the weakest effect for the Mediterranean region (see Model 5). In part, this is because firms price discriminated with respect to country of origin, and thus, the effect of cartel operation on immigrant quality was primarily on the within-country immigrant quality.

37. This pattern of results provides additional justification for lagging the entire set of explanatory variables by two quarters, as we do. When the series is not lagged in this manner, the “Start/End” variables are significant for the first year and of the wrong sign: the year after a cartel is created “looks” much more like a non-cartel year, and the year after a cartel ends looks more like a cartel year. Likely explanations are the reasons given in section 4 for lagging the explanatory variables.

Table 1. Classification of Cartel Status

Year	Quarter	Port of Origin				
		Great Britain	Mediterranean	North France	Rhine/Russia	Scandinavia
1899	I	Dark	Light	Unshaded	Dark	Dark
	II					
	III					
	IV					
1900	I	Dark	Light	Unshaded	Dark	Dark
	II					
	III					
	IV					
1901	I	Dark	Light	Unshaded	Dark	Dark
	II					
	III					
	IV					
1902	I	Dark	Light	Unshaded	Dark	Dark
	II					
	III					
	IV					
1903	I	Dark	Light	Dark	Dark	Dark
	II					
	III					
	IV					
1904	I	Light	Light	Unshaded	Dark	Dark
	II					
	III					
	IV					
1905	I	Light	Light	Unshaded	Dark	Dark
	II					
	III					
	IV					
1906	I	Light	Light	Unshaded	Dark	Dark
	II					
	III					
	IV					
1907	I	Light	Light	Unshaded	Dark	Dark
	II					
	III					
	IV					
1908	I	Dark	Light	Dark	Dark	Dark
	II					
	III					
	IV					
1909	I	Dark	Light	Dark	Dark	Dark
	II					
	III					
	IV					
1910	I	Dark	Light	Dark	Dark	Dark
	II					
	III					
	IV					
1911	I	Dark	Light	Dark	Dark	Dark
	II					
	III					
	IV					

Notes: Dark shaded areas correspond to the operation of an effective or tight cartel. Light shaded areas correspond to the presence of a simple price agreement or less effective cartel. Unshaded areas correspond to the absence of a cartel or a price war. Sources: see text.

Table 2. Westbound Trips Carrying 3rd Class Passengers.

Port of Origin	Port of Destination			
	Number of Sailings		Number of Quarters with Sailings	
	Canada	United States	Canada	United States
Great Britain	2543	4565	52	52
Mediterranean	5	4085	3	52
North France	128	982	24	52
Rhine/Russia	424	4913	42	52
Scandinavia	0	398	0	52

Sources: U.S. v. Hamburg-American Co., et al. (239 U.S. 466, 1916), Petitioner's Exhibits 154-158, 209-215.

Table 3. Determinants of Westward Steerage Flows - Main Regression Results.

	Mean Regressions: GLS		Log(Sigma) Regressions		Mean Regressions: OLS	
	Model 1	Model 2	Model 1 GLS	Model 2 GLS	Model 1	Model 2
Constant	7.610 <i>0.094</i>	7.677 <i>0.110</i>	-2.271 <i>0.284</i>	-1.825 <i>0.310</i>	7.435 <i>0.109</i>	7.438 <i>0.121</i>
Tight Cartel Agreement	-0.247 <i>0.041</i>	-0.321 <i>0.073</i>	0.139 <i>0.129</i>	-0.270 <i>0.175</i>	-0.226 <i>0.050</i>	-0.229 <i>0.068</i>
Loose Cartel Agreement		-0.134 <i>0.070</i>		-0.624 <i>0.185</i>		-0.005 <i>0.072</i>
Quarterly Trend	0.016 <i>0.001</i>	0.017 <i>0.001</i>	-0.006 <i>0.004</i>	-0.005 <i>0.004</i>	0.019 <i>0.002</i>	0.019 <i>0.002</i>
Year 1908	-0.861 <i>0.124</i>	-0.816 <i>0.131</i>	0.797 <i>0.221</i>	0.525 <i>0.232</i>	-0.784 <i>0.085</i>	-0.786 <i>0.091</i>
Spring	0.733 <i>0.053</i>	0.773 <i>0.050</i>	0.029 <i>0.156</i>	0.041 <i>0.154</i>	0.781 <i>0.060</i>	0.781 <i>0.060</i>
Summer	0.239 <i>0.050</i>	0.256 <i>0.047</i>	-0.057 <i>0.153</i>	-0.012 <i>0.151</i>	0.232 <i>0.059</i>	0.232 <i>0.059</i>
Fall	0.131 <i>0.047</i>	0.153 <i>0.044</i>	-0.204 <i>0.152</i>	-0.162 <i>0.151</i>	0.130 <i>0.059</i>	0.130 <i>0.059</i>
US Unemployment	-0.046 <i>0.018</i>	-0.058 <i>0.017</i>	0.146 <i>0.051</i>	0.132 <i>0.051</i>	-0.033 <i>0.020</i>	-0.033 <i>0.020</i>
Destination Wages	0.021 <i>0.007</i>	0.020 <i>0.007</i>	0.041 <i>0.019</i>	0.031 <i>0.194</i>	0.015 <i>0.007</i>	0.014 <i>0.008</i>
Origin Wages	-0.008 <i>0.009</i>	-0.006 <i>0.009</i>	-0.041 <i>0.032</i>	-0.019 <i>0.032</i>	-0.002 <i>0.012</i>	-0.002 <i>0.013</i>
Britain-USA	2.586 <i>0.057</i>	2.606 <i>0.053</i>	-0.270 <i>0.196</i>	-0.252 <i>0.193</i>	2.608 <i>0.075</i>	2.608 <i>0.075</i>
Mediterranean-USA	2.764 <i>0.070</i>	2.670 <i>0.070</i>	0.154 <i>0.205</i>	-0.039 <i>0.210</i>	2.769 <i>0.079</i>	2.767 <i>0.082</i>
Northern France-USA	1.307 <i>0.062</i>	1.372 <i>0.058</i>	-0.040 <i>0.201</i>	0.020 <i>0.200</i>	1.291 <i>0.077</i>	1.291 <i>0.078</i>
Rhine-USA	3.248 <i>0.061</i>	3.278 <i>0.056</i>	-0.098 <i>0.196</i>	-0.089 <i>0.193</i>	3.250 <i>0.075</i>	3.260 <i>0.075</i>
Britain-Canada	1.618 <i>0.118</i>	1.614 <i>0.110</i>	0.753 <i>0.335</i>	0.677 <i>0.331</i>	1.679 <i>0.129</i>	1.678 <i>0.129</i>
Rhine-Canada	-0.039 <i>0.145</i>	-0.016 <i>0.135</i>	1.373 <i>0.343</i>	1.315 <i>0.339</i>	0.023 <i>0.132</i>	0.022 <i>0.132</i>
R-squared	0.9367	0.9449	0.1198	0.1503	0.9178	0.9178

Notes: (i) The omitted route dummy variable is Scandinavia-USA. (ii) The number of observations is 331 for all specifications. (iii) Destination and origin wages are deviations from trend. (iv) US Unemployment applies to US observations only.

Table 4. Determinants of Steerage Flows - Robustness Checks and Analysis of Eastward Flows.

	Model 3	Model 4	Model 5	Model 6	Model 1E	Model 2E
Constant	7.342 <i>0.078</i>	7.405 <i>0.094</i>	7.689 <i>0.098</i>	7.037 <i>0.141</i>	5.908 <i>0.148</i>	6.048 <i>0.178</i>
Tight Cartel Agreement	-0.315 <i>0.040</i>	-0.425 <i>0.067</i>	-0.462 <i>0.093</i>	-0.240 <i>0.078</i>	-0.198 <i>0.067</i>	-0.372 <i>0.092</i>
Loose Agreement		-0.160 <i>0.066</i>		-0.192 <i>0.061</i>		-0.289 <i>0.120</i>
Tight Cartel * Britain			0.142 <i>0.105</i>			
Tight Cartel * Mediterranean			0.335 <i>0.153</i>			
Tight Cartel * Rhine			0.323 <i>0.118</i>			
Tight Cartel * Northern France			0.289 <i>0.138</i>			
Quarterly Trend	0.019 <i>0.001</i>	0.020 <i>0.001</i>	0.014 <i>0.002</i>		0.029 <i>0.002</i>	0.031 <i>0.002</i>
Year 1908	-0.764 <i>0.134</i>	-0.729 <i>0.139</i>	-0.845 <i>0.133</i>		0.435 <i>0.116</i>	0.379 <i>0.133</i>
Spring	0.717 <i>0.059</i>	0.743 <i>0.057</i>	0.748 <i>0.055</i>	0.789 <i>0.043</i>	0.975 <i>0.093</i>	1.003 <i>0.096</i>
Summer	0.219 <i>0.056</i>	0.217 <i>0.055</i>	0.279 <i>0.049</i>	0.294 <i>0.047</i>	0.842 <i>0.080</i>	0.888 <i>0.084</i>
Fall	0.095 <i>0.053</i>	0.105 <i>0.051</i>	0.162 <i>0.048</i>	0.208 <i>0.043</i>	0.838 <i>0.083</i>	0.899 <i>0.835</i>
US Unemployment			-0.031 <i>0.017</i>		-0.038 <i>0.026</i>	-0.053 <i>0.023</i>
Destination Wages			0.017 <i>0.007</i>		-0.050 <i>0.018</i>	-0.053 <i>0.019</i>
Origin Wages			-0.001 <i>0.010</i>		0.022 <i>0.011</i>	0.018 <i>0.011</i>
US Employment Index	0.020 <i>0.011</i>	0.021 <i>0.011</i>				
Destination GDP	0.025 <i>0.007</i>	0.028 <i>0.007</i>				
Origin GDP	-0.018 <i>0.011</i>	-0.009 <i>0.011</i>				
Britain-USA	2.632 <i>0.066</i>	2.641 <i>0.065</i>	2.535 <i>0.059</i>	2.606 <i>0.052</i>	2.561 <i>0.074</i>	2.590 <i>0.076</i>
Mediterranean-USA	2.754 <i>0.073</i>	2.639 <i>0.072</i>	2.594 <i>0.081</i>	2.806 <i>0.061</i>	2.857 <i>0.115</i>	2.774 <i>0.114</i>
Northern France-USA	1.285 <i>0.069</i>	1.344 <i>0.067</i>	1.165 <i>0.066</i>	1.394 <i>0.054</i>	-0.203 <i>0.441</i>	-0.438 <i>0.454</i>
Rhine-USA	3.268 <i>0.072</i>	3.293 <i>0.070</i>	3.082 <i>0.068</i>	3.272 <i>0.056</i>	2.721 <i>0.075</i>	2.726 <i>0.078</i>
Britain-Canada	1.811 <i>0.069</i>	1.844 <i>0.067</i>	1.585 <i>0.121</i>	1.785 <i>0.049</i>	0.917 <i>0.168</i>	0.851 <i>0.166</i>
Rhine-Canada	0.128 <i>0.109</i>	0.183 <i>0.106</i>	-0.180 <i>0.144</i>	0.155 <i>0.131</i>		
R-squared	0.9264	0.9303	0.9436	0.9553	0.9022	0.9051

Notes: (i) The omitted route is Scandinavia-USA. (ii) The number of observations is 331 for models 3-6, 272 for models 1E-2E. (iii) The year fixed effect estimates (standard errors) for model 6 are: 1900, 0.206 (0.121); 1901, 0.271 (0.129); 1902, 0.682 (0.121); 1903, 0.840 (0.132); 1904, 0.585 (0.143); 1905, 0.865 (0.123); 1906, 1.120 (0.124); 1907, 1.235 (0.121); 1908, 0.148 (0.141); 1909, 0.966 (0.130); 1910, 1.139 (0.130); 1911, 0.871 (0.134). (iv) Wages, US Employment Index, and GDP are deviations from trend. (v) US unemployment applies to US observations only. (vi) For Eastward flows, "destination" refers to European regions. (vii) All results are based on GLS regressions.

Table 5. Investigation of Possible Delayed Effects and Analysis Using Official Immigration Statistics

	Official Immigration Statistics		Delayed Effects
	Model 1M	Model 2M	Model 7
Constant	10.700 <i>0.151</i>	10.948 <i>0.158</i>	7.606 <i>0.094</i>
Tight Cartel Agreement	-0.193 <i>0.048</i>	-0.406 <i>0.065</i>	-0.255 <i>0.054</i>
Loose Agreement		-0.253 <i>0.094</i>	
1st Year after Cartel Ends			0.137 <i>0.127</i>
2nd Year after Cartel Ends			-0.090 <i>0.056</i>
1st Year after Cartel Starts			0.174 <i>0.090</i>
2nd Year after Cartel Starts			0.023 <i>0.080</i>
Trend	0.011 <i>0.001</i>	0.012 <i>0.001</i>	0.016 <i>0.002</i>
Year 1908	-0.450 <i>0.085</i>	-0.563 <i>0.091</i>	-0.825 <i>0.115</i>
Spring			0.729 <i>0.049</i>
Summer			0.234 <i>0.045</i>
Fall			0.112 <i>0.042</i>
US Unemployment	-0.031 <i>0.021</i>	-0.062 <i>0.019</i>	-0.053 <i>0.018</i>
Destination Wages	0.041 <i>0.011</i>	0.027 <i>0.010</i>	0.021 <i>0.007</i>
Origin Wages	-0.020 <i>0.012</i>	-0.002 <i>0.014</i>	-0.015 <i>0.010</i>
Britain-USA	0.557 <i>0.128</i>	0.688 <i>0.120</i>	2.560 <i>0.049</i>
Mediterranean-USA	1.579 <i>0.125</i>	1.534 <i>0.122</i>	2.787 <i>0.069</i>
Northern France-USA	-1.893 <i>0.158</i>	-1.773 <i>0.135</i>	1.332 <i>0.058</i>
Rhine-USA	2.122 <i>0.121</i>	2.201 <i>0.115</i>	3.264 <i>0.052</i>
Britain-Canada			1.609 <i>0.121</i>
Rhine-Canada			0.001 <i>0.143</i>
R-squared	0.9809	0.9838	0.9526
Number of Observations	65	65	331

Notes: (i) Scandinavia-USA is the omitted route. (ii) Wages are deviations from trend. (iii) US unemployment applies to US observations only. (iv) For the immigration data based regressions, Year 1908 is two quarters lagged to conform with the fiscal year. (v) All results are based on GLS regressions.

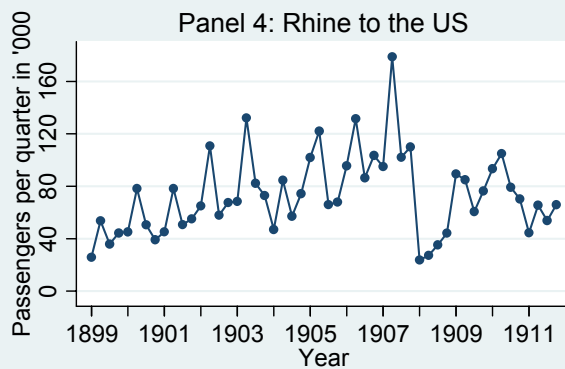
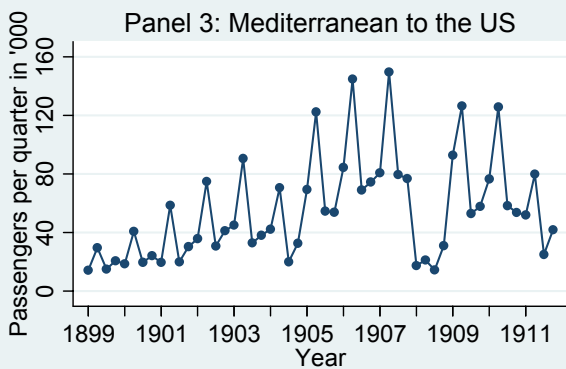
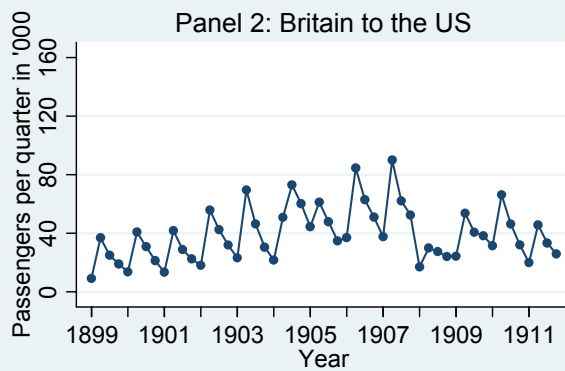
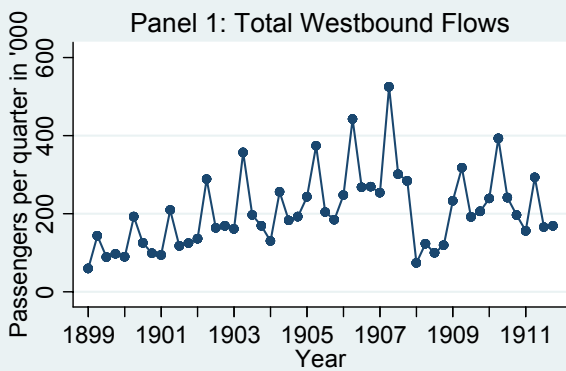


Figure 1: Westward Flows

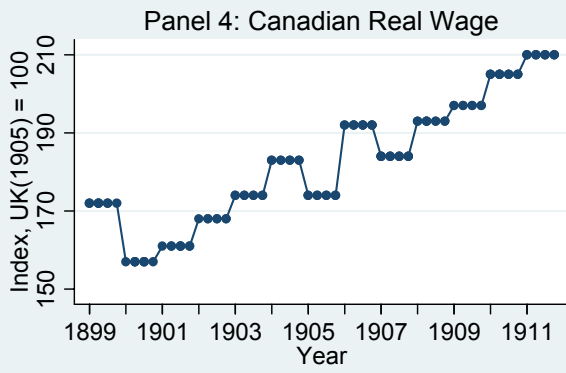
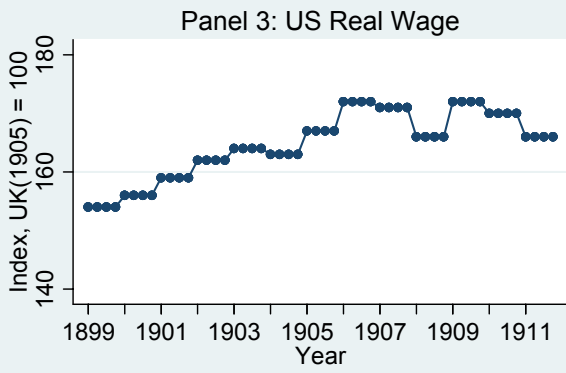
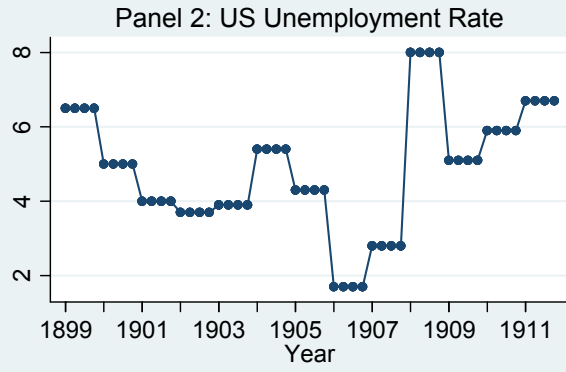
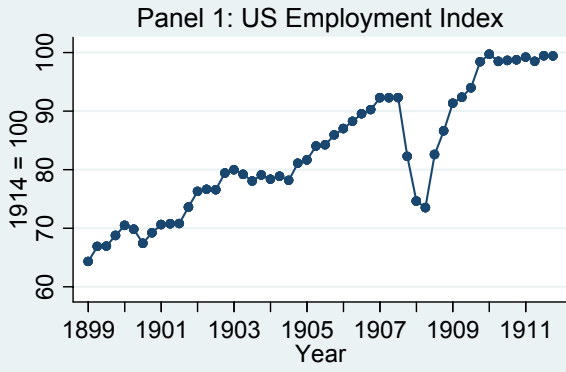


Figure 2: North American Economic Conditions

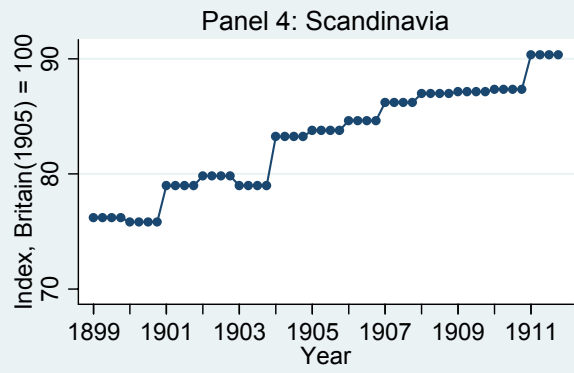
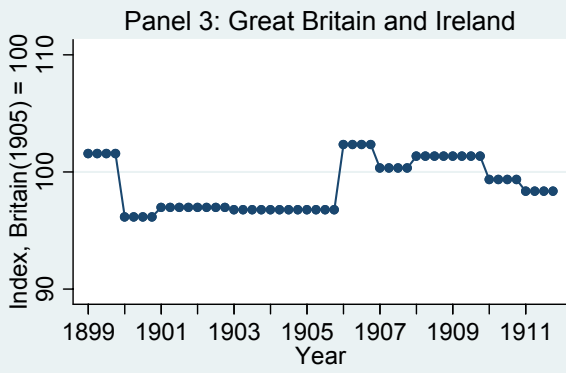
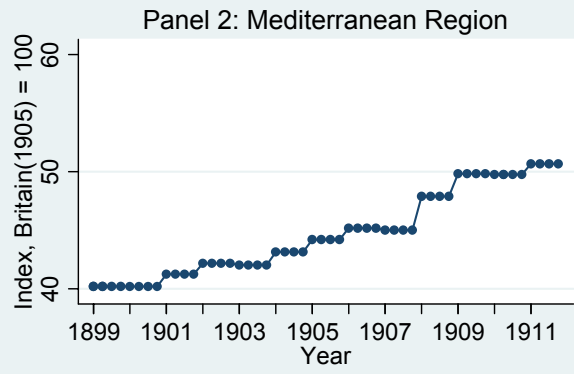
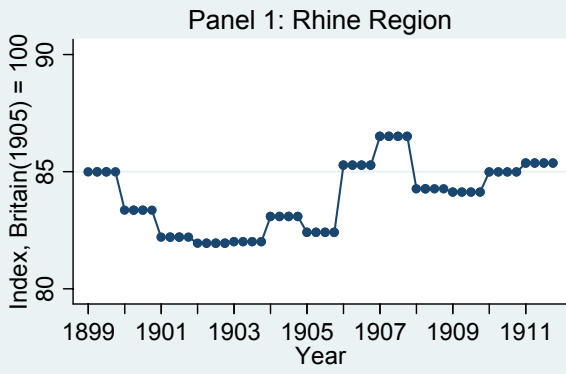


Figure 3: European Immigration-Weighted Real Wage Indexes