European Inter-Continental Emigration 1815-1914: Patterns and Causes

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1) *Sources, concepts, setting.*

International migration was one of the great processes of nineteenth-century history. The sheer magnitude of the movements involved; their quality of human drama; the fact that they were one manifestation of fundamental changes in the international economic scene; and their wide-ranging social and economic consequences, have guaranteed a lively interest from historians of every type. In recent years a number of econometric studies of migration have built upon the foundations of earlier statistical treatments. Non-quantitative works have taken many forms: local studies of emigration from or immigration into a particular district; imaginative, almost poetic, explorations of the psychic dimensions of migration such as those of Oscar Handlin; and the more predictable but often useful works of the 'filiopietists', tracing (and extolling) the contributions of particular national or ethnic groups to their adopted land.

The mere bulk of the literature on international migration would suffice to explain a wide gap between the specialist study

and the textbook summary. But their number is not the only obstacle to the easy acquisition of a mastery of the works of specialists; the inclination to read and ability to appraise a wide variety of styles of historical writing and a reading knowledge of at least several major European languages are also required. As with some other topics of modern history, a gap has opened up, which most have not yet bridged, between econometric and "traditional" studies. Even Philip Taylor's book *The Distant Magnet* (published 1971), in most respects the best and most thoroughly researched general summary of transatlantic migration, does not include in its footnotes or bibliography any reference to the econometric studies published prior to 1971. On the other hand one is often struck, in reading econometric studies, by the extremely limited range of comparative and non-quantitative historical material which seems to have been consulted in preparing them.

The scope of this series of essays is restricted in several directions. In the first place, I confine my attention to inter-continental emigration from Europe - though one of the themes to be developed is that for a proper understanding of this it is necessary to consider it more explicitly in relation to intra-European migration than has commonly been the case. Secondly, I deal only with *patterns* and *causes* of European emigration, leaving on one side or touching only lightly on its consequences, its organisation, and migration policies. And thirdly, the research which has gone into the writing of these essays has overcome the obstacles mentioned above, if at all, only by the practice of a certain eclecticism in regard to the choice of literature consulted, rather than by even an attempt at comprehensiveness. Indeed, I have barely scratched the surface of the existing literature of migration, particularly perhaps in regard to local and biographical studies. I particularly regret the absence from my reading of works in major East European tongues, though in the linguistic dimension these essays are perhaps less seriously deficient

than some other works, being based on reading in seven European languages, with occasional consultations in a further three.

These essays attempt both to make a modest contribution towards a synthesis of the existing literature on European intercontinental emigration, and to present the results of my own investigations into one or two important issues which emerged in preparing that synthesis. The avowedly exploratory character of these studies as they stand and the insuperable difficulty I have experienced in trying to integrate what I have to say into a single cohesive whole explain the presentation of my ideas in a series of "Essays" rather than in a monograph. This introductory essay discusses some aspects of the historical setting of intercontinental migration and says something about the sources. It is followed by three further essays each dealing with a fairly specific theme and each reasonably self-contained. Essay 2 offers a review of the modern statistical and econometric literature of European migration; Essay 3 analyses the character and significance of "repatriation", or return migration, especially from the U.S.A.; and Essay 4 presents some results of my readings in and calculations concerning Italian emigration. In the final essay an attempt is made to suggest a reinterpretation of the nature and causes of transatlantic emigration in the period before World War One, developing the argument in the light of the experience of countries whose patterns of emigration have not as yet been probed by the searchlight of econometric inquiry.

Though each essay is largely self-contained, there is more of a developing argument running right through the series than might at first appear. Readers of limited stamina may perhaps find their interest satiated by reading one or more of the three middle essays, or even perhaps this introductory one. But it would hardly be profitable to tackle Essay 5 unless the reader is game first to undertake a study of all four predecessors.

The textbooks on international economic history convey to their readers an impression of the magnitude of intercontinental migration in such statements as that " ...the emigration statistics ... suggest a total population outflow overseas of some 46 millions during the period 1821-1915 " [1] or that " Between 1820 and 1930 the recorded gross inter-continental migration ... totalled about 62 million ".[2] The migration specialist knows that such global estimates, though perhaps approximately correct, conceal many gaps and gloss over many elements of incomparability in the underlying national data on which, ultimately, they are based.

The proximate source of such statements is usually to be found in the remarkable collection of international migration statistics compiled on behalf of the International Labour Office by Imre Ferenczi, and published in 1929 by the National Bureau of Economic Research under the editorship of Walter Willcox.[3] Though many of the interpretative studies in Volume II have long become dated, the statistical compilation in Volume I has not yet been superseded, and the (often neglected) discussion of the statistics in Part I of this volume still has a great deal to teach us about the interpretation and the significance of the figures. It should be mandatory, for example, for every historian proposing a quantitative study of migration into the U.S.A. to spend a few moments studying the series of diagrams on page 194 which compare migration from each of six specified countries to the U.S.A., as recorded by the sending country on the one hand and the U.S. data on the other.

While the Ferenczi and Willcox volumes fully deserve the place in the literature they have so long retained, the opportunity

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[1] KENWOOD, A.G. and LOUGHEED, A.L., *The Growth of the International Economy 1820-1960.* London and Sydney, George Allen and Unwin and Australasian Publishing Co., 1971, p. 57.

[2] ASHWORTH, W., *A Short History of the International Economy since 1850* (3rd edn.). London, Longman, 1975, p. 196.

[3] Ferenczi and Willcox, 1929.

may be seized to record a scholarly tribute to what is in some ways an even more remarkable contribution to migration studies, namely the rather earlier collection of international migration statistics largely produced by the great Swedish statistician, Gustav Sundbärg.[4] Sundbärg's work is remarkable not just as a compilation of data, but for the richness of the interpretative hypotheses also offered to the reader. That the Swedish collection has been somewhat eclipsed as a data source for students of migration by the later I.L.O./N.B.E.R. collection is not to be explained in terms of the superiority of the latter work so much as in terms of its readier accessibility, its use of the English language, and the good fortune of a date of publication which, unlike that of Sundbärg's compilation, enabled the data to be continued up to 1924, the year in which the second U.S. quota Act effectively ended the freedom of movement which allowed the pre-World War One decades to become the greatest era yet in the history of intercontinental migration. It is pleasing to note that, the merits of the Ferenczi-Willcox study notwithstanding, one or two modern scholars, in quarrying data for econometric work, have in fact preferred to go back to the older Swedish study; and that one econometric historian has treated one of the most intriguing of Sundbärg's hypotheses to the courtesy of an econometric test (and confirmation).[5]

The Ferenczi-Willcox volume brings together, and aggregates and summarises in many ways, data for individual countries then available in published form or supplied by governments at the request of the International Labour Office. However, the speed with which the compilation was put together made substantial editing or rearranging of the basic national data impossible, and the collection as a whole is only as good as these data in their original form. In this connection it must

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[4] Sundbärg, 1910. See also the same, *Betänkande* (1913).

[5] Quigley, 1972, pp. 125-6.

be noted that quite apart from the question of the accuracy with which the data had been originally gathered, were transcribed by Ferenczi's teams of assistants, and were then put into print, there are a great many differences of coverage, scope and definition not merely from one country to another, but from one period to another.

Take by way of illustration the three tables which have perhaps been more frequently used than any in the volume, Tables I, II and III in the U.S. section of the National Tables. These tables give the aggregate numbers of alien arrivals or immigrants into the United States for each year of the period 1820 to 1924, broken down by nationality, country of origin, or country of last residence. At different times within this period the original data were collected by three different departments of the U.S. government: the Department of State (1820 to 1870); the Bureau of Statistics in the Treasury Department (1867 to 1895); and the Bureau of Immigration (from 1912, Naturalization and Immigration) from 1892 onwards. Within that period the the arriving aliens were counted, in successive periods, according to at least five differing conceptual schemes: (i) foreign-born passengers taken on board ship (Department of State series); (ii) the same, but excluding "transient" visitors (Bureau of Statistics series); (iii) all aliens, excluding those travelling cabin class, admitted to U.S.A. (Bureau of Immigration series, 1892 to 1901); (iv) the same, but *including* those travelling cabin class (1902 to 1907); (v) alien *immigrants* (for definition see Essay 2) to the U.S.A. (1908 onwards).[6]

The change from one basis to the next involved differences in measurement which were far from inconsequential. For example, for the four overlapping years for which both Bureau

[6] This paragraph is largely based on Hutchinson, 1958. There are also good general summaries of the sources of migration statistics in International Labour Office, 1922, and in Ferenczi, 1929.

of Statistics and Bureau of Immigration data were collected (1892 to 1895 inclusive) the Bureau of Immigration data give aggregate numbers lower than those of the Bureau of Statistics series by 7.0, 12.6, 9.2 and 7.6 per cent respectively - a difference presumably chiefly explained by the exclusion from the Immigration series of cabin class passengers who were included in the Statistics series.[7] An even larger discrete jump in the series occurred when the Bureau of Immigration went over to an *immigrants* rather than an admissions basis in the course of the first decade of the twentieth century. For 1907/08, for example, alien immigrants to the U.S.A. were only some 84.6 per cent of alien arrivals. All of the ratios mentioned between overlapping series are based on the total inward movement from all countries, and while for some individual countries of origin the discrepancy between the successive series would be smaller than the national average, for others it would of course be larger.

Apart from these conceptual differences, the measured total inflow and inflow from individual countries would be sensitive to variations in the degree to which the coverage of movements at the various entry points approximated to completeness. The ideal of complete coverage was almost certainly never attained in the pre-1914 period, the nineteenth-century data being defective by virtue, in particular, of the under-recording of movements across the land borders of the U.S.A., especially that with Canada. Generally speaking, this gap in the statistics was probably *proportionately* more important the the further back in time we go. Thus, for the years 1851-1865 there is a big discrepancy between U.S. and Norwegian statistics of Scandinavian emigration to the U.S.A., and the U.S. Census data on the Scandinavian-born suggest that it is the American figures which are further from the truth. The reason is that at that period the majority of Norwegians reached the U.S.A. via

[7] Hutchinson, 1958, p. 984.

Quebec, because the ships engaged in carrying lumber to the U.K. offered a cheap "backhaul" rate for emigrants. Many Irish used the same route in the earlier nineteenth century and the U.S. data accordingly understate immigrants from that country also.

Beyond factors such as these, which influenced the degree of accuracy of the recording of *total* movements, lies moreover the question of the accuracy of the *disaggregation* of total arrivals or admissions by, for example, sex, age and nationality. Take for instance the last-mentioned, a form of disaggregation vital in almost any study of the causes of migration. Starting from 1898-99 the "admission of immigrant aliens" series moves over from a "country of origin or nationality" to a "country of last residence" basis, though an alternative breakdown by "race or people" continues to be available. The new basis implied some changes, the most obvious being the disappearance of immigrants from "Poland". This country could hardly be recorded as the new arrival's country of last residence given that in the nineteenth century it did not of course exist. That fact had not however prevented large numbers of arrivals responding "Poland" to the country-of-origin-or-nationality question prior to 1898-99, and being so recorded. The difficulty affects the study of the migration not merely of Poles, but that from the three countries (Austria-Hungary, Germany, and Russia) within whose national boundaries ethnic Poles largely resided in the nineteenth century. There are reasons, too, for suspecting some considerable inconsistencies in the recording of arrivals of immigrants from (and of departures to) Austria-Hungary and the Balkans. Indeed quite apart from inaccuracies such as those mentioned for which there was a specific cause and even excuse, it would be expecting too much to suppose that an inadequately-paid and understaffed service should always have maintained the highest standards of care and honesty when faced with arrivals averaging several thousands every day. An official who had

served as Commisioner of Immigration for the port of New York over the years 1893 to 1897 later testified that many pages in the records supposed to contain the full details of each arrival's nationality, occupation and so on consisted merely of "an index of names". Brinley Thomas, commenting on the report, reasonably states that "some of the returns were just guesswork".[8]

Thus overall, we should regard this series of data on U.S. immigration as probably only tolerably accurate in the measurement of what it purports to measure, especially for the nineteenth century; and we should recognise that accuracy apart, a number of substantial amendments were made over the sweep of the decades to the conceptual basis of the collection and presentation of the raw data, which render the latter strictly speaking incomparable for periods spanning one or more of these changeover points. These facts have been generally ignored by historians using U.S. immigration data in the course of statistical and econometric studies of migration.

It is as yet impossible to offer any systematic appraisal of the migration statistics of the other major receiving countries, though a glance through the Ferenczi and Willcox volume at once suggests that, as is understandable, these are generally much less complete than for the U.S.A. Further, there are certainly some indications of major defects of coverage or quality in some of the data. For example, Latin American scholars have noted that Argentinian immigration data comprise "arrivals by sea", confounding transient visitors with genuine "migrants". On the other hand, movements across the estuary of the Plate were never properly known and still less land-border crossings.[9] There is reason to think that part of the substantial movement of disappointed migrants from Europe to Brazil who moved on to

[8] Thomas, 1973, pp. 44-5.
[9] Alsina, 1910, pp. 26, 41, 82-3; Sánchez-Albornoz, 1974, pp. 155-6.

Argentina went unrecorded both in the Argentinian and, of course, in the European data.

Despite such difficulties, it has several times been argued that the receiving country statistics are to be preferred to those — where they exist — of the sending country. This argument has perhaps been conceived, if not stated, in terms of the U.S. data under the tacit assumption that these are representative of "receiving country" data generally, which they certainly are not. But even in comparing U.S. data with those of the sending countries, the points made in the last few paragraphs justify doubt as to whether the U.S. data should in fact be unequivocally preferred.

The reasons which have been put forward for preferring receiving country data are predominantly of an *a priori* character. The most fundamental advantage of receiving country statistics is that they alone are conceptually aligned with the notion of "immigration", in that they count the number of foreign-born actually entering the host country, that is at the point at which they become "immigrants". Sending country data, on the other hand, are based on the movement, or more often the anticipated movement, of people leaving or at some preparatory stage before leaving their native country. There may be a larger or smaller discrepancy between the true number of "emigrants" and those actually recorded by the sending country statistics, depending on how long before the event these are gathered (and how much time, therefore, is left for a change of intention to occur) and how reliably predictive of the act of migration is the requirement or process which makes the advance measure possible.

In the detailed study of Italian emigration to follow we shall see that in the early twentieth century changes enacted to the law governing the possession of and the basis of application for passports substantially modified the relationship between the major series of Italian emigration data, which were

passport-based, and the immigration statistics of the receiving countries. In the later nineteenth century the number of migrants from Italy to the major overseas destinations, the U.S.A., Argentina, and Brazil, was some 20 percent less in the Italian statistics as compared with those of the receiving countries; but from the turn of the century to World War One this difference was reversed, in the case of the first and third countries, and narrowed in the case of Argentina. As for Italian emigration to Europe, so many migrants went without passports, or made multiple trips on the one document, that the number of departures recorded in the official, passport-based statistics may have fallen short of the true number by upwards of a half.

It should by now be apparent that wherever we decide that migration is best measured, conceptually speaking, this does not necessary lead to an unqualified preference for using the migration data of the country indicated, as both sending and receiving country data fall short, in practice, of the conceptual ideal. Patient statistical work directed towards reconciling the two sets of data for various pairs of countries, though perhaps humdrum to perform, would constitute a real service to scholarship. Even when the focus of attention is on annual aggregate migration alone, sending country data may be better in practice if not in principle. Two econometric historians studying emigration from Sweden (Quigley) and Norway (Moe) to the U.S.A. prefer to use the sending country data, and get excellent results in doing so. Scandinavian emigration statistics, as indeed Scandinavian demographic data generally, are particularly good, and another Scandinavian study, that of Danish emigration to the U.S.A., is also quantitatively based on home country (police) data.[10]

However, the real advantage of sending country data emerges when we are interested not just in the measurement of total

[10] Quigley, 1972; Moe, 1970; Hvidt, 1975.

migration flows, but in such characteristics of the migrants as age, occupation, place of residence prior to emigration and/or place of birth. On all such characteristics most receiving country data yield little or no or, for various reasons, unreliable information. Further, the disaggregated sending country data may well be available in the same format for all destinations equally. When the focus turns from mere total flows between a pair of countries towards the plotting of regional origins, migrant cohort characteristics, or inter-destination comparisons, sending country data, even if less reliable in aggregate terms, may therefore become quite indispensable. Certainly it is in the manipulation of home-country data that the frontier of quantitative studies of migration, now to be found in Scandinavia, is moving. Hvidt's study of Danish emigration, based on police records, was able to computerise data for some 172,000 emigrants classified by year and month of departure; sex; whether travelling singly or in a group; occupation; age; place of last residence; and destination. Nothing approaching this is conceivable on the basis of receiving country data.

The widespread preference for receiving country data, however rational it may seem in the light of some of the arguments advanced to justify it, is in one aspect a probably unconscious manifestation of the " U.S.A. - centric " bias which has characterised much migration literature to date. This bias, further discussed, is understandable in view of the prominence of the U.S.A. in the total pattern of nineteenth-century migration; that country took very close to two-thirds of gross recorded inflows in the years 1861-1910. However, its share of the total declined almost monotonically within each successive decade within that period, and fell to barely 50 percent for 1911-20. Moreover, in terms of immigration *rates* (that is, immigration as a proportion of the population of the receiving country), the U.S.A. does not seem nearly so powerful a magnet as when we focus only on absolute numbers. In terms of immigration *rates*

the U.S.A. did not rank in first place amongst the four major American countries of immigration in any decade from the 1860s to the 1920s, and not in second place in most.[11] Further, in most decades at least the U.S.A. would also have ranked behind Australia and New Zealand in respect of the same measure.*

The truth is that the attractiveness of the U.S.A. as a country of immigration is to be understood and explained in terms of its ability to absorb relatively large numbers of immigrants *because of its own size.* On the eve of the American Civil War, when intercontinental emigration was just getting into its stride, the population of the U.S.A. was already nearly 31 million, as compared with less than 3¼ million in Canada (which, in any event, probably remained a country of net *emigration* throughout the last four decades of the nineteenth century),[12] 1¼ million in Argentina, 1.1 million in Australia, and less than 100,000 in New Zealand. It is therefore not surprising that the United States should have led in terms of *absolute* numbers of immigrants. But the ability of the other countries mentioned to attract larger numbers of immigrants *relative to their own population* is one of the many reasons why it is desirable to consider intercontinental migration less exclusively in terms of migration to the U.S.A. than has been the case thus far.

The preceding discussion of the statistical bases for the study of pre-World War One migration may have generated the impression that we have no real warrant for making even approximate statements about the gross inter-continental outflow of Europeans in the nineteenth century. But this is probably too pessimistic a conclusion, as many of the deficiencies in the stati-

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* And almost certainly behind Uruguay, for which however I have not found sufficiently precise demographic data.

[11] Ferenczi and Willcox, 1929, Vol. I, Text Table 14, p. 209. For an earlier view critical of "U.S.A. - centric" bias see Thistlethwaite, 1960.

[12] Keyfitz, 1950-51.

stics offset each other and the overall total — as opposed to, for example, particular pair-of-country components of it — may not be far astray. It would be defeatist, too, to write off as a pure coincidence the reasonable degree of agreement between the overall totals as calculated from receiving and from sending country data independently. For the whole period 1821-1915 a gross overseas outflow of some 46 millions is indicated by the available emigration data, while the corresponding immigration statistics record an inflow of some 51½ millions.

Much more serious than these uncertainties, if our interest lies in the quantitative impact of migration on population or labour force, is our ignorance of the size — and sometimes our wilful ignoring — of the return flow. Essay 3 explores the nature and significance of return migration from the U.S.A. in the years immediately before World War One, basing itself on the statistics of departing aliens which become available from 1907/08 onwards. It is argued that an overall rate of repatriation of about one third is close to the mark for the U.S.A. for the years immediately preceding World War One. For earlier years estimates for repatriation from the U.S.A. can only be very tentative owing to the incomplete count of people leaving the country and the failure to discriminate between U.S. citizens and aliens doing so. M. Simon has estimated alien *ocean* departures from the United States as a percentage of alien intercontinetal admissions at 25.0 percent for the 1870s, 17.0 percent for the 1880s, and 44.8 percent for the 1890s.[13] These estimates are of course incomplete by virtue of omitting land border crossings in either direction.

For countries other than the U.S.A. even more work still remains to be done. Alien departures were 43.3 percent of alien arrivals in Argentina for the period 1857-1914, according to Argentinian statistics. These comprise all alien overseas move-

[13] Simon, 1960, pp. 664-6, 690.

ments, however transient the stay in Argentina, by second and third class travel, and thus include (for example) each arrival and departure of the *golondrinas*, the Italians who crossed the Equator every year for the southern hemisphere harvest season. In the case of Brazil, the statistics are too incomplete to provide a long-term estimate, and are probably of dubious reliability, particularly as concerns departures. For the years 1899-1912 inclusive a repatriant ratio as high as 65.6 percent is indicated, even though for the first few years of this period the tally of departures is known to be incomplete. Admittedly, the first decade of the twentieth century was a period of great economic difficulty for Brazil following the collapse of the 1890s coffee boom; these problems brought about a deterioration in already unsatisfactory conditions for immigrant workers on the coffee plantations, to such an extent that in 1902 the Italian government forbade the recruitment of Italian emigrants by São Paulo State.[14] For a few years following the Pirinetti decree there was in fact a net alien outflow from Brazil. Even allowing, however, for the special conditions of the first decade of the twentieth century, it is probable that net immigration into Brazil was particularly low as a proportion of gross.

As for the British Dominions, it has already been pointed out that Canada was probably a country of net emigration through the last four decades of the nineteenth century, the considerable inflows being offset by net losses both of aliens and of the Canadian-born, many of them to the U.S.A. Only in the first decade of the twentieth century did the net movement turn definitively inwards. For Australia and New Zealand the statistics deal essentially with total passenger movements, and *gross* inflows are particularly misleading here because of the relatively large volume of movement between the two countries. As Spengler points out, *net* inward passenger movement is particularly low in re-

[14] Carneiro, 1948, p. 1033.

lation to gross for both countries (*c.* 19 percent for Australia, 1901-30, and 30 percent for New Zealand, 1853-1930).[15] However, to interpret this as signifying a particularly unfavourable reaction to Antipodean conditions on the part of the immigrants is unwarranted, owing to the completely embracing definition of immigrant as " everyone arriving " (in the case of Australia, even including through passengers on board ship!) on which the calculation is based.

These points remind us of the basic fact that the measurement of " net " migration depends completely on our definition of " migrant ". If the " immigrant " is someone who comes to a country other than that in which he was born and stays there until his death, then alien net immigration is necessarily 100 percent of gross. If, on the other hand, " immigrant " means everyone who enters or even " touches down " at a foreign country then net immigration may have been only a small fraction of gross even for the nineteenth century; in our own more mobile days net immigration on such a definition is only a *tiny* fraction of gross even for countries, like Australia and New Zealand, where migration normally makes a substantial contribution to population increase. The ratio of " net " to " gross " migration is therefore critically dependent on the definition of migrant adopted, essentially an arbitrary choice. Before World War One there was already a movement towards discriminating between " migrants " and temporary visitors on the basis of an intention to stay in or be absent from a country for a period of not less than twelve months. Adopted as the basis for classifying aliens entering or leaving the U.S.A. as " immigrant (or emigrant) " and " non-immigrant (or non-emigrant) " from 1907/08 onwards, this practice was adopted internationally under the leadership of the I.L.O. in the 1920s. When New Zealand migration statistics went on to a similar basis soon after the war, the ratio of

[15] In Thomas, 1958, p. 27.

" permanently " departing residents (i.e. " emigrants ") to " permanent " arrivals (i.e. " immigrants ") was only 16.3 percent for the years 1921 to 1924; but for the same years the ratio of *all* departures to *all* arrivals was 76.4 percent.[16]

Two points concerning repatriation remain to be made. First, in the case of the British Dominions (probably in this respect unlike the other major contries of immigration) the number of returning immigrants is probably swollen by an appreciable net loss of Dominion-born migrating to Britain or to other countries. Secondly, in all major countries of immigration the rates both of inflow and of outflow declined during World War One, but the first much more than the second, so that *net* immigration contracted particularly sharply. Indeed, in the case of Argentina the war years saw a net *out*flow back to Europe.

It is as yet impossible to calculate with any confidence a " repatriant ratio " for the whole of European intercontinental emigration in the nineteenth and early twentieth centuries, even supposing the prior question of the definition of " migrant " to have been settled. An educated guess would be that for the period 1821-1915, and for intercontinental " emigration " as conceived in the statement quoted at the beginning of this essay that there was a " total population outflow overseas of some 46 millions " during those years, an overall return rate of 30 percent would be more likely to be an underestimate than an overestimate, and a rate of 40 percent is quite conceivable. Certainly to proceed, in any demographic or economic analysis, on the assumption that since return migration cannot as yet be accurately measured it can be disregarded is to invite error.

Further, repatriation *rates* on the whole tended to increase through time — the absolute number of repatriants increased very markedly — though not monotonically, at least for individual countries. For the U.S.A., the repatriant rate was higher, according

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[16] Calculated from Ferenczi and Willcox, Vol. I, pp. 1001, 1006, 1011, 1013.

to Simon's estimates, for the 1870s than for the 1880s, and higher for the 1890s than for either. It was probably also higher for the 1890s than for the 1900s. The maximum repatriant rate before World War One probably came in the 1870s for Argentina, in the 1880s for New Zealand, in the 1890s for Australia, and in the first decade of the twentieth century for Brazil. Without any doubt, overall repatriant rates were higher after about 1870 than before it.

By and large immigration into and return migration from the receiving countries were *inversely* related in the short and medium run, but *positively* related in the trend. The short-term inversity arises, of course, from the influence of economic conditions, which have opposite effects on immigration and on emigration. For example, the 1907/08 recession in the United States led both to a fall in alien arrivals and to a rise in alien departures. The causes of the long-run (that is, longer than business cycle) similarity of movement are twofold. In the first place, return migration can only take place when there is a pool of foreign-born from which it can be drawn; and all the evidence suggests that the very large majority of repatriants were drawn from the ranks of immigrants of less than ten (or indeed five) years' standing.[17] Other things being equal, the larger the pool, the larger the repatriant flow. At least, therefore, until the gross inward flow passed its peak and began to decline, and probably for some years afterwards, the larger that flow (and therefore the larger the pool of recent immigrants), the larger the return movement. Secondly, there were a number of factors which exerted in the trend a similar influence on emigration and repatriation alike. Prominent amongst these factors was the cost, speed and comfort of ocean transport. Improvements in land communications from the migrant's place of origin or of residence overseas to the seaports were also important. There were powerful "psychic"

[17] See Essay 3, forthcoming.

influences, too, which told in the same direction, principally the increasing knowledge of economic and other conditions relevant to migration and an increasing familiarity with it - gained both from the migrant's personal experiences and from that of his peers - which greatly facilitated the readiness to move to and fro as economic advantage dictated. The Italian historian of emigration, Coletti, achieved a fine insight when, writing a few years before World War One, he pointed out that transatlantic migration in his own day was becoming more and more like the traditional annual migration of Italians to neighbouring European countries - like it, that is, in being based on a very exact knowledge of overseas conditions and an increasing confidence in undertaking long journeys in search of work, and thus in consisting, in increasing part, of *intentionally* temporary emigration.[18]

The role of transport is evidently very important in intercontinental migration. Some econometric historians, indeed, have thought that the cost of moving should figure explicitly in their explanations of the rise of intercontinental migration, and hypothesise that the volume of emigration should be inversely correlated with the cost of transposition. Unfortunately it has been impossible to subject this hypothesis to an explicit test as a consistent and long-term series of fares on an annual basis has not come to hand. It may be doubted, however, that the expected inverse relationship would, in the event, emerge. Such evidence as has been uncovered suggests that transatlantic fares altered very little, in the trend, after about 1830.[19] Certainly there was a dramatic fall, as was to be expected, following the end of the

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[18] Coletti, 1911, p. 184.

[19] This statement and the specific figures in this and the following paragraph are based on scattered evidence encountered in a large number of sources. A considerable proportion of this evidence was gleaned from the following sources, which are of particular interest in connection with the shipping aspect of transatlantic migration: Guillet, 1963; Potter, 1960; Page, 1911; Cowan, 1961; Hyde, 1975; Taylor, 1971, Chaps. 7 and 8; Maginnis, 1892.

Napoleonic wars. In the later years of hostilities and immediately after the wars a steerage passage without food from the United Kingdom to the U.S.A. cost as mush as 10 or 12 Pounds Sterling, but as early as 1821 the passage was down to little more than £ 4 from Liverpool to Quebec and £ 5 from Liverpool to New York, and perhaps only half that from Cork or the West of Ireland. A feature of these early years was the possibility of striking bargains with the masters — more advantageously, of course, in years of smaller numbers of emigrants — and of economising by providing one's own food or by sailing on a cheap backhaul under particularly unpleasant conditions, on a Newfoundland fishing vessel or a St. Lawrence timber ship. For the late 1820s fares as low as 10 shillings are reported for the Ireland to Newfoundland crossing by fishing boat. From the 1830s the steerage rate from Liverpool to U.S.A., including rations, stabilised at between £ 3.10s. and £ 5, and it changed little thereafter, though the fare for the crossing by sailing vessel was driven down still further with the advent of the steamer. Further, rate-cutting in times of diminished emigrant outflow led to marked short-term fluctuations even after the trade was taken over by steamers, and despite the efforts of the shipping conference. In 1885, for example, the steerage fare from Liverpool to New York went from £ 3 to £ 5,5s. in the space of three months.[20]

In the light of these characteristics of the movement of fares — long-run stability combined with short-run volatility according to the pressure of demand — it is very probable that the correlation between fares and the volume of emigration would turn out to be positive rather than negative, particularly if the test related to the period after about 1830 or 1840.[21] What seems

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[20] Hyde, 1975, p. 84.

[21] Specific confirmation of this line of reasoning can be found with respect to numbers of Swedish emigrants during the 1880s and the fares from Göteborg to New York: a clear *positive* correlation is apparent. See Brattne, 1973, Diagram VIII, p. 143.

certain is that the significance of transport changes lay not so much in the direct cost per mile as in the increasing speed, comfort, safety, regularity and accessibility of passenger services. In the late 1820s the average voyage by sailing vessel took 40 days from Liverpool to New York, and 45 days to Quebec. In 1843, of 179 recorded voyages from the British Isles to Quebec, the longest was of 88 days, the shortest 27, and the average 44. The average time from Europe to U.S.A. in 1867 was still 44 days by sailing vessel, but in that year the steamers could do the trip in about 14 days. The White Star line had cut this to 9 days 16½ hours by 1875, and to 7 days 15½ hours by 1890. Before World War One Cunard's *Mauretania* has crossed the Atlantic in less than 4½ days. (She was not, of course, typical of emigrant ships, though she did carry large numbers of steerage passengers.)

This dramatic reduction in the duration of the Atlantic crossing was important in a number of ways. Effectively, it reduced the *cost* of the voyage when this is calculated, as is appropriate, as the sum of the money fare plus the opportunity cost of the earning time wasted on board ship. The latter, not the former, was the major financial advantage which the steamer offered in the migrant trade, and it was particularly important, of course, to the temporary emigrant for whom the financial attractions of a period of employment in the New World depended critically on his ability to amortise quickly the capital cost of going and returning. Thus speedier transport (by rail on the European continent, of course, as well as by steamer on the high seas) offered a financial inducement to temporary migration, as well as, more obviously, making this physically more feasible. (The *golondrinas* could not possibly have shared their year's labour in harvesting between Europe and South America in the days of sailing ships.) It is probable too that the growth of steamship fleets designed for the migrant traffic led to excess capacity on the *eastward* run, and thus cheap "backhauls" for repatriants - yet another reason

for the increase of return migration. (In the early nineteenth century, when the volume of emigration was much smaller, it was the *westward* run on which there was excess capacity of a sort, in the ships bringing the bulk exports of North America — grain, fish, timber, cotton — to European ports.)

The greater speed of rail and of ship, however, brought other advantages, too, beyond the narrowly financial ones. The appalling conditions of steerage accommodation on the ships which carried the emigrants of the early and mid-nineteenth century deterred all but the most resolute, the most desperate, and the most ignorant of emigrants. Even had conditions not improved with the advent of the steamer, as in fact they did, the great reduction in the average length of the voyage would at least have made these conditions easier to be borne. And not only of the *average* length; for the worst feature of all of crossing the ocean by sailing ship was the fact that the weather made the length of voyage notoriously *unpredictable*. A voyage lasting half as long again as the average — not an uncommon thing in the first half of the nineteenth century — brought appalling hardships from thirst, hunger, sickness and boredom. The greater punctuality of the steamer was thus an enormous reassurance, quite apart from its speed.

Even this does not exhaust the contribution which changes in transport made to the rise of intercontinental migration in the hundred years between Waterloo and the first world war. For in the later nineteenth century the profitability of many steamship lines and railway companies had become critically dependent on the migrant traffic, and they thus acted, in their own interests, as recruiting and advertising agencies for migration. While the efficacy of agents and advertisements in recruiting emigrants is often denied by historians — at least by American historians. Europeans are not so sure — it will be argued that the diffusion of knowledge about opportunities was a critical element in the geographical spread of the sources of migration

in the later nineteenth century, and advertising had its part to play here.

Thus changes in transportation were surely one of the major and most widely-influential factors in the rise of migration in the nineteenth century. Yet here, as so often when we pose questions about historical causation, it is difficult in the last resort to know just how far a given development was cause and how far effect. Surely, the influence of transport changes on migration was *largely* cause, since the big developments — the railway, the steamship, the screw, the steel hull, the turbine — were so much part of the logic of technological development, and served so many other ends as well as carrying migrants around the globe. They were effect as well as cause, however: for migrants were undoubtedly by far the largest single category of passengers on the world's fleets of passenger liners, and perhaps on some railways. The historian of the Cunard line has written that "...the great changes, both technological and commercial, which took place on the North Atlantic between 1860 and 1880, are explicable only in terms of the emigrant traffic." [22] And ports such as Naples, La Coruña, Fiume and Götcborg would hardly have attained the preeminence they did had it not been for the migrant traffic. And transport changes were not the whole cause either; for other broad historical developments are equally essential — and equally obvious — when we seek to explain why the century after Waterloo was the greatest epoch of intercontinental migration in the world's history.

In the simplest terms, if migration is to take place there must be people who want and are able to leave where they are; countries which they wish and are permitted to enter; and an acceptable means of conveying them. The later nineteenth century not only provided the third of these more amply than any

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[22] Hyde, 1975, p. 81. For the importance of emigrant traffic to the Hamburg-Amerika line see Sorre, 1955, pp. 64-5.

preceding period; it also met the first and second conditions more fully than any preceding period, and the second probably more than any subsequent period.

Poverty and famine, of course, were not new in the years following the Napoleonic wars. In some areas, for example in the West of Ireland, in the Scottish Highlands, and in parts of of Germany, they had however become more widespread as population had risen in the later eighteenth and early nineteenth century without any comparable improvement in the productivity of the soil. It was from such areas that the first post-war mass emigration — " mass " having regard to the indiscriminate and desperate nature of the movement, though small in absolute terms in comparison with what was to come — was provoked by the rural distress of 1816-17. But the increasing " push " from parts of Europe came about not just because of an increasing expulsive force generated by the deteriorating economic plight of particular classes or areas, but by the loosening of former restraints which had inhibited the search for a solution by moving elsewhere.

In Western Europe these restraints were not primarily of a legal character, though of course in Britain restrictions on the emigration of skilled workers were abolished only in 1825. The first post-war surge of emigration from south Germany found state governments and local officials bewildered and to some extent at odds with each other on the subject of granting passes to intending emigrants - though large numbers went without passes, anyway. As a historian of German emigration has written:

> " Noncoercive, vaguely antiemigrationist state policies were ... in large part frustrated, in the depressed areas where the fever struck most virulently, by local official and civic leaders, who saw the helplessness of the poor and who were not anxious to persuade paupers and ne'er-do-wells to remain in their communities." [23]

[23] Walker, 1964, p. 20.

It was not long, however, before such official impediments as there were were removed. Indeed in Britain particularly, but also in a few German states, *encouragement* to emigration through public and charitable subsidies quickly replaced impediments, for emigration came to be regarded as a valuable aid in coping with the distress caused to disadvantaged sectors and occupations by the accelerated pace of technical and economic change. Moreover, while the tone of governmental and press commentaries in Germany continued to be pessimistic about conditions in America, these enjoyed little credibility and had little deterrent effect as soon as the first emigrant letters bringing more optimistic tidings were received, to be eagerly read and discussed by a public avid for knowledge.

In Eastern and Central Europe legal impediments to emigration were of course far more serious. Even the abolition of serfdom following the '48 or by the Russian reform of the 1860s did not guarantee freedom of movement, as in most places the peasant remained tied more or less tightly to his village community and still required a passport to depart legally. Such permission was often given or withheld to suit the convenience of the dominant local class. In Hungary, for example, passports were much more readily granted in the region of peasant farming in the High Tatra mountains than on the Danube plain, where the interest of the great estate owners lay in retaining an ample supply of low-wage harvest labour.[24] Still, the eastwards march of personal freedom was obviously an important part of the explanation of the corresponding geographical shift of the sources of European emigration as the nineteenth century ran its course. And southwards march as well as eastward, for in Spain, too, emigration was strongly if incompetently discouraged in the first half of the nineteenth century. Only from 1853 onwards did a series of laws and decrees grudgingly and progressively

[24] Kosa, 1957, pp. 505-6.

liberalise it until in 1903, the mere production of an identity card was all that Spaniards needed to emigrate.[25]

But in East, West, North and South Europe alike legal prohibitions posed in the last resort a less widespread and less decisive limit to emigration than the lack of knowledge of other opportunities. It is in the increasing spread and sophistication of knowledge of the world overseas that the most fundamental of all historical changes related to intercontinental migration is to be found. *Related to*: for this knowledge was both cause and result of such migration, being gained and diffused most generally and most reliably by reports of the world overseas sent back by those who had gone there, or brought back by those who returned. Ingrid Semmingsen, historian of Norwegian emigration, sensitively writes of the shock the Norwegian and the Croat peasant alike experienced when first they heard of America from an emigrant letter or from a repatriant arrived home.[26] By the end of the first decade of the twentieth century the great Italian writer on emigration, Coletti, could claim with equal truth and sensitivity that in his own day, emigration no longer inspired fear; one went to America with the same indifference as one might go to the *agro Romano*. "The Americas have today become our neighbours: the picture of them in the minds of the emigrant peoples is so alive, so detailed, that comparisons between how much they have here and how much they might hope to have there are relatively easy."[27]

The increasing readiness and ability of Europeans to emigrate was matched — again, cause and effect are interwoven here, too — by the ability and willingness of lands overseas to absorb them. At the end of the Napoleonic wars Australia had a white population smaller than that of a modest English country town; New Zealand, a mere handful of resident missionaries,

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[25] Vicens Vives, 1959, pp. 27-30; Oddone, 1966, p. 97; Nadal 1971, pp. 155-7.
[26] Semmingsen, 1961, p. 26.
[27] Coletti, 1911, p. 200. (Author's translation.)

whalers and sealers, and traders. Argentina had a population of about half a million and Canada much the same. Brazil had something over 3 million people, but two-thirds of them were slaves, and the remainder a mixture of whites, mestizos, and freed blacks. Even the U.S.A. numbered no more than 8½ million inhabitants. These figures do not suggest any capacity to absorb large inflows of migrants quickly.

Moreover, prior to the nineteenth century not all of these countries were willing to accept immigrants even in proportion to their absorptive potential. In colonial times Spain and Portugal attempted to restrict immigration into their Latin American possessions to their own nationals. Although independent Argentina at once moved to encourage more general immigration, the unstable political regime worked in the opposite direction, and it was not until more settled conditions were reflected in the adoption of the Constitution of 1853 that the way was effectively opened for mass immigration. That Constitution adopted in full the philosophy of Juan Batista Alberti: "En América, gobernar es poblar." (*In America, to govern is to populate.*) The provisions of the 1853 Constitution, in fact, placed the immigrant in some respects in a more advantageous position than the native Argentinian, conferring on him all the same rights but exempting him from certain responsibilities, notably that of military service.[28] Small wonder that only a tiny fraction of the immigrants of the later nineteenth century sought to acquire Argentinian nationality, easy though it was to obtain.[29] Brazil, equally, threw open its ports to direct foreign trade and permitted land to be conceded to the foreign-born in 1808, and the the first assisted colonisation by non-Portuguese was that of the the Swiss at Nova Friburgo in 1820.[30] True "mass" immigrato this country, however, waited on the coffee boom of the later

[28] Alsina, 1910, pp. 43-5; Bunge, 1944, p. 31.
[29] Germani, 1970, p. 315.
[30] Carneiro, 1948, pp. 1014-5.

nineteenth century and the urgent need to find an alternative labour supply when it became obvious that the days of slavery were numbered.

There is therefore no real difficulty in explaining why there had been no mass emigration from Europe in the centuries before Waterloo, nor in explaining, in very broad terms, why the total volume of emigration should have tended to increase in the course of the nineteenth century and why it tended to become a more nearly continent-wide phenomenon. The problem is to explain the more detailed patterns of movement, chronologically and geographically, within that broad framework.

It is equally clear what brought the period of mass migration to a close. First the world war staunched the flow, and for a time (until shipping shortages decreed otherwise) also generated an increased rate of return to Europe — the mixed results of patriotism, of concern for family back ' home ', and of the misgivings on the part of some non-naturalised aliens as to their status and treatment should the U.S.A. become involved in war. This meant, of course, that net immigration declined much more sharply even than gross. After the war, the gross outward flow recovered quickly, but hardly to the levels of the pre-war decade, and only for long enough to prove the need, as U.S. opinion saw matters, for the great restrictive Acts of 1921, 1924 and 1927. These measures had counterparts, though less harsh ones, in Canada, where from 1923 onwards the immigration of people not of British stock was restricted, save for those intending to work on farms or as domestic servants. By administrative regulation Europe was divided into " Preferred " (that is, North-West) and " Other " areas, with a tighter control over the issue of visas to persons from the latter.[31] Latin America kept the door open longer, Argentina, Brazil and Uruguay abandoning unrestricted immigration only during the depression. Argentina

[31] England, 1929, Chap. II.

closed the door in 1931;[32] Brazil first abandoned immigration subsidies in 1927, then passed a series of restrictive administrative decrees beginning in 1930, and finally introduced a quota system by Constitutional amendments in 1934 and 1937.[33] Uruguay restricted entry in 1932 to special classes of workers with assured jobs to go to.[34] British emigration to the southern hemisphere Dominions received a boost in the 1920s; the U.K. government sought to relieve chronic unemployment and ensure the mother country's access to cheap food and raw materials by joint U.K. - Dominions assistance to emigration and settlement under the Empire Settlement Act of 1922. The scheme was far from being a huge success, however, and it was abandoned in the depression of the early 1930s.

That depression proved the *coup de grace* for the great inter-continental migration movement, for there was little point in fleeing from unemployment on Tyneside or in Naples merely to suffer unemployment in New York or Melbourne. Indeed, the depression caused a net movement back towards Europe, for if no work was to be had in either place, the Old World offered better support than the New both from family and from state, both financial and psychic; and savings lasted longer in Europe where living costs were lower. It is quite clear that the depression rather than the restrictions on immigration was the true cause of the smaller flows. Net immigration declined and even became negative in countries like New Zealand, which did no more to shield its workforce from competition for jobs than to suspend subsidies to immigration; and during the depression years immigration into the United States never came close to filling the quotas.[35]

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[32] International Labour Review, 1937, pp. 353-4.

[33] Doria de Vasconcellos, 1940, p. 213; Neiva, 1944, pp. 488, 517; Smith, 1963, p. 143.

[34] González-Rothvoss y Gil, 1949, p. 187.

[35] Citroen, 1948, p. 47.

2) *Some statistical and econometric studies: survey and critique*

It is natural that migration should have proved an attractive subject for economic historians of the cliometric school, as in one of its most obvious facets it is an inescapably quantitative topic. It is quite probable, in fact, that if one includes studies of internal as well as of international migration, this subject may have generated in the last ten or fifteen years a larger number of econometric studies than any other single area of historical investigation, not excluding slavery or railways. Even confining oneself to *inter-continental* migration of the pre-World War One period, one quickly identifies 18 leading studies, most of them published in the last ten years, calling for comment and summary.

One of these studies (Pope, 1976) is a brief but valuable critique of three earlier studies, one of them by Pope himself. The remaining 17 fall into five groups. First, eight of them are studies based on the estimation of migration equations for one or more pairs of sending and receiving countries, with annual values of migration flows, whether absolute numbers or rates relative to the population of the sending country, as the dependent variable and a number of economic series — income and employment data, for example — relating to either the sending or the receiving country, or both, as explanatory variables. These equations have been estimated by ordinary least squares or by some more sophisticated multiple regression technique. The studies in this group, in chronological order of publication, are those by Kelley (1965), Wilkinson (1967), Pope (1968), Moe (1970), Wilkinson (1970), Gallaway and Vedder (1971), Quigley (1972) and Richardson (1972). Secondly, two further studies use similar data and variables, but employ a cross-country rather than a time-series approach, aggregating emigration for each of a number of countries over the period 1861-1910 (Easterlin, 1961), or over two decadal intervals (Tomaske, 1971). Tomaske uses the fa-

miliar multiple regression approach, but Easterlin prefers to rely largely on various types of ranking procedures and on the analysis of lags. Thirdly, a group of four papers address the problem, not of the size of migration flows from one or more sending countries, but of the *dispersion* of the immigrants amongst various states in the U.S.A. These papers, again in chronological order, are by Vedder and Gallaway (1970) (a) and (1970) (b), Orsagh and Mooney (1970), and Gallaway, Vedder and Shukla (1974). Though these papers deal with a slightly different topic, it is intuitively likely that there would be a considerable overlap tetween the factors determining the location of immigrants *within* the U.S.A. and the factors determining the size and direction of U.S. immigration flows. Further, attractive features of these dispersion studies are that they permit the use of different data sources, such as census material, and the inclusion of variables reflecting possible influences on migrant behaviour which cannot readily be evaluated in studies of inter-country flows, such as climatic preference. Finally, the three remaining studies deserve to be placed, for differing reasons, in categories of their own. One is the contribution by Jeffrey Williamson, published in an article in *Explorations in Economic History* in 1974 under the title, 'Migration to the New World: Long Term Influences and Impact', and which also appeared in, somewhat modified guise, as chapter 11 of his book *Late Nineteenth-Century American Development: a General Equilibrium History*, also published in 1974. Williamson's work is to be distinguished from the studies already listed both by its more ambitious goals and by his explicit criticism (and, in part, rejection) of the economic-analytic basis of those studies. Finally, the same volume of *Explorations in Economic History* (1974) includes a short article by Poulson and Holyfield addressing the familiar problem of cyclic fluctuations in migration levels but distinguished by the use of spectral analysis; while Neal (1976) uses cross-spectral analysis to study long swings in Atlantic migration.

As is to be expected the increasing number of studies, many of them addressing very similar or even identical problems, has begun to provoke some discussion of the methods employed and results achieved. Useful critical comments and evaluations are to be found in Moe's 1970 thesis (pp. 192-214), in Williamson's 1974 article (pp. 363-9), in the paper already mentioned by Pope (1976), and in an unpublished paper by the same author.[1] I have drawn on these evaluations to some extent in the pages which follow. However, all three writers adopt in their critiques a primarily technical stance, confining their comments largely to questions concerning either the economic validity of the formulation of the equations developed in the papers surveyed or the techiques of statistical estimation employed. I first want to to make a much more general comment provoked by a reading of this econometric literature, and that is to draw attention to two biases which reduce the interest and significance of the whole body of literature concerned, though it would be quite unfair to advance them as criticism of any individual contribution. These biases are first, an orientation overly centred on the United States of America, and secondly, a relative neglect of the countries of the " New " as opposed to those of the " Old " immigration. Thus, in all except five of the eighteen studies described, U.S.A. is the only receiving country treated, while only one of the eight studies in group one * includes any sending country from the area of the " New " immigration. I know of no study which could properly be called econometric exclusively devoted either to emigration from any country of the " New " immigration, or to immigration into any Latin American country.

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* Wilkinson (1970), which includes Italy and Russia as two of the seven sending countries.

[1] D. Pope, " A Survey of the Literature on the Historical Determinants of Migration, with Some Tentative Ideas for a Supply-Demand Model ", Dept. of Economic History, Research School of Social Sciences Seminar' Australian National University, March, 1973.

Each of these biases is easy to account for. The first reflects, properly to an extent, the prominence of the U.S.A. in nineteenth-century migration, and also arises from the earlier development and larger numbers of practitioners of econometric history in that country. The second in part arises from the fact until very recent years consistent income or employment data were not available for countries of the "New", unlike those of the "Old", migration. One may hope that this imbalance will quickly be rectified now that such information is becoming available for a number of countries of south and east Europe.

Whatever their causes, these biases in the modern econometric literature of international migration inevitably imply some limitations, some obvious, some less so. Among the more obvious disadvantages is that strikingly important questions provoked by the *choice* between destinations which was available to and exercised by potential migrants do not insist on being answered when attention is focused on only one receiving country. If the archetypal nineteenth-century migrant was a rational being with the maximisation of lifetime net earnings as his objective function (which can be disputed, though is commonly assumed by econometric historians of migration), and if that goal could best be achieved by migration to the U.S.A. (though that too is debatable, as it is improbable that America had the highest real per capita income in the world in the mid-nineteenth century), then why did any migrant ever go anywhere else than the U.S.A.? Further, why did the choice of destinations vary as it did over space and over time, so that emigrants from Norway and Sweden at one extreme of Europe and from parts of the Balkans at another overwhelmingly selected U.S.A. as their preferred destination, in a ratio of almost 20 to one; whereas those from Spain and Portugal, from a third corner of the continent, overwhelmingly preferred destinations *other than* U.S.A.? And why did the destinations preferred by all the areas just mentioned remain reasonably constant over time, while some other countries, notably Great

Britain and Italy, not only revealed more varied patterns of destination, but *changed* their preferences over time? And why, moreover, did they change them in opposing directions? (So that Great Britons *began* by preferring U.S.A. as their prime target but switched gradually to the countries of the British Commonwealth; whereas Italians *began* by preferring other European countries, switched partially to Latin America, and *ended* by choosing U.S.A. as their prime target.) Surely these differences must suggest the possibility of determinants of migrant behaviour which cannot be fully explored in models relating solely to migration to the United States. A specification which appears satisfactorily to account for migration from a given country to the U.S.A. can hardly be equally satisfactory as a general explanation of that country's emigration, if in fact a majority of its citizens seeking their fortunes abroad chose destinations other than the U.S.A. In this connection it must be borne firmly in mind that in the three-quarters of a century or more before the depression of the 1930s, the United States and the Dominions were *substantially* competing attractions for Great Britons; Great Britain and the United States were *substantially* competing attractions for the Irish; Europe, Latin America, and the United States were *substantially* competing attractions for Italians; Europe and the United States, and in the twentieth century Canada and Brazil, were *substantially* competing attractions for Poles and for some other Central Europeans; Latin America, including the Antilles, was the chief destination throughout for emigrants from Spain and Portugal;* and only in the rest of Europe was the United States the consistent first choice.

The dominant focus on migration to the U.S.A. has also led to a relative neglect of possible influences on migration which were not directly operative in that particular context, such as

* However, see Essay 5, forthcoming, for a discussion of differing preferences as to destination as between mainland Spain and Portugal and the Atlantic islands.

subsidised immigration — though, of course, subsidies in favour of migration to, say, Brazil and the British Dominions may well have had a quite major, if indirect, implication for the level and composition of U.S. immigration. More generally, that focus has led to a relative neglect in the econometric literature of the interaction between influences favouring, and the levels of, migration to two or more competing destinations. (Though some analysts have attempted to make some allowance for this: for example, Williamson (1974) enters emigration to non-U.S. countries as an (exogenously-given) explanatory variable in his estimating equation, while Kelley (1965) allows British levels of of emigration to countries other than Australia to feature as one explanation of residuals lying between his predicted and the actual level of Australian immigration.) More subtly, the American focus and American authorship of most econometric studies of migration have led to a doubtless unconscious preference for modes of analysis and assumptions fashionable in America. Belief in the ability of an economic-maximising function to explain a great variety of social decisions, for example in such fields as marriage, education, and crime as well as in choice of occupation, is far stronger and more widespread in U.S.A. than in Europe, and this has carried over into the specification of migration equations. Save for one or two of the variables in the dispersion models in group three, such as climate and dislike of the U.S. South, there is not a single non-economic, non-demographic explanator in any of the models used in any of the studies listed above. Surely this is odd in view of well-known features of the history of migration to the U.S.A. itself - even in the nineteenth century, when economic motives for migration may be conceded to have been more prominent than in some other (including some later) periods.

Finally, too exclusive a concern with migration to one particular receiving country may induce an incautious habit of mind in which even well-known facts lying outside that particular frame of reference are lost sight of. Thus Williamson (1974) might

have been more cautious in ascribing the lower level of migration from Great Britain to U.S.A. in 1904/07 as compared with 1870 to "declining push", had he recalled that total annual emigration from Great Britain was in fact *larger* in the later than than in the earlier period - it was merely that a much smaller percentage of the later exodus was destined for the U.S.A.

It is hardly necessary to comment at length on the desirability of correcting the other major bias in the present econometric literature of migration - that is, by analysing the determinants of emigration from some of the countries of the "New", as opposed to those of the "Old", immigration. It is premature to join with Tomaske in concluding that the evidence does not indicate 'that the so-called 'new' migration from Eastern and Southern Europe need be treated as a special case differing in some fundamental way from the 'old' migration from North-western Europe." [2] At a minimum, the differing chronologies and patterns of inter-continental migration from these two areas would suggest that even if the same sorts of influence were at work, they must have operated in a manner differing sharply over time and space as compared with their role in northwest Europe. Surely one can pronounce confidently on the degree of similarity in the determinants of emigration levels in these two areas of Europe only *after* each area has been exhaustively studied, not before.

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Let us now turn from what the modern econometric literature does *not* tell us to what it *does*. First, notice that in regard to the study of migration, as with that of the economic history of railways, of slavery, or of money, econometric techniques were not introduced into an intellectual vacuum. Rather, they entered a field of study in which certain active controversies, or traditional

[2] Tomaske, 1971, p. 853.

interpretations actually or potentially under challenge, already engaged the attention of scholars, and these preoccupations suggested hypotheses for testing or offered targets for attack. Further, as in some other fields of study the econometric techniques were not brought to bear in areas which had been totally innocent of quantitative treatment. In the study of migration this, indeed, could hardly have been the case. However, the traditions on which the new techniques impinged had characteristically involved quantitative methods of an older and in some ways less rigorous vintage, relying heavily for example on visual impressions from the graphical presentation of data, on turning-point and lead-and-lag analysis, and the like.

The earliest of the 18 studies listed above (Easterlin, 1961) follows very much in this tradition and, indeed, is methodologically closer to the earlier quantitative studies than to most of those which have followed. More importantly, however, both Easterlin's paper and many of its successors have had as their aim, at least in part, to resolve issues which interested the the protagonists of this older literature. Perhaps the most important contributions to that literature were those of Harry Jerome (1926), Dorothy S. Thomas (1941), Brinley Thomas (1954, second edition 1973), and Simon Kuznets (1958).

These works are well-known, and in so far as their insights have either been rejected by or absorbed into the later literature, they do not call for detailed comment here. Jerome's early N.B.E.R. study argued essentially that fluctuations in American immigration were dominated by U.S. economic cycles, though he conceded that the relationship was closer after about 1870 than before it. Jerome was aware that this conclusion would become vulnerable if business conditions in the U.S.A. and in sending countries were shown to be *inversely* correlated, for then one could not be sure whether it was the pull of American prosperity or the push of European recession which really caused each upswing in westward movement, and conversely.

Jerome thought to have resolved this issue by demonstrating graphically that business cycles in the U.S.A., the U.K. and Germany, and less certainly Italy, were broadly synchronous rather than inverse, so that upswings which coincided with conditions of prosperity in the U.S.A. tended to do so *despite* simultaneous prosperous conditions in the sending countries. This demonstration was hardly fully compelling, however, and 13 years later Schumpeter's graphing of his pre-World War One Juglars suggested a good measure of synchronous fluctuation as between the U.S.A., the U.K., and Germany only in respect of monetary indicators, with rather less agreement in regard to production.[3] Kuznets supported Jerome's interpretation, but preferred to rest more of the burden of proof of the primacy of U.S. "pull" on the synchronous nature of fluctuations in emigration from various European countries to America. His argument was that since it was "highly unlikely that the timing of ... 'push' elements was the same in so many different parts of the world the similarity must be ascribed to some 'pull' factors."[4] The claim that this similarity of timing was "highly unlikely" was, however, left unsubstantiated, and it has in fact been challenged by European historians.* One hardly resolves a contentious issue by stating an untested hypothesis.

Dorothy Thomas made a close study of Swedish emigration and reached a somewhat different conclusion from that of Jerome

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* Presumably the dominant preoccupation with U.S. immigration explains why the obvious, parallel test was not made: namely, did emigration to *all* destinations from each sending country fluctuate synchronously — suggesting the influence of sending country conditions — or not? It might be added that Kuznets employed only a rather crude measure of the degree of synchronisation of migration totals, namely the change in migration volumes from decade to overlapping decade. Further, this measure does not reveal complete agreement amongst the various countries even as to the *direction* of change, and much less as to its *extent*.

[3] J. SCHUMPETER, *Business Cycles*, McGraw-Hill, New York, 1939, Vol. II, pp. 474, 487, 493, 512.

[4] Kuznets, 1958, p. 31.

and Kuznets. While she found clear evidence of the influence of American "pull", this was substantially counteracted by the competing attraction of jobs in Sweden during periods of industrial expansion there: "The latent agricultural push towards emigration became an active force only when Swedish industrial depression occurred simultaneously with expanding or prosperous contitions in the new world." [5] Brinley Thomas also favoured an explanation allowing for influences from both sending and receiving countries, but his proposed model was more ambitious (if less lucidly articulated) than that of Dorothy Thomas. He saw the succession of prosperous and depressed conditions on both sides of the Atlantic as being not autonomously determined, sometimes synchronous and sometimes inverse, but as being simultaneously determined in what was effectively a single economic system linked across the Atlantic by endogenous trade and factors movements into a pattern of interactive behaviour: "the Atlantic economy."

The early econometric studies thus inherited from their predecessors a legacy of at least three traditional predispositions or preoccupations: a tendency to view inter-continental migration overwhelmingly, if not exclusively, in terms of migration to the U.S.A.; a disposition to believe that the determinants of that migration would best be laid bare by studying them within a framework of research on business and other cycles; and a piece of conceptual baggage, the "push-pull" hypothesis, as the major organising theme. Of these three, enough has already been said about the first. As much as it is intended to say about the second — which is not much, the heuristic value of the study of cycles being, in this writer's view, distinctly limited — will be said as the need arises. But the "push-pull" dichotomy provides a convenient starting point for some more detailed comments on the econometric studies.

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[5] Thomas, 1941, p. 169.

The notion that the influences on migration can be usefully classified into "push" and "pull" factors has a venerable history, going back far beyond Harry Jerome's writings. Indeed, a German observer writing at the time of the emigration mania occasioned by the widespread harvest failures of 1816-17 — the first of the considerable list of "push elements" occurring simultaneously in several European countries, the phenomenon which Simon Kuznets characterised as "highly unlikely" — gave his opinion that "the reasons for so universal an *Auswanderung* mania lie not in an attraction from outside, but in a certain despair of the possibility of a future in the Fatherland." [6] It is strange that in the more than a century and a half since this first clear articulation of the push-pull dichotomy few commentators have attempted, let alone succeeded in offering, a clear definition of just what the distinction means. The decision to emigrate, after all, is a decision made in the mind of one individual, or perhaps collectively by a family. While the various economic and other considerations entering into this decision may relate to conditions in this place or in that, they must eventually come together and be weighed in the balance by the judgement of one individual or a small group, forming the elements of a *comparison* the outcome of which results in the decision to stay or to go. One wonders what Italian peasants could possibly have made of the question which at one time the *sindaci* used to put to them: are you leaving because of the hard conditions of life here, or in search of better circumstances abroad? [7]

Perhaps one can conceive that at times conditions at home may have been so completely insupportable that migrants abandoned their homes without any clear idea of where they might go or of what awaited them when they got there; and one might

[6] Quoted in Walker, 1964, p. 22. Note also the interesting hypothesis propounded in 1851 in the London *Times* that "emigrations commonly begin in repulsion, and go on with attraction." (Quoted in Abbott, 1926, p. 126.)

[7] Foerster, 1919, p. 32.

legitimately characterise such an exodus as determined by "push". The flight of starving peasants from Baden-Württemberg following the 1816 harvest failure had, indeed, much of this character; many of these wretches got no further than Rotterdam. As late as 1840 a Norwegian doctor who happened to take ship for the U.S.A. from Hamburg in company with some 30 Germans was astonished by the degree of their ignorance about their destination, whither they were all bound in the expectation of acquiring gold and silver without effort.[8] But such ignorance was characteristic only of *pioneer* emigration; and as soon as the letters began to arrive home (to be eagerly devoured by a public hungry for knowledge) and opinions began to form, however inaccurately at first, about conditions overseas, the decision whether or not to migrate became essentially the outcome of a process of *comparison* in which one is puzzled to know just what, psychologically speaking, is meant by "push" and "pull" factors.[9]

This is particularly so in the context of the hypothesis, which is generally if not universally accepted by present-day students of migration, that during the nineteenth century emigration was motivated essentially by the expectation of pecuniary gain. Since migration necessarily involves costs, this hypothesis, whatever the precise formulation, implies that there must be a suf-

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[8] Blegen, 1955, p. 79. The early decades of German emigration were marked by some striking miscalculations. Thus, a few desperate men were recruited to work in the plantations of the British West Indies after slavery was abolished; some joined Belgians in an attempted settlement of the Santo Tomas Bay in Guatemala; and there were several attempts to found a German colony on the Mosquito Coast in Nicaragua: Walker, 1964, pp. 80-1, 149.

[9] In a somewhat different context, this point was made with delightful and characteristic lucidity by MICHAEL LIPTON (*Why Poor People Stay Poor*, Temple Smith, London, 1977, p. 397, fn. 18):

"... if I move from *A* to *B*, I do so because I prefer *B* to *A*. If *A* is a house on fire, or *B* a gold mine, the causality is clear: but if on balance I prefer *B* to *A*, it makes little sense to ask, 'Do you prefer it because of *B* or because of *A*?' Most migration — except for disaster treks — is just such 'preference on balance'. It *does* make sense to ask what *changes* the balance."

ficient income differential in favour of the country towards which migration takes place. This differential, however measured, involves a comparison of incomes in the sending and in the receiving countries, and can become larger or smaller as a consequence of a change *in either of these*. For the purpose of the potential migrant's decision-making, it is hard to see how it can be material whether a given change in the differential is caused by an income change in the receiving country, or by one in the sending country. Hence we must agree with writers such as Wilkinson (1970), Moe (1970) and Pope (1968) who have used some measure of the income *differential* as their income variable, and find fault with Gallaway and Vedder (1971), Richardson (1972), and Quigley (1972) for adopting separate income measures for each country. Quigley perhaps does not deserve this censure in so far as he is able to enter wage variables for agriculture and industry separately for each of the countries concerned (Sweden and U.S.A.). The furnishing of sectoral wage data perhaps justifies the procedure, and in any event makes the fashioning of a measure of income *differential* a more puzzling task. Even so, it is hardly surprising to find that the null hypothesis that push and pull influences were the same (judging by the size of the elasticities) cannot be rejected at the 0.05 level in any of Quigley's estimated equations.[10] At the very least, to enter income or wage variables separately for each country unnecessarily sacrifices a degree of freedom, while it becomes much more seriously objectionable if the interpretation is placed on the estimated coefficients for the the two income measures that they provide a measure of 'push' and 'pull' respectively.

There is perhaps rather more conceptual justification for entering separate measures of unemployment in each country. For one thing, it seems a little improbable that a person contemplating migration will compare unemployment levels in quite

[10] Quigley, 1972, p. 121.

the way in which it seems natural to suppose that he will wish to compare real income levels — though an analyst of a more recent migratory flow, Belton Fleisher, has in fact got good results by using as an explanatory variable

$$\frac{U_{pr} - U_{us}}{U_{us}}$$

where U = unemployment rate, and the subscripts refer to Puerto Rico and the U.S.A. respectively, to explain fluctuations in Puerto Rican migration to the U.S.A. over the years 1947 to 1958.[11] Beyond this, it has been suggested that there may be an asymmetry in our *a priori* expectations about the way changes in unemployment might influence migration for which there there is no counterpart with regard to income. That is, that while increases or decreases in the rate of unemployment in the receiving country would always be expected, *ceteris paribus*, respectively to reduce or to increase the level of migration, it is not completely clear *a priori* that the converse would be true of changes in the level of unemployment in the sending country. This is because while a high rate of unemployment in that country might be expected to strengthen the *wish* to emigrate, it might reduce the *ability* to do so by making it harder to accumulate the fare. Hence while we should expect a negative coefficient on the unemployment variable related to the receiving country, it is not completely clear whether we should expect that on the sending country to be positive or negative. This seems a good reason for entering separate measures for each country, and it also seems to make more acceptable the interpretation of a coefficient of the appropriate sign as indicating 'push' and 'pull' than was the case with separate income measures.

Income and unemployment variables, or proxies therefore, have been the most frequently used explanators in econometric

[11] Fleischer, 1963.

studies of migration flows. The words 'or proxies therefore' perhaps deserve emphasis and some explanation, particularly in regard to the 'unemployment' variables, since the fact is that except for Norway there is hardly an acceptable direct measure of unemployment for any of the countries so far treated, sending or receiving, for any date prior to the late nineteenth century at the earliest. Quigley (1972) in fact offers this as one of his reasons for omitting an employment variable. In default of more direct indicators, some investigators have used some measure of output as a proxy for unemployment data. The suitability for the task of some of these proxies is open to serious doubt, and indeed the quality and the suitability of the data generally in many of these equations is a cause for concern and a possible partial explanation for some of the disappointing results.

How have these variables performed? By and large, the unemployment variables have undoubtedly been more successful than the income ones, in a variety of senses: they have yielded a higher proportion of significant coefficients, with signs conforming with *a priori* expectations at least as to employment in the receiving country, and have yielded more *consistent* results from study to study and from author to author. Generally, the level of unemployment in the receiving country has emerged as more significant, conformably with the point just made that the direction of influence of unemployment in the sending country is *a priori* ambiguous. Nevertheless, where the latter variable *has* yielded a significant result, it has been in the direction of suggesting that higher unemployment at home, on balance, does indeed promote rather than restrict emigration - a finding which is perhaps not, after all, surprising, in view of the ease with which the passage money could be raised by most would-be emigrants. Exceptions to these generalisations are the studies of Wilkinson (1970) and Richardson (1972). Wilkinson (1970), for most of his seven sending countries, finds the employment proxy (chiefly manufacturing production) in the sending country substantially more

significant than that in the U.S.A. (There is a direct conflict here with the findings of Gallaway and Vedder (1971), and this will be commented on shortly.) Richardson finds all unemployment variables (as well as all of the wage variables!) performing badly in his study of U.K. emigration to U.S.A., Canada, Australia and New Zealand.

An interesting point concerning the unemployment variable is that for Australia, for which a genuine if restricted unemployment series (as opposed to a proxy) is available for the nineteenth century, both Kelley and Pope obtained better results by using the *reciprocal* of the unemployment rate as the explanatory variable. The rationale proposed is that since the unemployment rate cannot be negative, a further reduction at very low rates may fail adequately to capture an increased buoyancy of demand for labour as expressed in job vacancies, opportunities for overtime earnings, and so on. Conversely, high rates of unemployment may conceal further "slack" in the form of underemployment. A further possibility, not advanced by Kelley or Pope, is that the deterrent effect of unemployment in the receiving country may reach a maximum at a level of unemployment which is not the highest actually recorded; 15 percent unemployment there may be just as discouraging as 20 percent.* Using the reciprocal, of course, appropriately magnifies and diminishes the variance of the variable at low and high levels of unemployment respectively.

On the whole, the income variables have fared rather less well. Wilkinson (1970) finds the income differential significant at the 95 percent level for the four countries for which he was able to employ it (Denmark, Sweden, Germany and the U.K.), though with a negligibly small elasticity for the last of these. Moe gets an even better result for Norway. Quigley, as already

* Unemployment in the receiving country would not be expected to have a large effect on the movement of dependants coming out to join a migrant already settled in the country. See below, pp. 641-642.

noted, uses sectoral wage levels (agriculture and industry) for both countries, and finds the coefficient on the wage variable significant at the 95 percent level on 12 of the 15 occasions on on which he uses his four formulations. Tomaske, in his cross-country analysis, finds the relative income level significant when the stock of previous migrants is also used as an explanator, but not when it is not.

By contrast Easterlin, in another cross-sectional analysis based on much the same countries as Tomaske but over a longer time period, finds that " the simple correlation between emigration and per capita income is low or negligible." [12] Kelley finds the rate of growth of income in both Australia and the U.K. statistically insignificant, and Pope finds the real wage differential insignificant as an explanator of U.K. migration to Australia, New Zealand and Canada alike. Richardson found only two wage variables significant at the 95 percent level and the sign of the coefficient of both of these conflicted with *a priori* expectations! Gallaway and Vedder, in contrast with Wilkinson (1970), found only a weak significance for U.K. wages and none for U.S. wages in explaining movement between those countries.

The migrant dispersion studies are also divided on this issue. Orsagh and Mooney find that the immigrant workers in the U.S.A., 1880-1920, were " no more sensitive to economic stimuli (i.e., wage differentials) in distributing themselves than the native-born American population." [13] Gallaway, Vedder and Shukla (1974) interpret this conclusion as compatible with that of their own study of the dispersion of the total immigrant flow of 1900, which includes the finding that the per capita income of the various states exerted a strong influence on immigrant locations. (That is, the " native counterparts " were highly responsive too.) On the other hand, the earlier study by Vedder

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[12] Easterlin, 1961, p. 336.

[13] This precis is by Gallaway, Vedder and Shukla, 1974, p. 225.

and Gallaway (1970) of the settlement preferences of Scandinavian immigrants to the U.S.A. in a number of benchmark years has revealed that not until 1900 was there a significant and positive elasticity of migrant location with respect to income. For 1850 and 1880 the elasticities were uniformly non-significant, and for 1850 most actually had the wrong sign. Yet, the distinctive geographical preference of the Scandinavian immigrants had become well established long before 1900.

It is not difficult to conceive several reasons for this equivocal performance of the income variable. In the first place, neither gross national (or state) product per head, nor even industrial wage rates, would necessarily represent at all accurately the earning opportunities for newly-arrived immigrants. Indeed, in the cross-sectional studies no single figure could possibly do so, given that (for whatever reason) there was such a wide disparity amongst the earnings rates of the different national groups in the U.S.A. Even the more appealing argument that such gross income measures should be an acceptable indicator of *changes* in the incomes actually attainable by migrants looks less compelling in the light of what is said below by way of explanation of the conflict between Wilkinson, and Gallaway and Vedder. Further, the hope of most immigrants was surely that they would be able to secure in the host country relatively better earnings opportunities, eventually if not immediately, than they could command at home, so that conceivably they might have moved even had overall per capita real GNP been identical in the two countries. Quite apart from all this, a migrant intending permanent settlement and seeking to maximise *lifetime* net earnings would be unlikely to be impressed by trivial short-term fluctuations of a few percentage points in the ratio between the average per capita incomes of the two countries concerned - even if he could perceive those fluctuations, a condition unlikely to be met.

Yet, as we have seen, some studies *do* find relative incomes a

significant determinant of migration. Is it that these studies have devised a better formulation permitting a more reliable test of the influence of the income differential? Inspection of the estimating equations suggests that this is not the case. However, what such an inspection does suggest as being possibly significant is that the variable has performed badly when a well-conceived measure of unemployment has also been included as an explanatory variable, and conversely. Thus the studies of Kelley, Gallaway and Vedder (1971), Richardson and Pope (1968) all find income of low or no significance, and all include one or two unemployment variables. By contrast the studies which find some (if sometimes qualified) support for the income variable, those by Quigley, Tomaske, and Wilkinson (1970), either include no employment variable at all or, in the case of the last author, use proxies which are of questionable acceptability and which perform only moderately well. The hypothesis which this suggests is that it was fluctuations in *employment* which were influential and which these models are "picking up"; but, where an employment variable as such is not entered in the equation, then the *income* variable(s), which are presumably to some extent correlated with employment fluctuations, "pick up" the effect of the latter.

This hypothesis (it is no more) receives some rather amusing confirmation when we compare the studies of Swedish migration to the U.S.A. by Wilkinson (1967) and Quigley respectively. Covering very much the same period, the two studies both develop migration equations which explain about three-quarters of the observed variation in Swedish migration to the U.S.A. Further, visual inspection of the graphs showing actual and predicted migration which each author presents suggests that the two predicted series are probably more like each other than either of them is like the actual series. Yet Wilkinson's equation includes employment but no income variables, and Quigley's equation income but no employment variables! Confronted

with such evidence, surely one must be cautious in the interpretations one places on these results?

The one study which seems inconsistent with the hypothesis sketched in the last two paragraphs is that of Moe. This author includes both income differential (real GNP per head of the labour force averaged over the five previous years) and unemployment (proxy only in the case of the U.S.A.) variables in his model explaining Norwegian migration to the U.S.A., 1873-1913. All these variables prove to be statistically significant, though the coefficient on the U.S. unemployment proxy is much larger than that on Norwegian unemployment, and the model as a whole explains over 87 percent of the observed variation. Conceivably this result may come about because the use of given-year employment data but of the average of the preceding five years for the income data more effectively separates these two than in some other models.

Again it may be related to an interesting and praiseworthy feature of Moe's model, which is, that the dependent variable measures *only* the emigration of persons in the " prime working ages ", and is standardised by expressing this as a *rate* of emigration per thousand of population in that age group. Moe is surely correct in claiming that his model — the comment would apply also to the models of most of the other writers — " makes sense *a priori* only when applied to this age-group rather than the total number of emigrants each year." [14] In other words, the movement of dependants joining a " primary " migrant already in the U.S.A. may be expected to be much less sensitive to small fluctuations in income or employment levels, and those analysts who have used *total* emigration as the dependent variable — which is to say, all of them except Moe — have probably suffered a dilution of the significance of their results as a consequence. Livi Bacci notes that in regard to Italian emigration, there was a strong

[14] Moe, 1970, p. 163.

negative correlation in the short term between total numbers of emigrants and the proportion of women and young children, confirming Moe's hypothesis.[15]

It is also interesting to note that when Moe further disaggregates his estimates to encompass the emigration of men and women separately, and then of the age-groups 15 to 29 and 30 to 44 separately, the following results emerge: men are considerably more "responsive" (as measured by the size of the regression coefficients) to both income differences and unemployment than are women (an unsurprising result, in view of the fact that male emigrants were much more likely to enter the labour force than female); and, the younger adult males are *much* more responsive to income differences, but rather less so to unemployment, than the 30-44 age group. Indeed, the income variable for the latter only just achieves significance at the 99 percent level and is the least impressive of all Moe's explanators.

Moe also charts predicted values of Norwegian emigration allowing each of his explanatory variables in turn to take their actual values, while holding the others constant at their means. The graphs of these four predicted values of EN suggest that the two unemployment variables in combination "explain" much of the short-term fluctuation of the (aggregate) predicted curve, while the other two variables, the income differential and a fourth variable, the relative number of Norwegians in the age group 20 to 29, combine in the main to explain the "long swing" components of the prediction, notably the peak of the early 1880s, the trough of the mid-90s, and the peak of the early 1900s.

Return for a moment to the direct conflict between the results of Gallaway and Vedder (1971) and of Wilkinson (1970) regarding U.K. migration to the U.S.A. Ignoring a further disagreement over lags, the chief issue is that while Wilkinson finds the income differential significant and employment oppor-

[15] Livi Bacci, 1961, pp. 18-19.

tunities not, the two former authors find employment opportunities in both areas " relatively strong " influences and wage levels relatively weak. Gallaway and Vedder surmise that the contrast between these results may arise because of the choice of an inappropriate statistical estimation technique by Wilkinson. This may be; but several alternative explanations lie to hand. The first is that, as already suggested, Wilkinson's choice of proxies for the missing employment data may have been unhappy, so that it is largely left to his income data to " pick up " the influence of fluctuating employment opportunities (with which the income data are correlated) and in doing so to increase the significance of his income variable. Secondly, the Gallaway-Vedder analysis may be vitiated by the use of passenger statistics for the dependent variable rather than " true " migration data. As is, or should be, well known, there was a particularly large flow of short-term visitors — businessmen, tourists, relatives visiting U.S. residents — between the United Kingdom and the U.S.A.; in some years about the turn of the century the total of U.K. passenger departures to U.S.A. was more than twice as large as the number of U.K. " immigrants " to the U.S.A. as recorded by U.S. immigration data, though the discrepancy was much smaller in earlier years.[16] Moreover, the figures of outward movements relate to the United Kingdom (that is, including Ireland), whereas Gallaway and Vedder's explanatory variables appear to relate to Great Britain only.[17] These two conceptual misalignments may result in some years in a value of the dependent variable larger than the " appropriate " one by a factor of at least four.

In the third place, and most importantly, Wilkinson uses national income data for his income variable whereas Gallaway and Vedder use the Phelps Brown and Hopkins real wage rate estimates. Between these two series there are two major differences.

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[16] With this in mind, the footnote on p. 891 of the Gallaway and Vedder study seems far too optimistic.

[17] This has already been pointed out by Ó Gráda: Ó Gráda, 1975, p. 149.

First, per capita national income data are of course national averages, while the wage rate data incorporate also the effects of changing income distribution - which, partly at least because of the impact of mass immigration, changed to the disadvantage of wage earners in the U.S.A. during this period. Secondly, the Phelps Brown estimates are corrected for changes in the price level, while the national income estimates are in current prices. Again, price movements probably *disfavoured* those on lower incomes in the U.S.A., relative to U.K., during this period. In short, both of these differences tell in the same direction, namely to place development of British incomes in a better light vis-à-vis the U.S.A. in this period when the *real wage rate*, as opposed to per capita national product, is the measure employed. This effect, apparently, also holds for other European countries in comparison with the United States. Thus, while U.S. product per head grew more rapidly than that of any European country save Sweden in the later nineteenth century and early twentieth century, *real wage rates* grew more rapidly in several European countries. The Phelps Brown-Hopkins data show a factor of multiplication of real wage rates over the interval 1860-1913 of 1.5 for the United States, as against 1.6 for France and also for Germany, 1.9 for the United Kingdom, and no less than 3-fold for Sweden.[18]

For the Gallaway-Vedder and Wilkinson studies these facts open up the possibility that the time-shapes of the two income variables employed may be much less similar than the first two writers supposed. And this is in fact the case. Calculating the ratio of U.S. to U.K. " income " for each of the years 1871 to 1914 first from national product (current price) data and then from the real wage rates series, I found the coefficient of determination between the two resulting series to be quite negligible

[18] Phelps Brown, 1950, p. 236. From data in Fuà (1969), I have calculated a factor of multiplication of real industrial wages in Italy, 1862-1913, of 2.15.

(about + 0.003). In effect, Gallaway and Vedder and Wilkinson have been using substantially unrelated data allegedly measuring the same thing. It should be added that in this respect Gallaway and Vedder (and Quigley, who also uses wage data) may well have chose more appropriately than scholars using national income data.

There is, in fact, a general need for greater care in the selection and appraisal of data. Even the careful Moe, whose excellent study is in most technical respects probably the best of those under consideration here, uses real GNP data for his major estimating equations — a variable showing a rising differential in favour of the U.S.A. — while tucked away in his appendices are data on Norwegian and American *wages* showing a sharp *narrowing* of differentials in the the later nineteenth century.[19] Williamson apparently uses for most of his analysis in *Late Nineteenth Century American Development* a " migration " series showing net additions to the foreign-born, which not only nets out the remigration of the foreign-born from the U.S.A., but also — and quite inappropriately from Williamson's point of view — the deaths of the foreign-born in America.[20] Kelley employs net immigration to Australia as his dependent variable, an inevitable choice, as only data on total movements, rather than long-term migration, are available for most of his period. However, he then interprets the significant showing of Australian unemployment as a " pull " factor, whereas, the ratio of departures to arrivals being very high, it might equally well be interpreted as a " push " variable, that is " pushing " departures from Australia to an even higher level than usual. There is abundant evidence that this in fact happened in many cases, for example from New Zealand in the late 1880s, from Australia and from Argentina in the early 1890s, or from the U.S.A. in the mid-1890s and in 1907-08.

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[19] Moe, 1970, Appendix Table B-1, p. 234.

[20] Williamson, 1974 (a), pp. 239-40; Kuznets and Rubin, 1954, table B-6, p. 102.

What is the overall conclusion generated by the econometric testing of these income and employment variables? There is quite a measure of recognition in the literature that while the income differential is the more important permanent explanator of migrant behaviour — it is hard to conceive migrants responding to long-term trends in relative unemployment, and of course the unemployment or underemployment characteristic of overpopulated rural areas will also show up as an income differential — employment conditions are the more important influence on short-term fluctuations in migration levels. It is then not surprising that in some of the studies the employment variables should, on the whole, have emerged as more significant, for where there is no pronounced upward or downward trend in the level of migration over time, as in the Swedish and Norwegian migration to the U.S.A. studied by Quigley and Moe, or the Australian immigration investigated by Kelley, the factors influencing the *short-term* fluctuations will dominate the equation. Further, we have seen that there are *a priori* reasons for thinking that migrants would not readily respond to, nor indeed perceive, relatively small changes in the income differential - even supposing that the income measures employed appropriaately measure this, which again as we have seen is in some cases distinctly questionable. The results of these studies need not, therefore, be regarded as casting any doubt on the proposition that the income differential was the underlying motive for migration, but neither can they be said to have done much to strengthen that proposition.

As for the " push-pull " controversy, the balance of argument favours the view that this dichotomy is a useful concept, if at all, only in the framework of the consideration of employment conditions, rather than of the income differential. In the former context there is some suggestion that employment conditions in the receiving country were more important than in the sending, but this is not a universal finding and in any event most studies,

by considering only the level of gross immigration, fail to notice that depressed conditions in the former "push" returning migrants home as well as buoyant conditions "pull" new migrants in.

The studies by Quigley and Wilkinson (1967), though their equations are differently formulated, agree in strongly confirming the older view of Dorothy Thomas that industrial conditions in Sweden were an important influence on the level of Swedish emigration to America. Conceptually, however, Swedish industry, given the predominantly rural origin of most Swedish emigrants, might more properly be regarded as a competing "pull" factor than a "push" one. The careful study by Moe suggests similar conclusions for Norway, again echoing those of a non-econometric investigator, Ingrid Semmingsen. Thus in this sense the terms "push" and "pull" appear to conflate geographical and causal concepts in an undesirable manner.

There is yet a further difficulty in evaluating the "push-pull" hypothesis. Quigley reasons that push and pull were of comparable importance since his estimated coefficient on pairs of U.S. and Swedish variables are of comparable size and significance.[21] Assuming that the variables are measured in the same units (since, of course, the size of any coefficient can be changed at will by changing the unit of measurement of the associated variable), this is an acceptable conclusion on one interpretation of what "push" and "pull" mean, namely that they refer to the elasticity of response to unit changes in certain factors in sending and receiving countries respectively. But an alternative view, and one suggested (*pace* Quigley) by a reading of Harry Jerome, is that the respective strengths of push and pull should refer to the proportion of the overall variation explained by changes in these factors; and this, of course, depends on the *variances* of the factors concerned as well as on the size of the coef-

[21] Quigley, 1972, p. 121.

ficients (elasticities) attached to them. Identical elasticities of response to (say) a one percent change in unemployment in both the U.S.A. and Sweden would still, on this interpretation of the terms, mean that " pull " dominated " push " *if* the variance of U.S. unemployment was much greater than that of unemployment in Sweden.

Kelley is aware of this point in his study on Australian net immigration, and claims to show the relatively greater importance of Australian unemployment as compared with British by working out the arithmetic. However, this demonstration is less than fully convincing because there happens to be a relatively large negative covariance between the two explanators, and uncertainty how to apportion this makes a firm pronouncement difficult. This is to restate, by a different route, the point already made, that where cycles in the two countries are to a substantial extent *inversely* correlated one cannot be sure whether it is the pull of prosperity in the one or the push of depression in the other which causes the movement.

Overall, then, the verdict of the econometric literature on the push-pull debate might well be " a draw ", subject to the qualification that the subject of the debate is so ill-defined and ambiguous that it may be preferable to regard it as a non-issue. This would seem a fair result, too, in view of the " line-up " of the studies, two of which end by opting for push; two or perhaps three favour pull; four are emphatic that both influences were at work, and to a substantial extent; and the remainder offer no conclusive opinion.

* * *

Before going on to discuss other possible explanatory variables explored in the econometric literature, it may be useful to insert a long aside at this point by way of commentary on the question: is it *plausible* that employment conditions in the receiv-

ing country could have played the role which many statistical and econometric studies propose? It has sometimes been complained that the notion that fluctuations in the level of U.S. economic activity determined the volume of migration in the short term cannot be sustained because of the sheer implausibility of the causal chain implied. Perhaps the clearest and most concise articulation of this view is to be found in the review by Professor C.F. Carter of Brinley Thomas' *Migration and Economic Growth*, to be found in the August 1955 issue of *Economic History Review*. Discussing Brinley Thomas' assertion that fluctuations in the quarterly figures of male immigrants followed with an average lag of two quarters behind ton-miles of freight carried by U.S. railroads, Carter (who apparently did not know that some thirty years earlier Harry Jerome had proposed a lag of as short as two *months*) objects:

> " But what can this mean? Can the news of rising activity in America have crossed the Atlantic, have found its way into thousands of peasant homes in Germany and Ireland and Scandinavia, have led to decisions that now is the time for a move, to the collection of the means for the voyage, the long journey to the port of embarkation, the sea voyage to America — all within half a year? It is surely most improbable that any causal connexion with so short a time lag can exist." [22]

Carter's objection seems eminently reasonable, and it is instructive, rather than merely pedantic, to demonstrate that in fact it is not as compelling as it seems at first glance.

In the first place one has to insist that there is no single indicator or indeed collection of indicators of economic activity which would be universally agreed to portray the movements of an economy through the business cycle. Nor, in any event, are some of the most favoured series available for the earlier decades of the nineteenth century. In his study of migration and

[22] " Economic History Review ", Second series, VIII, August 1955, p. 107.

business cycles, Jerome used as his indicators of American economic activity only the value of merchandise imports (from 1820), supplemented by the volume of pig iron production (from 1860), and then monthly values of an index of factory employment (of very restricted coverage) from 1890 to 1914. It is well known to students of business cycles that such indicators as these do not all fluctuate in parallel fashion at precisely the same time; because of their differing relationships to fluctuations in final or intermediate demand they lead or lag in front of or behind other indicators by varying amounts.

One therefore needs to ask precisely what it is to which potential migrants might be expected to respond? It seems highly unlikely, for example, that even the best-informed potential emigrant contemplating moving to the U.S.A. would have had the slightest knowledge of the contemporary level of U.S. merchandise imports or pig iron production, or that he would have regarded such information as relevant to his own prospects in America if he had known. Rather, the most likely factor to influence those contemplating migration might be advice received through letters from friends or relatives already in America as to the prospects of obtaining employment if, in fact, they sailed to the U.S.A. In this connection, students of the business cycle are well aware that the actual dismissal of factory employees is usually one of the last and most drastic steps to be taken by by a management concerned to cut costs during a period of depression. For instance, in his encyclopaedic study of business cycle indicators Geoffrey H. Moore points out that comparing a list of 26 statistical indicators of business activity with data on on manufacturing employment reveals that rate of gross accessions to manufacturing employment (ie. new hirings) and the average number of hours per week worked by employees in manufacturing industries lead the indicators by an average of four and six months respectively. By contrast, the overall unemployment rate is exactly coincident with the inverse of an index

of industrial production.[23] The reason for this is, of course, that managers refrain from taking on new workers before they start dismissing existing ones, and find it both easier and cheaper (because of the reduction of overtime payments) to reduce average hours of work than to lay men off. Such methods of reducing labour inputs have the further advantages of being easier to reverse when activity revives, of retaining the services of skilled and experienced workers, and of being more likely to avoid conflicts with trade unions.

On reflection it is easy to see that it is precisely such changes in management policy which would be most immediately obvious to immigrant workers already employed in manufacturing establishments. While these immigrants may have been reared in an environment far less sophisticated than the American industrial scene, there is every reason to believe that they were quickly able to develop the capacity to interpret changes in such things as new hiring rates or the average hours of work as pointers to the course of employment opportunities in the months ahead, and this is precisely the type of inference which they would be anxious to pass on to friends or relatives in Europe contemplating migration to the U.S.A., and which the latter would be anxious to receive. It is even easier to believe that such considerations would be eagerly studied by and influential in shaping the decision of the potential migrant when we bear in mind that the new immigrant typically arrived with minimal resources and thus needed to secure employment at the earliest possible moment. Moreover, because of the 'last in first out' principle both the recently-arrived immigrant who had succeeded in getting a job and the friend in Europe contemplating joining him would be acutely aware that migrants had a quite exceptional interest in buoyant employment conditions.

It is therefore likely that Carter both overestimates the dif-

[23] Moore, 1961, pp. 56-7.

ficulty of transmitting back to Europe information which would be likely to influence the decision of the potential migrant, and underestimates the time lag between such information becoming available and and its observed impact on recorded immigration totals. This lag would in fact include not only the period by which arrivals lagged behind the indicators used by writers such as Jerome, but also the substantial period by which these indicators, in turn, lagged behind the first changes in accession rates or in average weekly hours of work, the signs by which migrant employees in U.S. manufacturing activity would interpret the likely future course of employment opportunities.

Beyond all this it should be added that there is no need to suppose that anything like all potential migrants received or allowed themselves to be influenced by this type of information. While fluctuations in the level of net immigration to the U.S. were of course very substantial, even in the periods of the most depressed economic activity in the U.S.A. gross inflows were still very large. For example, a depression as severe as that of 1893-4 did not reduce the level of male immigration by more than about 40 percent below its trend level. It is a mistake therefore to suppose, as Carter seems to do, that fluctuations in migration in response to changes in American business conditions were brought about by migrants waiting, as it were, for positive good news which might lead them to the terminal decision to leave home. Rather the situation was that there was a constant and very large flow of migrants at all times, *some but far from all of whom* could be, and were, persuaded to defer their departure by the news of declining employment opportunities. These comments, of course, are not intended as a riposte to Carter alone; rather, they are relevant to any study which has used *proxies* such as an index of manufacturing activity for the unavailable U.S. unemployment data.

It is, however, true that from several points of view one could expect this responsiveness of the stream of potential emi-

grants to economic forecasts to have increased through time. Partly, the more migrants from any particular part of Europe were already in the U.S.A., the greater the likelihood that the appropriate information would be relayed back sufficiently quickly and to a large enough number of people in the homeland for an appreciable influence on migrants' decisions to result. Secondly, the quicker such news could reach Europe the less serious the difficulty which the shortness of the interval seems to pose. The completion of the first successful trans-Atlantic cable in 1866 and, more importantly, the great speeding up of postal communications made possible by the building of railways on both sides of the Atlantic and by the supersession of sail by steam on the Atlantic crossing would lead one to expect a prompter response from about 1870 than before that date; and this, indeed, is precisely what one finds. Jerome himself, one of the first and most influential champions of the view that the American business cycle determined the level of migration, agreed readily that not only were the 'lags' longer before about 1870 than afterwards, but that the correlation between volume of migration and level of U.S. activity was much closer for the later nineteenth and early twentieth centuries than for earlier decades. This is now generally recognised by all students of migration, including those of the econometric variety.

Finally, the migration of the later nineteenth and early twentieth centuries, as we shall see elsewhere, consisted to a much larger degree than did that of the early and mid-nineteenth century of those who did *not* intend permanent settlement in the U.S.A., but migrated for a finite period in search of wages which would enable them to save substantial sums and then return home. Such *Gastarbeiter*, as the Germans call them, clearly have a more vital interest in short - and medium-term economic prospects than have those contemplating a lifetime commitment to another country. Hence temporary migrants of this 'sojourner' variety both had a greater incentive to keep close watch

on the course of business activity and employment openings in the U.S.A., and could be expected to develop a more sophisticated understanding of changes which would help to provide a reliable forecast for the short or medium run in which they were interested.

* * *

Some of the other explanatory variables used in one or more studies may now be more briefly discussed. Three studies, by Tomaske, Easterlin, and Quigley, use demographic indicators in an attempt to test the theory that cycles in emigration were influenced by a cyclically fluctuating pressure on employment opportunities from changing numbers in a particular, emigration-prone age group. Tomaske and Easterlin use for this purpose the rate of natural increase twenty years earlier in each of their countries; Quigley uses the birth rate, lagged 26 years (the average age of Swedish adult emigrants.)

Easterlin's ranking procedures seem to show some positive relationship of lagged natural increase with rate of emigration for the countries of north and west Europe, but not amongst those of the south and east, and the overall relationship is slight. The fact that almost equally good results were found with a ten rather than a twenty year lag increases one's doubt about the significance of the results. Tomaske's results are even less convincing, while in Quigley's equations " there is only slight evidence that variation in birth rates or population pressures exerted an independent influence." [24]

However, one would ideally wish to test for this by including some measure of the relative numbers in the age groups particularly at risk, rather than by using a lagged measure of the rate of birth or of natural increase. The latter is a particularly suspect measure as it is hard to see how the number of deaths twenty

[24] Quigley, 1972, p. 121.

years earlier can have been relevant to the contemporary level of emigration. For Norway Moe is able to use a direct measure of the relative numbers in the 20-29 age group and finds this a significant explanator. (The variable is actually defined as the number of persons aged 20 to 30 living in Norway in year t-1 per 1,000 of those aged 15 to 44.) But Moe apart, the demographic cycle hypothesis receives relatively little support from the studies mentioned. However, the possibility in some studies of collinearity between the relative numbers of young adults and unemployment rates makes this conclusion somewhat tentative. It may be, that is, that the unemployment variable « picks up » much of the impact of changing numbers of labour force entrants via the pressure they exert on job opportunities.

The migrant dispersion studies, as already mentioned, permit the testing of a number of possible influences on migrant behaviour which cannot readily be accommodated in models of inter-country flows. Perhaps the most interesting paper is the study by Vedder and Gallaway [1970 (a)] of the settlement preferences of Scandinavian immigrants in the United States, based on U.S. Census figures of the foreign-born by States. Beyond the familiar income and job opportunity variables are included tests of the possible influence of distance from New York as " the leading port of entry "; State density of population (as a proxy for availability of land); and the tendencies to avoid Southern States, to settle in States with Scandinavian-like climates, and to settle in States with a port of entry - each of these three represented by a ' dummy ' variable. These additional variables must surely introduce a problem of collinearity: for example, there is evidently a great degree of overlap between the 12 States judged to have a Scandinavian-type climate and the ' non-Southern ' States of which they form a subset. Equally, *were* it true that New York was the leading port of entry throughout the period there would be an interrelation between the " distance from New York " and " tendency to settle in port States " variables. However,

for Norwegians, most numerous of the Scandinavian immigrants in the mid-century decades, New York was *not* at first the leading port of entry, as most of them came via Quebec and the Great Lakes-St. Lawrence waterway. From Quebec the ships returned with lumber for the U.K. market, and the route suited the overwhelming locational concentration of Norwegians in Wisconsin and, to a lesser extent, Illinois. These, rather than New York, were presumably the "port States" in which Vedder and Gallaway report Norwegians to have had a statistically significant tendency to settle in 1850, the first of their benchmark years.

To a degree, Vedder and Gallaway's ambitious equation is something of a steamhammer to crack a nut, at least as far concerns the Norwegians, the earliest and most locationally-concentrated of the Scandinavian immigrants. From the start there was an overwhelming concentration of Norwegians in Wisconsin and in three of its neighbouring States. In 1850 Wisconsin alone had about 70 percent of the Norwegian-born, with a further 19 percent in Illinois. By 1870 there had been some dispersion into the neighbouring States of Iowa and Minnesota, Wisconsin and Iowa accounting for two-thirds of the total and the four States combined for 92.3 percent, a shade more than in 1850. In 1890 Wisconsin and Iowa still accounted for 52 percent of the total, and the four States together for nearly 70 percent. Thereafter dispersion continued, but quite slowly.

It therefore seems legitimate to state that the outstanding feature of the locational preferences of Norwegian immigrants entering the U.S.A. in the later nineteenth century (the largest decadal inflow arrived in the 1880s) was the wish of later arrivals to settle close to their kinsfolk already established in the U.S.A. That said, the important thing is to know why the first generation of Norwegian immigrants settled where they did; and this it is possible to learn, in much richer detail and depth than could possibly be disclosed by a multiple regression equation, from the large number of letters and the emigrant advice books from the

pens of Norwegian emigrants of that time. The impression left by a reading of these sources is that the first generation of Norwegians came to America predominantly to farm, and that with this in mind their choice of location, given their lack of capital, was limited to the then "frontier" regions where free land was available. Within those, they chose locations with a climate as similar as possible to that to which they were accustomed. (Illinois, popular in the first years, soon lost ground as it developed a reputation as an "unhealthy" State.) Later arrivals simply joined communities where there were already substantial numbers of their compatriots, moving out from those communities, as necessary, to find vacant land.

Beyond these features, the early Norwegian-American sources are notable for the following characteristics: the very strong religious tone of the letters; the repeated emphasis, even on the part of those whose overall assessment of America was favourable — and these were not an overwhelming majority — on the need for hard work and a spirit of endurance if the emigrant was to make good; and the evidence provided that such concepts as "income differential" and "job opportunities" are conceptually ill-attuned to the motivations lying behind these earlier pioneer migrations.[25]

It is therefore not surprising to find that Vedder and Gallaway's model is not very successful in explaining the variation in the number of Norwegian settlers among States in 1850, the only variables significant even at the 95 percent level being the "tendency to avoid the American South" and the "tendency to settle in port States." The income variable does not become significant until 1900, and for 1850 actually has the wrong sign (a result according with the qualitative evidence). Further, one strongly suspects that when the income variable does become significant in 1900, it is not so much because the Norwegian-born

[25] On the motivations of the early Norwegian settlers see Blegen, 1955 and 1969.

have become markedly more responsive to the lure of high wages, as that by that time the changing distribution of income within the U.S.A. had elevated the North Central States in which Norwegians had long been principally established to a much higher relative position in respect of per capita income than at mid-century.[26]

The most interesting of the variables *other than* the income and employment ones is that representing last year's immigration or, sometimes, the cumulated total of previous immigration from the same country. $M_{(t-1)}$ is used in the equations of Wilkinson (1967), Wilkinson (1970), Kelley and Quigley to help explain this year's immigration, $M_{(t)}$, while Tomaske, whose estimation is based on inter-country comparison of decadal totals, uses R_i, the stock of previous migrants to the U.S. per 1,000 of the population of the *i*th country. *These "previous migration" data emerge as the most consistently significant variable in this whole body of literature.*

The rationale for the inclusion of some measure of previous migration is twofold. First, as just demonstrated for Norwegian migration to the U.S.A., there are substantial psychological benefits to the new arrival, and perhaps material ones too, in settling near kinsfolk or compatriots in an alien country, so that other things being equal the number of previous immigrants in any area from a given country or region will help determine the number and locational preference of later immigrants from that country or region. Secondly, in so far as the determinants of migration may operate with a lag rather than instantaneously, it is possible to use the number of previous migrants in order to "capture" the influence of these determinants in previous years. The first of these influences is most appropriately captured, presumably, by using R, the stock of the previous mi-

[26] R.A. Easterlin, "Regional Income Trends, 1840-1950", in *The Reinterpretation of American Economic History*, eds. R.W. Fogel and S.L. Engermann, Harper and Row, 1971.

grants, as an explanator, as the important feature here is the *number* of already-settled immigrants from a given region rather than how long they have been there. The second influence is best measured by entering $M_{(t-1)}$, the actual number of immigrants in the previous period, as an explanatory variable, which in effect says that migrants respond to a geometrically-declining average of previous economic conditions.

However, these two "influences" of previous migration are not *uniquely* captured by R and $M_{(t-1)}$ respectively, and it thus becomes a difficult matter to decide on the correct interpretation when a significant result for such a variable appears. It deserves to be emphasised that when R or $M_{(t-1)}$ is included as an explanatory variable an apparently impressive result may emerge even from an equation which is not in fact telling anything at all about the fundamental causes of migration. If migration between a given pair of countries follows a straight-line trend through time, whether rising, falling, or stationary, then $M_{(t-1)}$ plus a constant will *completely* explain $M_{(t)}$. Even where migration fluctuates from year to year about a constant trend, then $M_{(t-1)}$ will give a fair explanation if the periodicity of the fluctuations is not too short. This is exactly like the weather forecaster who throws meteorology into the waste-paper basket and predicts that today's weather will be much like yesterday's, a strategy which can be quite successful if the climate is not too changeable. For example, the equation

$$\widehat{M}^g_t = a + b\ (M^g_{(t-1)}),$$

which says that this year's emigration from Germany will be a constant multiple of last year's plus a constant, when estimated for the years 1875-1913, yields the result

$$\widehat{M}^g_t = 6{,}639 + (0.888)\ M^g_{(t-1)}$$

$$r^2 = 0.780$$

$$n = 39$$

- a highly significant result and one fully as good as most of those in the literature under survey in this essay.[27]

There is a moral to be drawn here, and remembered when interpreting some of these results. For example, in Tomaske's study the stock of previous migrants (R_i) is the only consistently significant explanator; and when it is omitted, the proportion of the variation explained ($\bar{R}^2$), which is better than 85 percent for all the equations in which R_i is included, falls for the 1880s to less than 30 percent - a level which is not statistically significant at the 95 percent level. The result for the 1890s is substantially worse even than this. A slightly uncharitable critic would say that all that Tomaske's equations are telling us is that the rates of migration from various countries of origin to the U.S.A. in the 1880s and the 1890s were determined by much the same factors as in earlier decades - only we don't know what they were!

Where, however, the "previous migration" variable is not included it is likely that the estimated coefficients of the explanators actually used will be biased upwards. The reason for this is as follows. Migration along a particular path in any one period is positively influenced by the volume of previous migration along the same path: this is the unanimous verdict of all students of migration, of whatever persuasion. If a variable representing the volume of previous migration is not included, then that influence will tend instead to "attach itself" to any other variable or variables positively related with the volume of previous migration. If, then, the remaining determinants of movement along that migration path — the income differential, and so on — are reasonably constant over time, these variables

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[27] Orsagh and Mooney noted in 1970 that their R_{-1} variable "alone does almost as well as the whole set of independent supply variables in explaining the relative geographical distribution of our migrant labour force." (Orsagh and Mooney, 1970, p. 312.) However, it will be apparent from the text that I cannot join Orsagh and Mooney in interpreting this as necessarily "very convincing proof of the 'friends and relatives' hypothesis".

will to some extent act as proxies for the missing " previous migration " variable. The income differential variable, for example, will not only latch on to the effect of that differential in *directly* motivating this this year's migration, but will also respond to the influence of the pool of previous migration which was in part determined by the values of that same variable in earlier years.

In their latest paper, written in collaboration with Shukla, Gallaway and Vedder acknowledge this dilemma: entering the " previous migration " variable will bias the coefficients of the other variables downwards (as well as introduce collinearity problems), while omitting it may bias those coefficients upwards. As they comment: " We seem to be ' damned if we do and damned if we don't '." [28] Their solution is to employ a two-stage estimating scheme, first regressing the " previous migrants " variable against the other independent variables and then entering the *residuals* left by that equation as the explanatory variable in the final estimating equation. Their reasoning is that this procedure will purge their equation of multicollinearity by first getting rid of the indirect influence of the other explanators on the stock of previous migrants, leaving any *independent* influence of that stock to be measured by the " residuals " variable. The results suggest strongly that the stock of previous migrants does indeed exert such an independent influence - a result fully in accord with other types of evidence.

It may well be that in Vedder and Gallaway's 1970 study of the settlement preferences of Scandinavian immigrants to the U.S.A. failure to include a " previous migrants " variable imparts a progressively increasing upward bias to the coefficients of the variables actually employed. This might help to explain why several of those variables tend to become more significant, and why the proportion of the total variation explained increases,

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[28] Gallaway, Vedder and Shukla, 1974, p. 221.

with each successive benchmark year at least up to 1900. Thereafter, with new immigration declining sharply after about 1910, and with the Scandinavian-born already resident in the U.S.A. moving about in search of new opportunities, the influence of the location of earlier immigrants does indeed tend to decline.*

* * *

So far, one of the eighteen studies mentioned earlier in this essay, that is Williamson's 1974 paper, has attracted only passing reference. Williamson's paper stands on its own by virtue of its more searching economic analysis and its more ambitious goals, as compared with the studies already reviewed. Indeed, the culmination of his study is that, having empirically estimat-

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* Since writing these paragraphs a number of recent papers by Dunlevy and Gemery directly addressing these issues have come to my notice: see, in particular, J.A. DUNLEVY and H.A. GEMERY, *The Role of Migrants Stock and Lagged Migration in the Settlement Patterns of Nineteenth Century Immigrants*, "Review of Economics and Statistics", May 1977, pp. 137-144.

The authors are concerned to clarify the role in estimating equations of variables measuring previous migration along a given route. They do this, in the paper cited, by estimating several versions of an equation designed to explore the determinants of the intended destinations (by State) of 19 nationalities of U.S. immigrants of the year 1898. The equation is first specified without any measure of previous migration of each nationality to the various States: then, sequentially, with measures of the stock of previous immigrants surviving to the 1900 Census in each State and of the flow of immigrants in 1897 to such States, at first separately, and then together.

The results are interpreted by Dunlevy and Gemery as suggesting that the *stock* variable and the *flow* variable measure different things, the first the "friends and relatives" effect and the second the lagged influence of the other explanatory variables in earlier years — though it is argued that when only one of these variables is included it "picks up" some of the effects of the other — and that *both* should be included in a properly specified equation. Conformably with earlier evidence and with *a priori* expectations, the r^2's rise when the previous migration variables are included, but the parameter estimates of the other explanatory variables and their level of statistical significance decline — very markedly so when *both* previous migration measures are included. The authors comment that "the combination of migrant stock and lagged migration acts as a dominant variable."

It should be added that while the results of Dunlevy and Gemery's analysis are neither unexpected nor *prima facie* unreasonable, it is far from clear to the writer that their model formulations and estimation technique guarantee the removal of the collinearity problem around which the issues in question largely revolve.

ed the elasticity of response of European migrants to American employment conditions, and also the strength of "push" influences in Europe, he is then able to introduce migration as an endogenous variable into the neoclassical model of the U.S. economy which is the central feature of his book on late nineteenth-century American development. By doing so, Williamson is able to furnish answers to such hypothetical questions as: How would America have developed in the period 1870-1914 had push conditions in Europe remained unchanged? and, How would immigration have been influenced had the "frontier" not disappeared in the late nineteenth century, or had it already ceased to exist in 1870?

These problems fall outside the range of this essay. But a number of features of the analysis which leads Williamson towards the position from which he can tackle them fall to be mentioned. First, he makes a criticism of all previous econometric studies of European migration to the U.S.A., namely that they use a single equation system to investigate an interactive structure. Williamson asserts, following Easterlin, that between 1850 and 1914 Europe lost one quarter of its labour force to the New World, while from 1870 to 1910 one third of the increase in the American labour force came from immigration. In these circumstances, he complains, it "is a strong assumption indeed" to proceed as if "trans-Atlantic migration had no effect on employment and wage conditions in either country." In Williamson's view, the volume of migration is best viewed as an international redistribution of labour between sectors of an integrated (if imperfectly so) market, constantly adjusting the respective supplies of labour towards those values which, given the respective *demands* for labour, would bring about the equilibrium wage differential (which need not be, and was not, zero). The achievement of this equilibrium is not hypothesised to have been instantaneous; instead, a simple "adjustment" model is incorporated to allow for lags in response and the like. Starting from labour

demand and supply equations for each country, Williamson shows that a single estimating equation for the level of migration between any pair of countries should *not* include income (or unemployment) data, but should use as its explanators only the level of output and the labour supply in each country, the latter measure incorporating emigration to and immigration from third countries. From this point of view, Williamson finds fault with all previous studies except those of Easterlin and Wilkinson (1967.)

A further consequence of the need to view migration as part of an interacting system is that it creates difficulties in interpreting the econometric results in terms of "push" and "pull". For example, a high coefficient on output in the receiving country need not indicate a high elasticity of response on the part of Europeans to American economic conditions. Rather, it may signify a low elasticity of labour demand to wages in America (that is, it takes a large inflow of workers from Europe to restore equilibrium because American wage rates fall only slowly under the pressure of the increasing labour supply.)

Williamson's argument is tautly presented and not easily summarised, so that by and large it must be left to the individual reader to explore it at first hand. Certainly his approach may be criticised; in particular, it will not appeal to those who find the neo-classical general equilibrium analysis an inappropriate framework for studying the sweep of nineteenth-century history. And in fairness to other analysts, it must be said that if the proof of the pudding is in the eating, then Williamson's culinary skill is not all that impressively attested. Estimating his reduced form, "output-plus-labour-supply-only" equation for four countries, the results are in almost all respects inferior to those of most other investigators, whether we have regard to the proportion of the total variation explained, the level of significance of the estimated coefficients and their conformity with *a priori* expectations, or the values of the Durbin-Watson statistic.

Perhaps these disappointing results arise in part because Williamson, curiously enough, seems less concerned than other writers to experiment with modifications which might produce better ones. However, this modest outcome does not prevent a measure of agreement on some points between Williamson and earlier writers. For example, Swedish emigration is shown to have been heavily influenced by the level of Swedish industrial output, confirming the earlier conclusion of Dorothy Thomas, Wilkinson (1967) and (by implication) Quigley. The explanation of U.K. migration to the U.S.A., on the other hand, is somewhat unimpressive, not merely in that Williamson's equation leaves more than half of the total variation unexplained — this is true also of his results for Denmark and Germany — but because for the U.K. alone two of the five estimated coefficients, those on U.K. output and on the U.K. labour supply, have the sign contradicting *a priori* expectations. Here again, Williamson's results are foreshadowed to some extent in earlier studies which also failed to agree on an "explanation" of British emigration. One recalls the direct conflict between the results of Wilkinson (1970) and of Gallaway and Vedder (1971) on U.K. migration to the U.S.A.; the fact that Wilkinson himself found the U.K. elasticity of response significant only with respect to the income differential, and then only with a very low value; and the fact that Richardson found hardly any significant "explanation" at all for U.K. emigration levels, save in regard to the close link with capital export. It seems warranted to conclude that the array of explanators usually considered in econometric studies is far more capable of generating an acceptable "explanation" of Swedish emigration to the U.S.A. than of British, and one wonders what the unconsidered determinants of the latter might be. Perhaps insufficient account has been taken of the fact that British migrants faced more significant alternative destinations (to the Empire) than did the Swedes; or perhaps the relatively high level of skill of British emigrants,

the 'special relationship' with the U.S.A., and the common language rendered British migration to America less sensitive to fluctuations in the variables which determined that from some other countries.

The chief interest of Williamson's study, however, lies for us not in his analysis of the determinants of migration to the U.S.A. from Denmark, Sweden, Germany and the U.K., but in the bold enterprise in which, having derived an elasticity of response to American wage rates for each of these four countries, Williamson uses a weighted average of these elasticities to "decompose" the recorded change in European emigration to the U.S.A. as between 1870 and the annual average for 1904-07 into the effects of U.S. "pull" and European "push". For the four countries of Northwest Europe used to estimate these elasticities, Williamson finds that the actual decline of 85,600 (net) migrants came about as the result of a huge decline in European "push" (— 169,000) overwhelming a more modest increase in U.S. "pull" (+ 83,400). Applying the same weighted elasticity of response to *all* European emigration to the U.S.A., it still appears that the effects of European "push" on the change in migration levels over the same period was negative, though it appears to be a legitimate inference — Williamson does not explicitly report on this — that there was some *increase* in "push" from the countries of Central and Southern Europe.

"Bold enterprise" is perhaps an appropriate description for Williamson's procedure, since to use a migration elasticity derived from equations which "explain", generally speaking, less than 50 percent of the variation for the four Northwest European countries, is itself optimistic. To go on to use the same elasticity for the very different countries of Central and Southern Europe whose migration has not been studied at all is perhaps rather more than optimistic, though Williamson points out that it would need a very large error in the estimated migration elasticity to reverse the results (that is, to make the change in Euro-

pean push positive rather than negative). In any event, the result is worth reporting not just because of the uniqueness, at least among this collection of studies, of the methods employed, but because in very broad terms Williamson's conclusion here is in accord with that tentatively advanced in these essays. Emigration from many parts of North and West Europe to the U.S.A. had passed its peak well before World War One; and in the view taken here, this was because the *relative* rise of wage incomes and the multiplication of jobs in the industrial areas of Europe provided stronger and stronger competition to the U.S.A. within Europe. Not merely did migration from these parts of Europe to U.S.A. decline, absolutely and of course still more as a proportion of the population of the sending countries, but some of the more highly industrialised parts of the Continent became, overall, areas of net *im*migration. Switzerland reached this situation about 1888 and the western, industrialised, States of Germany followed in the 1890s.[29] Only the continued heavy outflow from the agricultural regions of East Germany left the Empire, on balance, a country of continuing though much reduced outmigration on the eve of World War One. What is even more interesting (and less well known) is that before 1914 this same transition was also beginning to become apparent at least in one area of the *New* immigration, namely the Po valley in Northern Italy. (See Essay 4.)

From this point of view, then, mass emigration looks like the outcome of a race between on the one hand a pressure of numbers which in the nineteenth century can for the first time find relief elsewhere in emigration to other continents, and on the other the growth of adequate, and adequately rewarded, employment opportunities at home. In this connection it is interesting to mention a hypothesis regarding Sweden tested by

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[29] Bickel, 1947, p. 5; Thomas, 1958, p. 212. In the years immediately before World War One the foreign-born constituted a higher proportion of the population of Switzerland than of U.S.A.; BICKEL, *op. cit.*, p. 166.

Quigley. The hypothesis was first stated by Sundbärg, the outstanding Swedish demographer who shortly before World War One made the first great collection of international migration statistics. Sundbärg's contention was that if Sweden's industrial development in the nineteenth century had started just twenty years earlier, Swedish emigration would have been negligible. The hypothesis is *prima facie* plausible because of Sweden's very rapid growth in the later nineteenth century - per capita real product rose by an average of 2.3 percent per annum over the years 1870-1913, a rate which implies an increase of almost 60 percent in twenty years. Quigley tests this hypothesis by inserting into his estimated equation the (t + 20) values for the Swedish variables, and finds Sundbärg's contention broadly confirmed.[30]

* * *

In conclusion a few points may be briefly restated, and a few new ones made, by way of overall assessment of the econometric literature reviewed in this essay. By and large reaction to this literature must be one of some disappointment, for not only has it failed to generate important new insights — this in itself is not a crippling criticism, as there may be no important new insights to be achieved — but it has had only limited success in confirming or denying old interpretations. The conclusion which perhaps emerges most strongly concerns the clear influence of conditions in America, especially in manufacturing industry, and especially after about 1870, on short-term fluctuations in U.S. immigration levels. At the same time there is also considerable, though less unanimous, support for the view that conditions in Europe had a strong influence on emigration levels in the short term (as they certainly had in the long). This emerges particularly clearly for Sweden and also for Norway, but less so for Britain.

[30] Quigley, 1972, pp. 125-6.

This last divergence is one of the clearer indications so far to emerge from the econometric studies that the whole of Europe was *not* a single, undifferentiated reservoir of potential migrants from which the United States sucked immigrants regardless of local conditions. "Traditional" historians of emigration have never been in any doubt about this, but the mistaken view referred to has been encouraged and propagated by the U.S.-centric and highly aggregative nature of most of the statistical and quantitative work to date. As econometric studies are extended to the countries of South and East Europe and to migrations to destinations other than the U.S.A. one may hope that the true position will become more widely understood.

However, the limits of the econometric approach will continue to be severe as long as this is confined to studies aggregated at the *national* level, for there is overwhelming evidence that for a deep understanding of the process of emigration there is a need for a *regional* and, in some cases, an *occupational* focus. In this regard the lack of data on *regional* income and employment levels in Europe precludes, for the moment, a rigorous and full econometric treatment at the sub-national level. However, in regard to the migration data themselves there is no reason to rest content with series to be found in the records of receiving countries which merely record annual total movements (often in only one direction) between pairs of countries. The superiority of receiving country data has been too readily assumed, as argued elsewhere, even in regard to these gross aggregates. But when disaggregation, except perhaps by sex, is the aim, then one *must* go to the sending country data which, at least for a few countries, provide much richer information. Further, in a few cases comparable data appear also to be available for internal migration, and this, as in Sweden, may permit the study of migration across the national boundaries to be integrated with that of internal migration. Inability to command such a focus has been one of the crippling deficiencies of quantitative studies of national

emigration to date, and the " frontier " of quantitative research into migration, at least in Scandinavia where the data appear to be best, is now in this geographically more local but conceptually more comprehensive study of mobility, rather than in the further pursuit of additional highly-aggregated national studies.

Finally, lest it may be thought that this overall assessment of the econometric literature lacks charity, having regard to the quite impressive levels of " explanation " achieved by some of the equations, the reader should be reminded how easy it is to achieve high R^2s in the quantitative study of nineteenth-century history. A simple correlation between $M_{(t)}$ and $M_{(t-1)}$ achieves impressive results, as demonstrated above, and yet it offers much less conclusive proof of the *causal* significance of earlier migration along any given route than " traditional " accounts of the process of " chain " migration. Again, where there is no pronounced trend in migration volumes, a measure of (or good proxy for) unemployment in the receiving contry will do most of the " explaining " by picking up the short-term fluctuations, even though it has nothing to say about the underlying causes. And if there *is* a pronounced trend, upwards or downwards, as in (say) Italian emigration to the United States, then the inclusion of any other variable with a significant upward or downward time trend will do the trick. For instance, a regression of that emigration over the years 1876-1913 on telegrams despatched in Sweden * — a pair of variables between which some causal relationship could doubtless be perceived by some dogged Ph.D. candidate — yields an r^2 of 0.738 which, with $n = 38$, is a highly significant result.

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* As recorded in Table G8, p. 657, in B.R. Mitchell, *European Historical Statistics 1750-1970*, London, Macmillan, 1975.

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* *Note*: This *Key to references* services the footnotes and text of all the essays.

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