ESSAYS ON THE MOBILITY OF GOODS AND PEOPLE

by

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M.Acc., University of Waterloo, 1988
B.A., University of Waterloo, 1988

A THESIS SUBMITTED IN PARTIAL FULFILLMENT OF

THE REQUIREMENTS FOR THE DEGREE OF

DOCTOR OF PHILOSOPHY

in

THE FACULTY OF GRADUATE STUDIES

(Commerce and Business Administration)

We accept this thesis as conforming
to the required standard

THE UNIVERSITY OF BRITISH COLUMBIA

October 2000

Department of Faculty of Commerce and Business Administration

The University of British Columbia
Vancouver, Canada

Date Oct 13 / 2000

The University of British Columbia

Abstract

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This thesis comprises three essays on the international movement of merchandise and people.

The first essay measures the effects of foreign aid flows on a donor's merchandise exports. On average, donor countries tie approximately 50% of their foreign aid to exports, but the export stimulation of aid may exceed the amount that is directly tied. This essay uses the gravity model of trade to statistically test the link between aid and export expansion. The results suggest that aid is associated with an increase in exports of goods amounting to 120% of the aid. The essay also makes comparisons among donors and finds that Japan, which has drawn harsh criticism for using aid to gain unfair trade advantages, derives less merchandise exports from aid than the average donor.

The second essay investigates the effects of immigration on Canada's pattern of trade. I derive three alternative functional forms capturing the relationship between immigration and trade based on the proposition that immigrants use their superior "market intelligence" to exploit new trade opportunities. I then employ province-level trade data with over 150 trading partners to identify immigrant effects and obtain results suggesting that immigrants account for over 10% of Canada's exports.

The third essay addresses the question of whether tax differences contribute toward the brain drain from Canada to the U.S. This essay tests whether the U.S.'s lower taxes draw Canadians

south by examining a sample of Canadians living in Canada and a sample of Canadians living in the U.S. Using information from these samples I estimate how much these individuals would earn in the opposite country and estimate the taxes they would pay. I find that the people who have the most to gain in income and in tax-savings are the most likely to choose to live in the U.S., and thus corroborate the claim that tax differences contribute toward Canada's brain drain.

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PREFACE

An earlier version of chapter two of this thesis appears as paper # 98-21 of the RIIM (Research on Immigration and Integration in the Metropolis) series of working papers. The paper is jointly authored with Keith Head and John Ries. However, the first version of that paper was solely authored by me. Subsequent revisions included valuable contributions from Dr. Head and Dr. Ries, who are on my thesis committee. However, they have acknowledged that I contributed the majority of the substance of the paper.

ACKNOWLEDGEMENTS

I owe a great deal of thanks to Keith Head, my thesis advisor, and to John Ries, with whom I also worked closely. Both of them gave excellent advice, and my respect for them grew the more I worked with them. I would also like to thank the other members of my advisory committee, Jim Brander and Amin Mawani, who also added valuable advice, assistance and encouragement. In addition, I would like to thank my fellow students who have helped me with advice and encouragement. Students who have been most helpful are Paul Chewlos, Jennifer Cliff, Lorenzo Garlappi, Dora Lau, Olaf Rieck and Sanjeev Swami. I would also like to thank some other friends who have been especially encouraging – Jackson Clelland, Howard Kroon and Soori Sivakumaran. Finally, I owe much thanks to my family, which has supported me with encouragement, understanding and prayers.

INTRODUCTION

This thesis comprises three empirical essays on the international movement of goods and people. In the first essay I measure the role that foreign aid plays on donors' merchandise exports to recipient countries. The second essay reports a similar measurement of the link between immigrants and the trade of provinces. The third paper examines whether tax differences between Canada and the U.S. contribute towards Canada's brain drain to the U.S.

The first essay uses the gravity model of trade to measure the effect of countries' foreign aid on merchandise exports. To provide guidance on the links between aid and trade, analysts typically use tying percentages reported by donor countries. However, many observers believe that tied aid figures do not capture the full amount of trade triggered by foreign aid programs. This study provides a more comprehensive measurement of aid-triggered merchandise trade by measuring aid's effect on *total* trade of goods.

The statistical tests show that aid generates goods exports that exceed the amount of aid. This outcome implies that encouraging their governments to give aid may be an effective way for businesses to expand their exports.

Besides the policy implications of these measurements, this essay also introduces a useful method of accounting for unmeasured factors that could affect both aid levels and trade levels. The study uses residuals from import regressions to estimate these unmeasured factors. The import residuals are a statistically significant explanatory variable for export levels, and provide much greater assurance that the results of these tests are not attributable to unmeasured factors.

The second essay uses the gravity model to measure the degree to which Canadian provinces' trade with foreign countries is related to immigrants from those countries. This measurement is useful because it provides information on one facet of the economic value of Canada's immigrants.

Head & Ries (1998) carried out a similar test at the national level and found a substantial correlation between the number of Canada's immigrants from various countries and Canada's bilateral export and import levels with those countries. However, there is a risk that their results may be driven by unmeasured factors that affect both immigration and trade levels. This essay reexamines this question with province-level data, so that the statistical tests can use fixed country effects to reduce the risk that such unmeasured factors distort the results. In addition, this essay adds a language variable to test whether immigrants' main advantage is their knowledge of the foreign country's language. Moreover, this essay derives and tests several alternative models that could explain the immigration effect.

The most successful models are based on the theory that immigrants can expand trade because they have connections in the business community of their countries of origin. This outcome suggests that when businesses recruit people to expand trade, they should look for individuals who mostly likely have network connections. That is, they should prefer first-generation immigrants who had been involved in the business community of their former home countries.

The third essay addresses a topic that has received much media attention recently – whether taxes contribute to Canada's brain drain to the U.S. This essay examines the issue by looking at the location choice of a sample of Canadians – some of whom live in Canada and some of whom live in the U.S. I have data on each household's income and taxes, as well as other demographic characteristics. Using these data, I estimate the income and tax levels the household would realize if it lived in the opposite country. I then test whether the households with the most to gain in income or tax savings are the most likely to choose the U.S.

This essay finds that tax effects do influence people's decision on where to live. However, their responsiveness to both income and tax differences is nevertheless small. People tend not to move despite substantial economic gains that they can obtain from moving. This outcome explains why businesses find it costly to transfer people to new locations. It also implies that Canadian firms often need not match rival U.S. salaries or equalize their Canadian employees' taxes to U.S. levels, since most Canadians are unlikely to move.

The first two essays are similar in that they use the gravity model, and also that they consider factors that affect trade levels. Both essays posit that lowered trading barriers and trading costs (resulting from aid in the first essay, and resulting from immigration in the second essay) increase trade levels. The second and third essays look at issues concerning the movement of people. One of these essays examines a *result* of migration – trade pattern effects – and the other examines one of the *causes* of migration – tax differences between countries. Despite some common ground between these essays, each one can be read as an independent empirical study.

CHAPTER 1: AID AND TRADE – AN EMPIRICAL STUDY

1.1 INTRODUCTION

Each year developed countries give about $50 billion of bilateral aid to developing countries. Donors make extensive use of their aid programs to promote their own exports. About 50% of all bilateral aid is tied or partially tied to exports by formal agreement, and donors' aid programs may be generating exports in other ways as well. Donor governments defend their practices by claiming that they would be unable to maintain domestic support for spending on aid without some benefit accruing back to the taxpayers. If aid budgets depend on exports gains, then surely it would be useful to have a measurement of those gains. A measurement of trade gains is particularly lacking for Japan, which is reputed to be unusually mercantilist in using its aid program to secure exports, even though Japan's tying percentage is well below the donor average.

This paper employs a gravity model to predict *total* exports of merchandise from a donor to a recipient, with foreign aid included among the independent variables. Rather than measuring only exports directly linked to aid-financed projects, this paper's method should also capture any indirect effects that aid exerts on export levels. The paper first measures the typical aid-trade link amongst all donors, and then makes separate measurements for Japan and the other donors.

The results of this paper should interest donor governments making decisions on foreign aid policy. The need for this information is probably most apparent in the United States, where the absence of empirical evidence has forced policy-makers to guess at the links between aid and trade. The arguments some senators make to urge their colleagues to support continued aid, are revealing. For example, in 1994 senators supporting aid contended, "though some amount of humanitarian, altruistic spending is included, almost all of the $13-plus billion in spending in this [foreign aid] bill is designed to serve U.S. security and economic interests. ... Though foreign operations appropriations bills are called foreign aid bills, a more appropriate title would be domestic assistance bills."[1] In contrast, senators seeking to reduce foreign aid contend that foreign aid is money "squandered" and that the U.S. government ought to cut "spending which does not even benefit Americans, but benefits people in foreign lands."[2]

A measurement of the aid-trade link can also inform discussions on the trade practices of donors. The Americans often criticize Japan for using "aid as a predatory instrument for unfair commercial gain."[3] Until the early 1980s Japan used to tie most of its aid, but in the 1980s Japan has reduced its tied aid percentage substantially – to 27% in 1992. Yet this reduction has not silenced the critics, who maintain that the tying percentage does not really capture the true trade benefits of aid. "Despite considerable progress, suspicion lingers that Japan's *de jure* untying has not been translated into *de facto* untying. ... A common and strongly held impression in business circles, as well as the international development community is that Tokyo is unusually systematic and aggressive in promoting exports through aid."[4]

This paper is the first to measure exports associated with aid for all major donors. Most other studies focus on a single donor country, and generally focus on procurement associated with projects financed by aid money. Jepma (1994) provides a summary of these studies, and concludes that procurement from a country amounts to about 50 to 80 percent of the amount of aid.[5] These tests, however, do not necessarily measure the full amount of exports created by aid, since aid may lead to exports for projects not directly financed by

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[1] Republican Policy Committee (Don Nickles, Chairman), Senate Record Vote Analysis, 103rd Congress, 2nd Session, July 15, 1994, page S-9089 Temp Record, Vote No. 203. (Access on the web through http://www.senate.gov/~rpc/rva//home.htm.)

[2] Republican Policy Committee (Don Nickles, Chairman), Senate Record Vote Analysis, 102nd Congress, 2nd Session, Sept. 30, 1992, page S-15819 Temp Record, Vote No. 252.

[3] Bloch, p. 71. Similar descriptions of criticisms against Japan can be found in Preeg (Trade, Aid & Capital) p.179, Yanigihara & Emig, p.51-52, Preeg (Comment) p.112-115, Preeg (Tied Aid Credit Issue) p.17, and *The Economist* - May 7/94 p.20.

[4] Yanigihara and Emig, p 53. Similar arguments can be found in Preeg (Trade, Aid and Capital) p. 178-179, and Bloch p. 72.

[5] Jepma, 1994, p 12-13.

the aid.

A recent paper by Nilsson (1998) uses the gravity model to examine the links between aid and exports of goods from the European Union. Nilsson computes an elasticity of aid and uses this estimate to infer that the average EU donor derives $2.60 of exports per dollar of aid. He also repeats this computation for each EU donor to identify which donors derive the most exports per dollar of aid.

My paper's comparison of donors also corresponds rather well with the literature that seeks to explain donors' aid patterns. An interesting paper by Alesina and Dollar (1998) examines the degree to which giving patterns can be explained by various altruistic or self-interest factors. Their explanatory variables include measures for colonial ties, the recipient's income per capita, the recipient's level of democracy, and friendship. They find that self-interest factors explain much of the giving patterns, and they find substantial differences amongst donors.

This paper also introduces a new method for handling missing variables that affect both the independent and dependent variables in a gravity model. Failure to account for factors that affect both aid levels and export levels could lead one to over-attribute export levels to foreign aid. While some potential variables can be identified and controlled for, such as language, other relationships, such as political ties, are difficult to measure and still other relationships may not be identified at all. To address this problem I use residuals from a gravity regression on *imports* from recipients to donors to serve as a proxy for special trading relationships pertaining to exports from donors to recipients.

My paper is organized as follows. Section 1.2 introduces some terminology used in international aid, and provides a brief overview of the aid process. Section 1.3 posits some theoretical reasons why aid might increase trade by more than the tied amount. The section identifies potential direct and indirect effects on trade. Direct effects include any exports won on projects financed by aid, regardless of whether the aid is officially tied; indirect effects include any other ways in which aid can give the donor's exporters an advantage in the recipient's market. Section 1.4 outlines the general structure of the gravity model used in the statistical tests before introducing aid. Sections 1.5 and 1.6 outline the specifications and report the results of my first tests, which look at links between exports and aid provided in the same year. Section 1.7 expands the tests to consider prior years' aid, to determine how much the trade benefits of aid persist after the year the aid is disbursed. Section 1.8 isolates the aid/trade relationship for Japan, to see whether criticism of Japan's aid practices is supportable. The section also summarizes the results of tests for all the other donors. Finally, section 1.9 concludes.

1.2 AID PRACTICES AND DEFINITIONS

In 1961 the major donor countries organized the Development Assistance Committee (DAC). DAC now has 21 member countries.[6] The members of this committee have agreed upon various standards on giving aid, and on reporting practices. The OECD gathers the reported data.

About 81% of foreign aid is provided bilaterally, by a specific donor to a specific recipient. The remaining 19% is provided multilaterally through international agencies such as the World Bank or the IMF.[7] My statistical tests consider only bilateral aid.

Official development aid (ODA) can either be grants or loans. To qualify as ODA, a transaction must meet the following tests: (1) it is administered with the promotion of economic development and welfare of developing countries as its main objective, and (2) it is concessional in character, with a grant element of

[6]The 21 DAC member countries are Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Ireland, Italy, Japan, Luxembourg, the Netherlands, New Zealand, Norway, Portugal, Spain, Sweden, Switzerland, the United Kingdom and the United States.
[7]These figures are for 1992. (The bilateral proportion of foreign aid rose to 88% by 1996.) The source of these figures is: OECD (1998).

at least 25%[8]. (The grant element is the excess of the face value of a loan over the present value of interest and principal payments.) Unfortunately, there are some variations amongst countries on what constitutes ODA for reporting purposes. Some countries, for example, routinely treat all export credits as ODA, and others only treat some types of export credits as ODA.[9] In addition, the OECD definition of ODA suffers from some conceptual difficulties, as identified by Chang, Fernandez-Arias and Serven (1998). The main difficulty lies with the OECD's practice of using a 10% interest rate as the bench-mark of a fair-market-value loan. These difficulties can affect donors' and recipients' aid figures unevenly. (Chang, Fernandez-Arias and Serven computed alternative measurements of aid figures, but their detailed data on each donor's aid to each recipient is not available.) Despite some measurement problems with the ODA data, it is the best data available for my tests.

ODA is considered untied if the recipient is free to spend the money on goods and services procured from any developing country and any OECD country. Tied aid includes loans and grants provided on the condition that goods will be procured from the donor country, or from some other specified geographical region (e.g. the European Union). Partially tied ODA is loans and grants tied to procurement of goods and services from the donor country and from some restricted list of countries including substantially all developing countries.[10]

The 1992 bilateral ODA and tying percentages for each donor are shown in Table 1.1. In this particular year, Japan gave the most bilateral aid. Only 16% of its aid was tied and an additional 11% was partially tied. In contrast, the donor community as a whole averaged 40% and 12% respectively.

[8]OECD (1994), *Geographical Distribution of Financial Flows to Developing Countries*, p. 333.
[9]Jepma (1991), p. 32.
[10]OECD. 1994. *Development Co-operation: Efforts and Policies of the Members of the Development Assistance*

Table 1.1 1992 Bilateral ODA Commitments and Tying Percentages				
Donor	**Total Bilateral ODA (in $millions)**	**Percentage Tied**	**Percentage Partially Tied**	**Percentage Untied**
Australia	743	63%	0%	37%
Austria	1,326	21%	0%	79%
Belgium & Lux.	565	79%	4%	17%
Canada	1,744	43%	18%	39%
Denmark	--	--	--	--
Finland	473	46%	3%	51%
France	5,820	53%	16%	31%
Germany	6,415	55%	0%	45%
Ireland	--	--	--	--
Italy	2,444	70%	2%	28%
Japan	11,174	16%	11%	73%
Netherlands	1,966	12%	60%	28%
New Zealand	64	0%	0%	100%
Norway	527	18%	0%	82%
Portugal	82	97%	0%	3%
Spain	885	100%	0%	0%
Sweden	1,664	15%	0%	85%
Switzerland	548	28%	8%	64%
United Kingdom	1,711	67%	0%	33%
United States	8,848	42%	21%	37%
Total DAC	46,999	40%	12%	48%
Sources: OECD (on-line data).				

Donors give aid in sponsorship of specific projects. Some projects are capital in nature, such as the construction of roads, bridges and power generation plants, and the development of telecommunications infrastructures and agricultural irrigation projects. Other projects are non-capital in nature, such as the provision of food and support for health care, education and family-planning projects. A project idea may be initiated by the donor (specifically by aid-workers in the field), by the recipient, or by third parties (such as non-government organizations or universities). Once a project has been approved, the donor and recipient

Committee, p. F4.

negotiate an agreement. One of the issues addressed in the agreement will be procurement requirements. Procurement clauses often tie the aid by requiring the recipient to purchase goods and/or services from suppliers located in the donor country. Even where aid is not tied, procurement clauses will typically outline the tendering processes and contract-awarding process that the recipient must follow for the project. In principle, the recipient is usually supposed to be responsible for administering the procurement process. However, donors often take an active role in this process. The administration might also be contracted out to a consultant (often from the donor country). A few donors have their own specialized government procurement agencies that administer procurement on behalf of the recipients.[11] This service is sometimes also provided for projects not financed by aid. Donor countries will either pay suppliers directly or make payments to the recipient government, which then pays the suppliers.

1.3. POTENTIAL LINKS BETWEEN AID AND TRADE

There are numerous ways in which foreign aid can enlarge a donor's exports to the recipient. In this paper the links between aid and trade are classified into direct and indirect links. Exports directly linked to aid are those which arise on projects financed by aid. Indirectly linked exports are all other export gains that are attributable to aid but are not specifically part of an aid-financed project. The reason for this distinction is that our understanding of these links will govern the model specification used in the statistical tests. One would expect directly-linked gains to be roughly proportional to the amount of aid, while indirectly linked gains would not necessarily be proportional to aid, but would nevertheless magnify trade levels.

1.3.1 Direct Links

The most obvious direct aid-trade link occurs with explicitly-tied aid, where a recipient that receives tied aid is obligated to use those funds to buy goods or services from the donor. Some tied aid may finance exports that the donor could have achieved without the aid, but one would expect that most tied aid directly increases

[11] In 1981 those countries were Canada, the Netherlands and the United Kingdom (OECD, 1981).

the donor's exports.

Direct links to aid can also occur without an explicit tying arrangement. There are a number of ways a donor can effectively tie aid, intentionally or unintentionally, without a formal tying agreement.

A donor may implicitly tie aid by choosing to finance development projects that require supplies from industries in which the donor is strong. In some cases, exporting firms may initiate this process, and advise the recipient on how to secure aid from the firm's government for a project to which the firm can provide supplies.

Alternatively, a donor can implicitly tie aid by imposing bidding procedures that the recipient must follow in the procurement process. These procedures are thought necessary to minimize loss due to corruption. The bidding procedures will often be modelled after the donor's own domestic bidding procedures. The donor's domestic firms are the most familiar with such bidding procedures, and those firms will have an advantage in bidding to supply the recipient country's project.

1.3.2 Indirect Links in the Year Aid is Disbursed

Aid can also trigger trade that is not related to any particular project. For example, the recipient country may feel obligated to buy from the donor country to maintain goodwill so that the recipient can secure more aid in the future. This explanation has a certain circularity to it, since aid triggers exports and exports trigger aid. The story can easily be structured as a game. The objectives of donors and recipients can be summarized as follows. Donors seek to maximize their utility on two fronts; they seek the satisfaction of helping out poorer countries and they seek profits and employment derived from exports to the recipient countries.[12] Recipients must trade off their desires to receive aid, and their desires to avoid buying goods or services that do not give them the best value for their money.

The aid and trade relationship could be described as an infinite series of prisoners' dilemma games. We could imagine a pay-off structure as follows:

		Selfish Donor	
		Gives Aid	Gives No Aid
Recipient	Buys from Donor	3,1	0,2
	Buys Elsewhere	4,-1	1,0

The equilibrium in a one-shot game would be that the donor gives no aid and the recipient buys elsewhere. However, in an infinitely-repeated game the donor and the recipient may be able remain in the mutually preferable aid/export equilibrium. The donor decides how much aid to give, without knowing whether the recipient will "conform" by importing goods according to the donor's expectations (i.e. according to the implicit understanding). Meanwhile, the recipient government decides how much it will import from the donor country, without knowing whether the higher import levels will be rewarded with future aid. As long as the donor and the recipient "play nice", an implicit tying arrangement can be maintained.

1.3.3 Indirect Links – Trade Benefits Realized in Subsequent Years

The benefits of aid given in a particular year may persist and expand future exports. Exporting into a country typically involves an investment of effort into learning the business environment of the particular country. This effort includes identifying a market, developing customer relationships, establishing a reputation, establishing distribution channels, learning import and local laws and processes, and many others. Usually, the exporter will obtain much of this information in the course of its first sales into the country. Similarly,

[12] Some countries, notably the U.S., also seek security to protect their foreign interests and to avoid being drawn into a war.

the buyer also makes an investment when choosing a new supplier. Once a buyer has had experience with a particular supplier, there is a tendency to stick with the supplier, since the cost of checking other suppliers is often perceived to be greater than the expected benefits derived from looking for new suppliers.

A buyer's own experience with a supplier is a prime source of the buyer's information about the supplier and its products. In domestic markets, buyers can often overcome barriers to information about new suppliers through contacts in the local business environment. However, when suppliers are in different countries that have different cultures, have different languages and are far away, these barriers loom larger. Once a recipient has purchased goods on account of aid, some of the information barriers have been overcome. As a result, one would expect the reduction of information barriers to have positive effect on future years' trade, and perhaps to the current year's trade as well.

1.3.4 Causation

Consideration needs to be given to the question of whether it is appropriate to treat aid as an exogenous explanatory variable. If aid is influenced by trade levels (as suggested by the "goodwill" argument mentioned earlier), then aid would not be independent. In response to this concern I would argue that governments are unable to adjust aid levels very quickly in response to current trade levels, given that the aid process often involves several governmental departments, and involves numerous steps from project identification through to final government approval (done only once a year in many countries). As a result, the statistical tests can treat aid as a predetermined variable. Aid may be dependent on *past* trade, but it is not dependent on *current* trade.

Another causation issue is whether there are other factors that affect both aid and trade levels. The methods used to limit the effect such factors exert on the coefficient estimates are described later in the paper.

1.4 THE GRAVITY MODEL

To capture all the export gains associated with aid, we need a model that predicts total bilateral trade, and the gravity model is the best model available for this task. In the gravity model, predicted trade is the product of two components - potential trade and a discount factor reflecting trade barriers. Potential trade is the trade that would occur if there were no barriers to trade in finished goods (in a world of identical, homothetic preferences, with no trade of intermediate goods). Under these conditions, potential trade between countries *e* and *i* would equal $Y_e Y_i / Y_w$, where Y_e is the output (or income) of the exporter, Y_i is the income of the importer and Y_w is the income of the world. That is, country *i* buys its proportionate share (Y_i/Y_w) of country *e*'s output. In addition, many researchers also include the trading partners' income-per-capita as variables since the wealthier countries' output and consumption tends to involve tradable goods more than the less developed countries.

The second factor in the model captures the effect of barriers to trade. This factor equals 1 minus the proportion of potential trade that is blocked by trade barriers. A number of variables can be considered to compute this factor. At a minimum, all gravity models include distance as one of the determinants of this factor. This paper also includes variables measuring the trading partners' openness to trade and the degree to which the partners share a common language.

Before considering how foreign aid will be incorporated into the gravity model, the remainder of this section addresses in more detail the non-aid variables used in the gravity model. The logged gravity model for exports from donor country *d* to recipient country *r* is as follows[13]:

$$\ln T_{dr} = \beta_1 \ln(Y_d Y_r / Y_w) + \beta_2 \ln(Y_d / POP_d) + \beta_3 \ln(Y_r / POP_r) + \beta_4 \ln D_{dr}$$
$$+ \beta_5 \ln OPEN_d + \beta_6 \ln OPEN_r + \beta_7 LANG_{dr} + C + \varepsilon_{dr},$$

[13]Given that I have data for 1970 to 1992 for each donor/recipient pair, each of these factors should technically have a subscript *t* to indicate the year. However, since this subscript would apply to every single variable, it is simply

where:

T_{dr} = exports from donor country d to recipient country r,

POP_d, POP_r = population of countries d and r,

D_{dr} = distance between donor country d and recipient country r,

$OPEN_d$, $OPEN_r$ = openness of countries d and r to trade,

$LANG_{dr}$ = common language factor between countries d and r,

C = a constant, and

ε_{dr} = error term.

Y_d, Y_r and Y_w are the GDPs of countries d and r, and of the world. These data were obtained from the World Development Indicators CD-ROM published by the World Bank.

Distances equal the great circle distance (in miles) between the commercial capitals of the respective countries.[14]

Openness to trade for country j (i.e. $OPEN_j$) equals ($EXPORTS_j$ + $IMPORTS_j$) / (2 Y_j). This variable serves as a proxy for a variety of trade barriers, such as tariff levels, quotas and red tape. The variable was used once by Head and Ries (1998), but most researchers do not use this variable when employing the gravity model, because one of the components of the independent variables $OPEN_d$ and $OPEN_r$ is exports from the donor to the recipient, the dependent variable. Ordinarily, using the dependent variable to calculate an independent variable would be inappropriate, but in this case it is justifiable for two reasons. First, a country's trade with a single other country will normally only be a small part of the country's total trade. This is particularly true for donors. The biggest trading partners of donors are other donors, but trade

dropped to minimize the clutter in the equations.

[14] For most countries, the "commercial capital" is obvious. Where it is less obvious, I used some judgment, based in part upon which city is geographically closer to the centre of the country. For the U.S. I used Chicago, since New York is rather far from the centre of the country.

between donors is not included in my statistical tests. This argument is weaker for recipients, because some recipients conduct a large portion of their trade with the United States, Japan or a European country with colonial links. However, there is a second argument. Although the independent variable uses exports to determine its value, the variable is really just a proxy for the factors (such as tariffs) identified above. This computation is simply a convenient way of putting a number to these factors. Moreover, if a recipient were to cut off trade with a significant trading partner, much of that trade would likely be replaced by new trade with other partners. Put another way, the variable measures the amount of trade in which a country will engage, regardless of whom the trading partners may be. For the above reasons, this variable should not significantly distort the results on donor-recipient trading relationships. Conversely, the inclusion of the variable contributes to the model by capturing relevant factors that would otherwise be omitted.

$LANG_{dr}$ measures the probability that a randomly drawn person from country *d* and a randomly drawn person from country *r* can speak a common language. The computation of this variable can best be described with an example. Consider France and Morocco. 96.4% of France's population speaks French and 1.8% of its population speaks Arabic. Meanwhile, 20% of Morocco's population speaks French and 74% of its population speaks Arabic. The common language factor therefore equals (.964)(.2)+(.018)(.74) = 0.20612. The sources for this language data are described in Appendix 2.1.

It is important to control for these factors, since all of these factors are correlated with aid, as shown in Table 1.2. To preserve parallelism, this table uses similar variables to those used in the trade regressions. If the focus of the regression were instead the prediction of aid levels, then using population variables ($\ln(POP_d)$ and $\ln(POP_r)$) would be more appropriate than the GDP variables. Switching to population variables generates coefficients for the logged populations that are identical to those of the logged GDPs, but the income per capita variables change to 1.87 for the donor and –0.60 for the recipient. The high coefficient for the donor's income per capita suggests that aid-giving is like a luxury good.

Other variables often employed in gravity model tests include adjacency, colonial ties, and remoteness. My tests exclude the adjacency variable because the only adjacent donor-recipient trading partners are the United States and Mexico. Including this variable produces results that are virtually identical to the results without the adjacency variable. A colonial-ties variable is not used because my tests use better measures. The commonly used dummy variable for colonial ties is dichotomous, even though the recentness and intensity of colonial ties can vary substantially across trading partners. Moreover, my tests include a non-dichotomous language overlap variable that captures to a large degree the strength of colonial ties. In addition, the import-residual variable described later, captures other special trading relationships between trading partners. Some researchers also like to include a remoteness variable, since a country that is remote will tend to trade more with a partner of a given distance than centralized countries. My tests do not use remoteness variables, because the effect of remoteness is already captured by the openness variables.

The data for exports include all traded *goods*. Ideally, the statistical tests would also include trade in *services*, particularly in a study involving aid, which is often tied to consulting or construction services, or is provided in the form of donated services. Unfortunately, exports of services had to be excluded due to the unavailability of comparable data for the relevant trading partners.

The data include bilateral merchandise trade flows between 20 donor countries and 109 recipient countries (listed in Appendix 1.1) for the years 1970, 1975, 1980, 1985 and 1990. (The intervening years were left out to minimize distortions from serial correlation.)

1.5 ELASTICITY SPECIFICATION OF AID

The first specification with aid as an independent variable uses the common approach of including the log of aid as one of the regressors (with a modification described below). The specification assumes that aid

Table 1.2
Results of a Regression with Aid as the *Dependent* Variable

Specification:

$$\ln(A_{dr}) = \beta_1\ln(Y_d) + \beta_2\ln(Y_r) + \beta_3\ln(D_{dr}) + \beta_4\ln(Y_d/POP_d) + \beta_5\ln(Y_r/POP_r) + \beta_6 LANG_{dr} + \beta_7 MILLS_{dr} + C + \varepsilon_{dr}$$

	Estimate	Standard Error	P>|t|
$\ln(Y_d)$	1.12	0.06	0.000
$\ln(Y_r)$	0.54	0.03	0.000
$\ln(D_{dr})$	-0.77	0.07	0.000
$\ln(Y_d/POP_d)$	0.75	0.16	0.000
$\ln(Y_r/POP_r)$	-1.15	0.07	0.000
$LANG_{dr}$	2.03	0.16	0.000
$MILLS_{dr}$	1.51	0.29	0.000
C	-21.5	3.0	0.000
No. of obs.	4523		
Adj R^2	0.36		

Notes:
(1) This regression uses the years 1970, 1975, 1980, 1985 and 1990.
(2) $MILLS_{dr}$ is Mills' Ratio, computed based on the probability that the observation has a positive aid amount.

magnifies trade, either by upwardly biasing trade between the donor and the recipient, or by reducing barriers to trade. For now the specification does not yet separate out direct effects from indirect effects as described in section 1.3. This simple specification has two primary benefits. The first benefit is that the model can be tested using an OLS regression. The second benefit is that the aid coefficient can be interpreted as an elasticity of aid.

In order to get the most accurate coefficients possible for non-aid variables, the statistical tests should include observations where aid equals zero. In that case, however, I cannot simply use log(aid) as the aid variable, because this value is undefined when aid is zero. One common method of handling this type of problem is to add 1 to the data before logging it – in this case producing log(1+aid). This approach incorrectly assumes that "1+" is an immaterial adjustment as long as all positive values in the data are large numbers. The "1+" is not immaterial, however, when aid is zero. If the specification were modified to log(0.1+aid) or log(10+aid), the predicted coefficient changes enormously. A better solution is to allow the data to determine how to handle cases where aid is zero, with the following specification:

$$\ln T_{dr} = \ln\Gamma_{dr} + \beta_8 \ln(\max\{1, A_{dr}\}) + \beta_9 NAD_{dr} + \varepsilon_{dr},$$

where:

$$\ln\Gamma_{dr} = \beta_1 \ln(Y_d Y_r / Y_w) + \beta_2 \ln(Y_d / POP_d) + \beta_3 \ln(Y_r / POP_r) + \beta_4 \ln D_{dr} + \beta_5 \ln OPEN_d + \beta_6 \ln OPEN_r + \beta_7 LANG_{dr} + C;$$

A_{dr} = the ODA (measured in 1987 $US) given by donor *d* to recipient *r*; and

NAD_{dr} = "No Aid Dummy", which = 1 if $A_{dr}=0$, but equals 0 if $A_{dr} > 0$.

(Γ_{dr} is used to simplify presentation. The variable includes all the independent variables and their parameters, except those that relate to aid.)

Note that:

$$\beta_8\ln(\max\{1,A_{dr}\}) + \beta_9 NAD_{dr} = \begin{cases} \beta_8\ln A_{dr} & \text{when } A_{dr}>0, \text{ and} \\ \beta_9 & \text{when } A_{dr}=0. \end{cases}$$

Thus, β_8 measures the elasticity where aid is positive, and β_9 serves as an adjustment to the constant for cases where aid is zero. Logged trade when aid is positive exceeds logged trade when aid is zero by $\beta_8\ln A_{dr} - \beta_9$.

Ideally, I would have liked to have distinguished between tied aid and untied aid. Unfortunately, tying percentage data is only available as an aggregate figure for each donor. Consequently, each observation's tying percentage is not known, forcing tied and untied aid to be treated identically.

The results of OLS regressions using this specification are reported in Table 1.3. Column 1 reports the results using pooled data and shows a coefficient for $\ln(\max\{1,A_{dr}\})$ of 0.180. This result suggests that increasing aid to a country by 10% increases the donor's exports to the recipient by 1.8%. This elasticity can be used to infer how much exports should increase per dollar of aid by determining how much predicted trade would increase if aid were increased by 1%, and dividing that additional trade by 1% of aid. The result of that calculation (reported at the bottom of table 1.3) is that each dollar of additional aid on average yields an additional $2.14 of merchandise exports. Thus the return on aid appears to be much larger than the tying percentages would suggest.

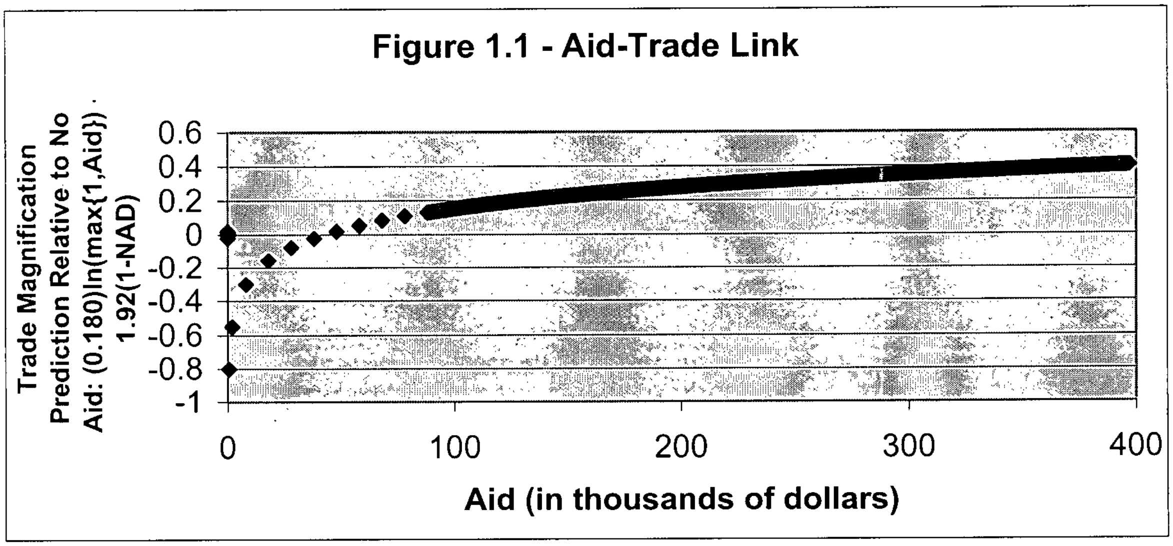
The coefficient for NAD_{dr} is 1.92, which should be understood in tandem with the $\ln(\max\{1,A_{dr}\})$ variable. Figure 1.1 presents an interpretation of the combined results of the two variables. Logged trade when aid is positive exceeds logged trade when aid is zero by $(0.180)\ln A_{dr} - 1.92$. Where aid is greater than $88,000 (which is the smallest positive aid amount in the data) the addition to logged trade is represented by the solid line on the right hand side of the graph. The dotted curve to the left of $88,000 of aid shows the

Table 1.3
OLS Regression Results - Elasticity of Aid Specification

Specification: $\ln T_{dr} = \ln\Gamma_{dr} + \beta_8 \ln(\max\{1,A_{dr}\}) + \beta_9 NAD_{dr} + \varepsilon_{dr}$

	Pooled OLS	Using Residuals from Imports	Using Fixed Effects
$\ln(\max\{1,A_{dr}\})$	0.180 se=.011;P=.000	0.150 se=.010;P=.000	0.054 se=.012;P=.000
NAD_{dr}	1.92 se=.16;P=.000	1.60 se=.15;P=.000	0.64 se=.17;P=.000
$\ln(Y_dY_r/Y_w)$	1.06 se=.01;P=.000	1.05 se=.01;P=.000	2.41 se=.14;P=.000
$\ln(D_{dr})$	-0.81 se=.03;P=.000	-0.80 se=.03;P=.000	(dropped)
$\ln(OPEN_d)$	0.68 se=.04;P=.000	0.65 se=.03;P=.000	0.52 se=.13;P=.000
$\ln(OPEN_r)$	0.90 se=.03;P=.000	0.92 se=.03;P=.000	0.23 se=.05;P=.000
$LANG_{dr}$	1.54 se=.08;P=.000	1.63 se=.08;P=.000	(dropped)
$\ln(Y_d/POP_d)$	0.68 se=.04;p=.000	0.65 se=.03;p=.000	0.52 se=.13;p=.000
$\ln(Y_r/POP_r)$	0.90 se=.03;p=.000	0.92 se=.03;p=.000	0.23 se=.05;p=.000
Import Residual		0.22 se=.01;P=.000	
Obs.	8,377	7,630	8,377
Adj. R^2	0.748	0.769	0.894
Root MSE	1.348	1.189	
Implied "Return" of Exports on Aid	$2.14	$1.71	$0.63

Note 1: Figures shown in large fonts are the coefficient estimates. In small fonts beneath each estimate are the estimated error and the corresponding probability of getting such an estimate if the true coefficient were 0.
Note 2: Additional control variables included in the regression but not shown include year dummies and Mills' Ratio.



$(0.180)\ln(A_{dr}) - 1.92$ curve in the range where no aid values are observed. (The graph only shows the region of small aid values.) Thus, for the smallest positive amount of aid in the sample, bilateral trade is higher than it would have been in the absence of aid.

Nilsson used a simpler version of the elasticity specification for his tests on European Union countries. He simply inserted the log of aid into the gravity model, and thus presumably dropped observations with zero aid. His method produced a higher elasticity of 0.23. I obtain a similar result when I exclude observations with zero aid. Using my data with all donors, I obtained an elasticity of 0.24. However, two other coefficients change significantly as well – the coefficient for the income variable $\ln(Y_d Y_r / Y_w)$ drops from 1.06 to 0.92 and the coefficient for the language variable drops from 1.54 to 1.02.

The pooled OLS regression reported in column 1 does little about the problem of unmeasured variables that affect both trade levels and aid levels between countries. The parameter estimates may be capturing the effect of such unmeasured variables and may therefore mislead us regarding the true links between aid and trade. The usual solution researchers adopt for gravity models is to add dummy variables to the model for factors that may signify special relationships. For example, a dummy variable for colonial links may be considered. Unfortunately, this approach involves a very crude proxy for these relationships and will still miss aspects of the special trading relationships between nations. Consequently, I explore two different methods to overcome this problem.

The first method relies on an assumption that unmeasured variables would on average affect imports in the same way that they affect exports. This method involves first running an OLS regression on the donor's *imports* from the recipient, and then using the residuals from that regression as an independent variable in an OLS regression on exports from the donor to the recipient. The results of this procedure are reported in the second column of Table 1.3. Using this test, the elasticity falls somewhat to 0.150. This elasticity translates into an average of 1.71 cents of exports generated per additional dollar of aid. The parameter on the import residual is 0.22 and is statistically significant at the 99% level.

The second method uses fixed effects for country pairs. This method does not rely on the assumption required for the first method, but it sacrifices the informational content of cross-sectional variation in the data. The results of the test using fixed effects are reported in column 3. In this case the estimate of the elasticity falls to .054, but still remains significantly different from 0. The fixed effects result implies that exports derived from a dollar of aid amount to 63 cents. This is my most conservative result, yet this amount is well above the average official tying percentage. Moreover the 63 cents only include merchandise trade, while the official tying percentages involve trade in both goods and services.

In addition to the tests reported in Table 1.3, variations of the tests using the elasticity specifications were examined. In one variation, the openness variable was excluded from the tests, since most researchers do not use this variable in gravity model tests. This modification had a small effect on the coefficient for aid, but a large effect on the coefficient for the income variable. In the "pooled OLS" test, the coefficient for aid increased slightly from 0.18 to 0.20 while the coefficient for the income variable dropped from 1.06 to 0.84. The theoretical underpinnings of the gravity model suggest that the coefficient for the income variable should be 1, so the inclusion of the openness variable seems to improve the regression's predictions. Moreover, excluding the openness variable reduces the R-square statistic from 0.748 to 0.710. The model with import residuals generates similar results. Excluding the openness variables raises the coefficient for aid from 0.15 to 0.16, lowers the coefficient for the income variable from 1.05 to 0.83, and lowers the R-square statistic from 0.769 to 0.748.

In a second variation, each variable was substituted with a variable equal to the current year's value minus the previous period's value. Thus this test measures the change in trade as a function of the change in aid and the change in other variables. The results of this test correspond closely with the results of the fixed effects test reported in column 3 of Table 1.3. The aid coefficient was 0.061, which is close to the fixed effects outcome of 0.054. The coefficient of the income variable was 2.05, which deviates considerably from the expected coefficient of 1. As with the fixed effects test, this test loses the effect of cross-sectional variation.

In a third variation the lagged dependent variable was included as an independent variable. This variable is recommended by Eichengreen and Irwin (1996), who contend that the gravity model is often used without adequate care for considering omitted variables, which can lead to erroneous conclusions. Omitted variables can involve persistent errors for a pair of trading partners or can involve historical relationships. Eichengreen and Irwin conclude that the gravity model should always have the lagged dependent variable as a regressor. While it is indeed true that omitted variables need proper attention, the solution can often be achieved in other ways that permit a more natural interpretation of the coefficient estimates. In this paper. import residuals accomplish this objective.

For comparison, a lagged dependent variable was incorporated in a variation to the tests reported in Table 1.3. In the pooled OLS test, the coefficient for $\ln(\max\{1,A_{dr}\})$ remains statistically significant at the 99.9% level, but drops from 0.180 (reported in Table 1.3) to 0.072 (not reported in a table). According to Head and Ries (1998), using the lagged dependent variable allows a factor, such as aid, to affect trade in two ways – directly and indirectly. The direct effect comes from the aid variable itself while the indirect effect comes through the lagged dependent variable, because aid affected prior years' exports, which affect the current year's exports. Based on this reasoning, Head and Ries approximate the total effect of a variable to be $\beta/(1-\lambda)$, where β is the variable's coefficient estimate and λ is the coefficient estimate of the lagged dependent variable. In this case β=0.072 and λ=0.607, generating a total effect of 0.183, which is very close to the coefficient estimate reported in Table 1.3. When adding the lagged dependent variable to the model with import residuals, the coefficient estimate for $\ln(\max\{1,A_{dr}\})$ continues to remain statistically significant at the 99.9% level, and the computed total aid effect amounts to 0.138, which is not much below the 0.150 reported in column 2 of Table 1.3. Thus, the use of the lagged dependent variable does not yield results significantly different from those reported in Table 1.3.

The statistical tests reported in the next sections no longer report fixed effects results, because this method probably does not yield the best estimates. It is interesting that this conservative method still yields a statistically significant result and that the implied "return" on aid is greater than official tying percentages, but the method probably understates the true link between aid and trade since fixed effects reduce the signal to noise ratio and bias the estimates toward zero. Errors in the measurement of aid (which have been identified by Chang, Fernandez-Arias and Serven (1998)) bias coefficient estimates toward zero much more strongly in a fixed effects model than in a model without fixed effects.[15] Moreover, the remaining statistical tests involve maximum likelihood estimates (MLE's) in which fixed effects are problematic. The remaining statistical tests in this paper rely on the import residuals variable to account for special relationships between trading partners.

1.6 DIRECT AND INDIRECT EFFECTS ON TRADE

The above specification has the advantage of permitting an OLS regression, but may not be the most suitable method of accounting for the effects of aid on trade. As was argued in section 1.3, one could divide the effects of aid into two components – a direct effect and an indirect effect. The direct effect includes any exports on projects directly financed by aid, and one would expect such export gains to have a proportional relationship to the amount of aid. Consequently, exports arising from the direct effect would be added to the exports that would otherwise exist. The indirect effect involves all other export gains, of the type that results from an upward bias in trade levels, or possibly from a reduction of informational trade barriers. The indirect effect would therefore have a magnification (i.e. multiplicative) effect on trade levels.

The specification used to measure both types of effects is as follows:

$$T_{dr} = [\, \Gamma_{dr}\, (\max\{1,A_{dr}\})^{\beta 8}\, e^{\beta 9 NAD dr} + \beta_{10}A_{dr}]\, e^{\varepsilon dr}.$$

$\beta_{10}A_{dr}$, which is proportional to aid, measures the direct effect and $(\max\{1,A_{dr}\})^{\beta 8}\, e^{\beta 9 NAD dr}$ measures the indirect effect.[16] This specification requires the use of a maximum likelihood estimate.[17] The results are reported in Table 1.4. The first column reports the results using pooled data, and suggests that the direct effect on trade is 42 cents per dollar of aid and the indirect effect's elasticity is 0.089. This elasticity translates into an average of 89 cents of goods exports per dollar of aid, so the direct plus indirect effects sum to $1.31 as reported at the bottom of the table.

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[15] See Griliches (1986), pages 1478-1480.

[16] Technically, $\beta_{10}A_{dr}$ should be deducted from the recipient's income in computing Γ_{dr}, since the recipient is obliged to spend $\beta_{10}A_{dr}$ on goods from the donor. This amount of the recipient's income is therefore not available for general imports that get affected by donor size, distance, etc. However, $\beta_{10}A_{dr}$ is negligible relative to Y_r. A_{dr} exceeds 1% of Y_r in only 0.2% of the observations.

[17] The log likelihood function is $\ln L = -(n/2)(\ln 2\pi + \ln\sigma^2) - (1/2\sigma^2)\Sigma[\ln T_{dr} - \ln(\Gamma_{dr}\, (\max\{1,A_{dr}\})^{\beta 8}\, e^{\beta 9 ADUMMY dr} + \beta_{10}A_{dr})]^2$.

Table 1.4
Aid's Direct and Indirect Effects on Trade

Specification: $T_{dr} = [\Gamma_{dr} (\max\{1,A_{dr}\})^{\beta 8} e^{\beta 9 NAD dr} + \beta_{10}A_{dr}] e^{\varepsilon dr}$

	Pooled MLE	Using Residuals from Imports
A_{dr}	0.42 se=.01;P=.000	0.33 se=.05;P=.000
Max{1,A_{dr}}	0.089 se=.013;P=.000	.080 se=.18;P=.000
NAD_{dr}	0.84 se=.18;P=.000	0.74 se=.18;P=.000
Y_dY_r/Y_w	1.14 se=.01;P=.000	1.21 se=.01;P=.000
D_{dr}	-0.82 se=.03;P=.000	-0.83 se=.03;P=.000
$OPEN_d$	0.77 se=.04;P=.000	0.72 se=.04;P=.000
$OPEN_r$	0.96 se=.03;P=.000	0.98 se=.03;P=.000
$LANG_{dr}$	1.56 se=.09;P=.000	1.69 se=.08;P=.000
Y_d/POP_d	0.20 se=.04;p=.000	0.26 se=.04;p=.000
Y_r/POP_r	-0.05 se=.02;p=.014	-0.06 se=.02;p=.001
Imports Residuals		0.23 se=.01;P=.000
σ^2	1.76	1.38
Obs.	8,377	7,630
Adjusted R^2	.754	.773
MLE	-14,259	-12,057
Implied "Return" of exports on Aid	$1.31	$1.17

Note 1: Figures shown in large fonts are the coefficient estimates. In small fonts beneath each estimate are the estimated error and the corresponding probability of getting such an estimate if the true coefficient were 0.

Note 2: Additional control variables included in the regression but not shown include year dummies and Mills' Ratio.

The second column reports the results using residuals from a regression on imports. As previously explained, the residual from the import regression is assumed to capture unmeasured variables affecting trade levels between the donor and the recipient. With this modification, the direct effect drops to 33 cents per dollar of aid, and the indirect effect's elasticity is 0.080, which translates into an average of 84 cents of goods exports. The direct plus indirect effects sum to $1.17.

The most striking conclusion from this measurement is that the indirect effect appears to be more than double the direct effect. We also observe that the direct effect of 33 cents of trade per dollar of aid falls below the average official tying percentage of about 50% . This difference is probably accounted for by the fact that the official tying percentage includes both goods and services exports, while the 33 cents measures only goods exports.

1.7 PAST AID'S EFFECT ON CURRENT EXPORTS

The statistical tests described up to this point assume that the trade benefits of aid are limited to the current year only. However, there are several reasons to hypothesize that the trade benefits of aid extend beyond the initial year. First, if the aid was originally tied (whether explicitly or implicitly) a trading relationship has been created. That relationship may yield additional transactions, possibly on account of follow-up work, supplies, upgrades, or complementary products. Alternatively, future transactions may follow due to the fact that costs associated with dealing with a new customer or supplier have already been overcome. Further, the established relationship may strengthen the supplier's/customer's ability to transact with other enterprises in the customer's/supplier's country.

The next test therefore considers the possibility that previous years' aid may contribute toward the current year's trade. The elasticity specification is used to measure the effect of prior years' aid, since one expects

Table 1.5
Trade from Aid of the Current Year and the Prior Five Years

Specification: $T_{dr} = [\Gamma_{dr}\,(\max\{1,A_{dr}\})^{\beta 8}\, e^{\beta 9 NAD dr}\,(\max\{1,A^*_{dr}\})^{\beta 11} + \beta_{10}A_{dr}]\, e^{\varepsilon dr}$

	Using Residuals from Imports
$\max\{1,A^*_{dr}\}$	0.009 se=.004;P=.044
A_{drt}	0.33 se=.02;P=.000
$\max\{1,A_{dr}\}$	0.073 se=.012;P=.000
NAD_{dr}	-0.74 se=.17;P=.000
Y_dY_r/Y_w	1.12 se=.01;P=.000
D_{dr}	-0.83 se=.03;P=.000
$OPEN_d$	0.71 se=.04;P=.000
$OPEN_r$	0.97 se=.03;P=.000
$LANG_{dr}$	1.69 se=.08;P=.000
Y_d/POP_d	0.24 se=.04;P=.000
Y_r/POP_r	-0.05 se=.02;P=.003
Imports Residuals	0.23 se=.01;P=.000
σ^2	1.38
Obs.	7,630
Adjusted R^2	.773
MLE	-12,056
Implied "Return" on Current Aid	$1.09
Implied "Return" on Past Aid	$0.11

Note 1: Figures shown in large fonts are the coefficient estimates. In small fonts beneath each estimate are the estimated error and the corresponding probability of getting such an estimate if the true coefficient were 0.

Note 2: Additional control variables included in the regression but not shown include year dummies and Mills' Ratio.

this type of trade expansion to arise from greater opportunities to conduct trade, and would generally not arise on projects directly financed with aid. The specification is as follows:

$$T_{dr} = [\Gamma_{dr} (\max\{1, A_{dr}\})^{\beta 8}\, e^{\beta 9 NAD dr} (\max\{1, A^*_{dr}\})^{\beta 11} + \beta_{10} A_{dr}]\, e^{\varepsilon dr}$$

where A^*_{dr} is the average aid of the previous five years. A separate "no aid dummy" term was not included for past aid, since that variable is highly co-linear with the "no aid dummy" variable for current aid.

The results are reported in table 1.5. The effect of past years' aid is surprisingly small. The past years' elasticity of 0.009 implies that a dollar of aid generates only 11 cents of exports in the years following the donation. We also observe that the current year results change very little from the previous tests. The total merchandise trade generated from a dollar of aid amounts to $1.09 in the current year and 11 cents in subsequent years for a total value of $1.20.

The small effect of past years' aid is puzzling. One might have thought that trade benefits would persist because customer/supplier relationships have been established, but this result suggests that the value of establishing trading relationships is small. In light of this, it appears unlikely that the *current* year's large indirect effect is the result of such customer/supplier relationships either. Consequently, these results suggest that the current year's large indirect effect arises from our other explanation for indirect trade gains; it appears that trade gains may largely be recipients' investments in goodwill, and that recipients time these investments to occur in the year they receive aid.

1.8 JAPAN AND OTHER DONORS

As stated in the introduction, Japan has repeatedly been accused of commercial self interest in its aid practices. This criticism has persisted even though Japan's tying percentage has dropped well below the DAC average. Accordingly, I test the hypothesis that Japan's aid is more linked to exports than other countries' aid. The test uses dummy variables for Japan in a specification that assumes aid affects trade both directly and indirectly. Prior years' aid is not included in this specification since it adds little to the results. The following structure is used.

$$T_{dr} = [\, \Gamma_{dr} (\max\{1,A_{dr}\})^{\beta 8}\, e^{\beta 9 NAD dr}\, e^{\beta 12 (JDUMMY dr)} (\max\{1,A_{dr} JDUMMY_{dr}\})^{\beta 13}\, e^{\beta 14 (NAD dr)(JDUMMY dr)}$$

$$+ \beta_{10} A_{dr} + \beta_{15}(JDUMMY_{dr})(A_{dr})]\, e^{\varepsilon dr}.$$

$JDUMMY_{dr}$ is a dummy variable equal to 1 if the donor is Japan, and 0 if the donor is not Japan. The term $e^{\beta 12(JDUMMY)}$ is needed to ensure that β_{13} to β_{15} do not, in part, measure a tendency of the gravity model to over or underestimate Japan's trade. If β_{13} and β_{15} are positive, Japan's aid is effectively more tied to its exports than other countries' aid. If these coefficients are negative, the opposite is true. The left side of table 1.6 reports these results. Japan's direct effect is 27 cents higher than the donor average of 33 cents, resulting in a total direct effect of 60 cents. Its elasticity on indirect effects is 0.098 less than the donor average of 0.092, leading to a small negative measurement of Japan's indirect effect. The implied return of the indirect effect works out to negative 12 cents, giving an overall return of 48 cents, well below the donor average of \$1.17 computed in section 1.6. (The negative indirect effect is not significantly different from zero.)

In the 1980's Japan reduced its tying percentage substantially, so I reran the tests, separating Japan's results for 1970, 1975 and 1980 from its results for 1985 and 1990. The specification is as follows:

$$T_{dr} = [\, \Gamma_{dr} (\max\{1,A_{dr}\})^{\beta 8}\, e^{\beta 9 NAD dr}\, e^{\beta 12 (JDUMMY 1dr)} (\max\{1,A_{dr} JDUMMY_{1dr}\})^{\beta 13}\, e^{\beta 14 (NAD dr)(JDUMMY 1dr)}$$

$$e^{\beta 16 (JDUMMY 2dr)} (\max\{1,A_{dr} JDUMMY_{2dr}\})^{\beta 17}\, e^{\beta 18 (NAD dr)(JDUMMY 2dr)}$$

$$+ \beta_{10} A_{dr} + \beta_{15}(JDUMMY_{1dr})(A_{dr}) + \beta_{19}(JDUMMY_{2dr})(A_{dr})]\, e^{\varepsilon dr}.$$

where:

$JDUMMY_{1dr}$ = 1 if donor is Japan and the year is 1970, 1975 or 1980, and = 0 otherwise; and

$JDUMMY_{2dr}$ = 1 if donor is Japan and the year is 1985 or 1990, and = 0 otherwise.

This specification is like the previous one except that there are now two Japan dummies rather than one. The

Table 1.6
Japan Versus the Donor Average

First Japan Specification: All Years		Second Japan Specification: 1970-1980 versus 1985-1990	
$\max\{1, A_{dr}\}$ (indirect effect)	0.092 (se=.034;P=.006)	$\max\{1, A_{dr}\}$ (indirect effect)	0.091 (se=.013;P=.000)
NAD_{dr}	0.92 (se=.41;P=.024)	NAD_{dr}	0.92 (se=.19;P=.000)
A_{dr} (direct effect)	0.33 (se=.23;P=.161)	A_{dr} (direct effect)	0.33 (se=.05;p=.000)
$JDUMMY_{dr}$	0.85 (se=.13;P=.000)	$JDUMMY_{1dr}$	0.91 (se=.14;P=.000)
		$JDUMMY_{2dr}$	0.63 (se=.26;P=.017)
$\max\{1, A_{dr}(JDUMMY_{dr})\}$	-0.098 (se=.041;P=.018)	$\max\{1, A_{dr}(JDUMMY_{1dr})\}$	-0.073 (se=.049;P=.138)
		$\max\{1, A_{dr}(JDUMMY_{2dr})\}$	-0.136 (se=.051;P=.007)
$NAD_{dr}(JDUMMY_{dr})$	-1.07 (se=.61;P=.077)	$NAD_{dr}(JDUMMY_{1dr})$	-0.66 (se=.74;P=.370)
		$NAD_{dr}(JDUMMY_{2dr})$	-1.80 (se=.84;P=.031)
$A_{dr}(JDUMMY_{dr})$	0.27 (se=.26;P=.298)	$A_{dr}(JDUMMY_{dr})$	1.20 (se=14;P=.000)
		$A_{dr}(JDUMMY_{dr})$	0.19 (se=.11;P=.088)
Implied Return for Japan	0.48	Japan Implied Return:	
		1970-1980	2.06
		1985-1990	-0.08

Note 1: Figures shown in large fonts are the coefficient estimates. In small fonts beneath each estimate are the estimated error and the corresponding probability of getting such an estimate if the true coefficient were 0.

Note 2: Additional control variables included in the statistical tests but not shown here include $\log(Y_d Y_r / Y_w)$, $\log D_{dr}$, $\log(OPEN_d)$, $\log(OPEN_r)$, $LANG_{dr}$, $\log(Y_d/POP_d)$, $\log(Y_r/POP_r)$, year dummies, Mills' Ratio, and Import residuals.

first dummy covers the period during which Japan reported high tying percentages and the second dummy covers the period during which Japan reported low tying percentages.

The results of this test (reported on the right hand side of table 1.6) correspond with the change in Japan's tying percentage. From 1970 to 1980, the "return" on aid is estimated at $2.06 of exports of goods for each dollar of aid. After 1980, Japan's estimated return is eliminated.

Table 1.7 reports the implied returns for all donors based on identical tests. (The table excludes Portugal, because it had only two observations of non-zero aid). Countries that have the highest "return" of exports

Table 1.7
Relationships between Aid and Trade for Specific Donors

Donor	Implied Return on Aid	Rank	Donor Tying % Incl. Partial Tying[1] 1980	1985	1990
Australia	4.53	2nd highest	34	47	84
Austria	1.10	8th highest	99	97	61
Belgium	0.88	8th lowest	74	63	--
Canada	1.00	Median	90	58	61
Denmark	0.26	4th lowest	42	40	--
Finland	0.89	9th lowest	63	19	73
France	2.64	4th highest	57	57	53
Germany	1.01	9th highest	18	36	56
Ireland	1.24	7th highest	--	0	--
Italy	0.56	6th lowest	74	83	83
Japan	0.48	5th lowest	74	39	23
Netherlands	0.08	2nd lowest	44	40	49
New Zealand	5.00	Highest	52	22	0
Norway	-0.04	Lowest	31	30	39
Spain	0.62	7th lowest	--	--	--
Sweden	0.11	3rd lowest	17	31	22
Switzerland	1.35	5th highest	39	33	37
United Kingdom	2.80	3rd highest	--	72	--
United States	1.34	6th highest	73	59	31

Note 1: Source: OECD. These figures are the tying percentages associated with ODA commitments of those particular years. (In contrast, the aid data used for statistical tests are ODA *disbursements*.) Pre-1979 data on tying are not available.

per dollar of aid are New Zealand, Australia, the United Kingdom and France. Countries with the lowest "return" are Norway, the Netherlands, Sweden and Denmark, followed by Japan. These results look consistent with the results reported by Alesina and Dollar (1998). The countries with the lowest exports per dollar of aid tend to be the same countries that are the most altruistic by the measures considered by Alesina and Dollar. The notable exception is Japan, which they find to be among the more selfish donors, since Japan tends to give the most to countries that support it in international affairs (as measured by how often those countries vote with Japan in the U.N.). The estimated "return" on aid computed in these statistical tests do not correspond very strongly with the official tying percentages. The correlation between the estimated "return" on aid and the average reported tying percentages is 0.004. Spearman's rho, which correlates the countries' rankings, is 0.17. The countries with the highest returns on aid (New Zealand, Australia, and to a lesser degree the United Kingdom and France) tend to report moderate levels of official tying. Meanwhile some countries with high levels of official tying percentage – such as Italy and Austria – generate moderate amounts of trade. Part of this disparity might be attributable to exports of services, which are not included in the trade data.

Based on my tests on specific countries, there is no evidence that Japan exploits aid for commercial advantage more strongly than the average donor; on the contrary, Japan appears to derive less trade from its aid program than most other donors.

1.9 CONCLUSION

My statistical tests imply that the link between aid and trade is much larger than the official tying percentages suggest. The best estimate is that 33 cents out of every dollar of aid comes directly back to the donor for exports of goods related to the aid-financed project and that another 84 cents finds its way back to the donor for exports of goods not directly linked to aid projects. These measurements exclude exports of services, which surely comprise a significant part of donors' exports to recipients.

Somewhat surprisingly, the trade benefits appear to be limited almost entirely to the year that the donation is made. Less than 10% of the trade value of aid comes after the year of donation.

With respect to individual donors, there is no evidence that Japan continued to effectively tie its aid more than other donors after Japan reduced its official tying percentage. In fact, Japan appears to gain less commercial advantage from aid than most other donors. Donors that show a high export return on aid are New Zealand, Australia, the United Kingdom and France. Donors that earn a low export return are the Scandinavian countries, the Netherlands and Japan.

In the future, if data on service exports were to become available, it could be incorporated into the existing data and provide a clearer picture of the relationships between aid and trade. Similarly, if data on tied, partially tied and untied aid for each donor-recipient pair became available, it would be useful to exploit that data to measure the return on each type of aid.

CHAPTER 2: IMMIGRANTS AND THE TRADE OF PROVINCES

2.1 INTRODUCTION

According to the 1996 Census, one out of every six Canadian residents was born outside of Canada. This high level of immigrant population can be expected to exert significant influence on the Canadian economy. Academic research has traditionally focused on the effect immigrants have on the labour market and social services. This paper continues the research on another aspect of the economy immigrants may influence—international trade.

We employ provincial trade and immigration data to relate immigration levels to imports and exports with more than 150 trading partners over the period 1992-1995. Using a gravity model framework, we posit three alternative specifications reflecting immigrant influence on trade. The first specification assumes a constant elasticity between trade levels and immigration levels. The second specification, the "propensity" model, posits that an immigrant's propensity for trade creation is a constant multiple of non-immigrants' propensity. The third model allows the immigrant effect to decrease as the number of immigrants in a province rises. All of the specifications control for the income of provinces and trading partners, the distance between them, as well as time-invariant, unmeasured provincial characteristics that influence trade. We also include a variable reflecting common language overlap as well as Canadian-partner country fixed effects. The language variable tests whether knowledge of a language is the prime factor in the immigration effect, and the fixed country effects capture relationships between Canada and foreign countries that simultaneously influence trade and immigration.

There are several intuitive and theoretical reasons for believing that immigrants play an important role in expanding Canada's foreign trade. One of the bases for this belief is that immigrants face smaller trade barriers than non-immigrants. For example, immigrants already know the language spoken in their land of origin. They tend to be familiar with their previous country's business laws and practices. They may already have established networks of customers and/or suppliers. They are more familiar with customers' preferences. A non-immigrant may find potential trading opportunities infeasible because the costs of familiarization with the foreign environment may be too high, while an immigrant might be able to profitably carry out the same trade. In addition, immigrants may expand imports because they have preferences for certain goods produced in their country of origin. These preferences may be based on tastes developed before immigration. Moreover, immigrants might expand Canadian exports due to Canada's immigration policy. Canadian policy tends to favour immigrants who will expand Canada's economy, and ideally will boost Canada's exports.

Two previous papers have estimated the significance and magnitude of the effect immigration levels have on trade. Gould (1994) compiled U.S. national trade data and found a statistically significant relationship between immigration and trade. Similarly, Head and Ries (1998) tested the hypothesis with Canadian national trade data and obtained a similar conclusion. However, we now have trade and immigration data at the province level, allowing us to introduce fixed country effects into our statistical tests. A concern about the Head and Ries study was that there may be political factors or other national factors that affect both immigration and trade and thus give results that wrongly appear to suggest that immigration promotes trade. For example, both immigration and trade with Hong Kong and Great Britain may be attributable to commonwealth links. Without employing fixed country effects, we might erroneously link high trade with these countries to immigration when the trade might actually be attributable to a commonwealth link. Similarly, low levels of trade and immigration with North Korea reflect the closed North Korean economy, rather than any causal link between immigration and trade. By introducing fixed country effects, we can examine trade and immigration variation among the provinces and thus control for national level factors.[1]

This paper is organized as follows. Section 2.2 describes the gravity model of trade. We use the model in a slightly different way from the conventional practice, and explain our reasons for doing so. Section 2.3 describes Canadian immigration trends and expands on why we hypothesize that Canadian immigrants boost Canada's foreign trade. Section 2.4 describes how we incorporated language in our statistical tests and section 2.5 describes our specifications and reports our results. Section 2.6 concludes the paper.

2.2 THE GRAVITY MODEL

2.2.1 Description of the Gravity Model

The gravity model is an effective tool for empirically measuring the effects of various factors on trade levels.[2,3] The model provides a remarkably good fit, typically producing an R squared of about 0.7. The gravity model is the best tool we have to measure immigration's effect on trade levels.

The gravity model predicts that trade is proportionate to each trading partner's economic weight (usually measured by GDP) and is negatively correlated with the distance between the traders. The standard gravity equation is:

$$T_{ei} = (Y_e\, Y_i \,/\, Y_w) \text{ x } (D_{ei}^{\beta 1} \text{ x } \Phi_{ei}) \text{ x } e^{\varepsilon ei}$$

[1] Gould (1994) uses fixed country effects in his analysis of U.S. national data. He examines variation over time rather than cross-sectionally across states.

[2] For example, Wei and Frankel (1994) employed the gravity model to measure the effect that regional trading blocs have on trade. McCallum (1995) and Helliwell (1996 and 1997) used the model to measure the effect that the Canada-US border has on trade, relative to inter-provincial trade.

[3] Theoretical support has been provided by Linneman (1966), Leamer & Stern (1970), Anderson (1979), Krugman (1979), Bergstrand (1985) and Deardorff (1995). Yet, the theory supporting the model relies on certain assumptions. One assumption pertains to preferences and/or production mixes. This issue will be addressed in section 2.2.3 of the paper. Another assumption is that the tradability of all goods are equally responsive to distance.

where:

T_{ei} = trade from exporter e to importer i;

Y_e , Y_i, Y_w = GDP of exporter e, importer i and the world (w);

D_{ei} = distance between countries e and i;

$\beta 1$ = some parameter that we expect to be negative;

Φ_{ei} = a factor capturing the effect of trade barriers other than distance; and

ε_{ei} = the error term.

A useful way of understanding the gravity model is as follows:

Predicted Trade	=	Potential Trade	X	Discount Factor Reflecting Trade Barriers

"Potential trade" refers to the trade that would occur if no barriers to trade existed (in a world of identical, homothetic preferences, and with no trade of intermediate goods). Under these conditions, potential trade is $(Y_e Y_i / Y_w)$. If there were no barriers to trade, we assume that a country's output would be sold to various countries in proportion to those countries' GDP. Similarly we assume the goods a country consumes would be purchased from various countries in proportion to those countries' GDP.

The "discount factor reflecting trade barriers " is the second component of the gravity model. This factor represents 1 minus the proportion of potential trade that is blocked by trade barriers. By "trade barriers", we mean anything that can interfere with trade. The term refers both to government-imposed barriers (such as tariffs and quotas) and to natural barriers (such as transportation costs and the inability to communicate).

The "natural" barriers are strongly correlated with distance. For example, as distance increases,

1. shipping costs increase,

2. responsiveness to customers' orders decreases,
3. perishable products age more,
4. the ability to communicate decreases (because languages of distant countries will usually be less familiar than those of countries that are nearby), and
5. familiarity with opportunities decreases.

For these reasons distance appears to be a good proxy for many "natural" barriers to trade.

Other barriers to trade could also be incorporated into the model if suitable measures or proxies of such barriers can be found. For example, the ability to speak a common language can be incorporated into the model. Other barriers are harder to measure. Government-imposed barriers, such as aggregate tariffs,[4] quotas, and red tape are very difficult to quantify. In the gravity model above, the "discount factor reflecting trade barriers" is ($D_{ei}^{\beta 1}$ x Φ_{ei}).

2.2.2 Restructuring the Gravity Model

Researchers using the gravity model typically use each partner's GDP as an independent variable. That is, they run regressions of the form

$$\ln T_{ei} = \beta_1 \ln Y_e + \beta_2 \ln Y_i + \beta_3 \ln D_{ei} + \ldots + C + \varepsilon_{ei}$$

However, the theory supporting the gravity model indicates that trade should be directly proportional to each trading partner's GDP (after adjusting for distance and other factors). As a result, theory stipulates that β_1 and β_2 ought to equal one. Statistical tests consistently corroborate the theory; they regularly estimate β_1 and β_2 to equal approximately 1.

In light of the above, we modify the gravity model. Since there really is no reason to doubt that trade is proportional to the trading partners' GDP, it makes sense to divide the left hand side and the right hand side of the gravity equation by the term $Y_e Y_i / Y_w$. Consequently we can create a new variable, G_{ei}, as follows:

$$G_{ei} = T_{ei} / (Y_e Y_i / Y_w) = D_{ei}^{\beta 1} \times \Phi_{ei} \times e^{\varepsilon ei}$$

Now the dependent variable no longer represents trade, but rather represents the proportion of potential trade that is realized.

Advantages to this modification are:

1. The revision prevents errors in GDP measurement from biasing our estimates of the parameters in the gravity model. This is important, because most economists doubt the accuracy of the reported GDP amounts of many countries. These inaccuracies can bias our parameter estimates.
2. A country's GDP is partially dependent on trade. As a result, the standard format of the gravity model has a problem with simultaneous equations. Trade is partially dependent on income and income is partially dependent on trade. Moving GDP to the left-hand side of the equation eliminates this problem.
3. The restatement of the model focuses the equation on the barriers to trade, which is what we really intend to examine in our statistical tests.

Disadvantages to making this modification are:

1. The model looses flexibility; the data can no longer attribute a parameter other than 1 to the GDP. If the true parameter differs from 1, this method can bias the immigration coefficient to the extent that GDP is correlated with immigration.
2. If we allowed the data to determine the parameter, and we obtained a parameter estimate significantly different from 1, this result would alert us to the possibility that we are missing an important variable.

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[4] Tariffs on any one product are easy to quantify, but they are hard to aggregate because they vary a lot across products and at a very detailed level.

3. The dependent variable's interpretation is slightly more complex than that of the standard gravity equation. Exports (or imports) are a simpler concept than "the proportion of potential trade that is realized".

We make this modification because we believe the first two advantages strongly outweigh the other considerations. Naturally, our R squared statistics will decrease substantially from the results that the standard format of the gravity model generates, since much of the gravity equation's explanatory power lies in the income component of the model.

2.2.3 Differences in Consumer Preferences and Production Patterns across Canada

In their simplest forms, the supporting theoretical models assume that consumer preferences are identical across countries. Deardorff has considered cases where consumer preferences and production patterns vary across countries. In such cases, potential trade may be higher or lower than ($Y_e Y_i / Y_w$), depending on the degree to which country *e* produces the kinds of goods country *i* prefers.

Consider trade in good *j*. Suppose country *i* spends θ_{ij} of its income on good *j*, and country *e* derives γ_{ej} of its income from the production of good *j*. With no barriers to trade, country *e*'s exports to country *i* in good *j* will be:

$$T_{eij} = Y_e Y_i \, \gamma_{ej} \, \theta_{ij} \, / \, \Sigma_i \theta_{ij} Y_i.$$

Aggregate exports from country *e* to country *i* will therefore be:

$$T_{ei} = \Sigma_j T_{eij} = Y_e Y_i \, \Sigma_j [\gamma_{ej} \, \theta_{ij} \, / \, \Sigma_i \theta_{ij} Y_i].$$

Note that if consumer preferences are identical across countries, the θ_{ij} is constant across all *i*, and $T_{ei} = Y_e Y_i / Y_w$. The same result occurs if production mixes are identical across countries. Potential trade can only deviate from ($Y_e Y_i / Y_w$) if *both* consumer preferences *and* production mixes vary across countries.

Deardorff goes on to show that even where both the exporter's production mix and the importer's preferences deviate from the world average, the expected potential trade will nevertheless tend towards $(Y_e Y_i / Y_w)$ if these deviations are not correlated. On the other hand, if countries have correlated differences in preferences and production mixes that correspond with a variable we wish to study, our statistical tests could generate a biased coefficient for that variable.

In summary, the potential trade part of the model will be appropriate if at least one of the following is true:

1. production mixes are similar across exporters (i.e. all exporters produce the same types of goods in roughly the same proportions);
2. preferences are similar across importers; or
3. differences in importers' preferences and exporters' production mixes are not correlated with the variables on which our study focuses.

Below we assess each of these criteria. We find that the first criterion appears to be false. The other two criteria, however, do not appear to pose a significant problem, though we cannot conclusively demonstrate that they are true.

2.2.3.1 Production Mixes

There clearly are differences in production patterns, across foreign countries and across Canadian provinces. Countries and provinces tend to specialize in the production of certain types of goods. Among provinces these differences are very pronounced. Forestry products dominate British Columbia's exports; the prairie provinces' major exports are agricultural products and oil; Ontario and Quebec export manufactured goods; and the maritime provinces export forestry, fishery and oil products. We clearly cannot assume that Canada's production patterns are similar among provinces. Similarly we cannot assume foreign countries' production mixes are similar to each other.

2.2.3.2 Importers' Preferences

Next we look at importers' preferences. We subdivide this issue into several categories – consumer preferences among provinces, demand for intermediate goods across provinces, consumer preferences among foreign countries, and demand for intermediate goods across foreign countries.

We believe that consumer tastes are similar across Canada. Canadians across the country have a similar level of affluence. While cultures vary somewhat across the country they likely do not materially affect the types of goods that people buy.

However, we need to consider more than the imports of final consumer goods, because intermediate goods account for a substantial proportion of international trade. While we are confident that variations in consumer tastes will not bias our results, we cannot be so confident that the demand for intermediate goods is similar across provinces. The demand for intermediate goods depends on what goods each province's industries need. Patterns of input imports will not vary much across provinces with resource-based economies, because the primary inputs of resource-based industries are not imported. However, the manufacturing provinces will have a different import pattern, with greater imports of manufactured components.

Next we consider whether we can assume consumer preferences are similar across foreign countries with respect to the products those countries would buy from Canada. Because our model uses fixed country effects, this assumption is not altogether unreasonable. We need not assume, for example, that Angola's consumption patterns are similar to Japan's. We just need to assume that the types of goods Angola demands from Canada are similar to the types Japan demands. Non-Canadian suppliers might meet the differences between Angola's and Japan's consumption. For example, developing countries might spend a greater proportion of their income on locally grown food. As a result, such a country might trade less, as a percentage of GDP, than a developed country might. The use of fixed country effects in our model reduces the distortion that this difference in consumption patterns might otherwise have caused.

Finally we turn to foreign countries' imports of intermediate goods. Here we look again to Canada's primary industries and find some reasons to believe that foreign countries' demands for such goods will not be very biased toward certain countries. Of Canada's primary resource-based industries, fisheries and grains are close to being consumer goods, while forestry and oil products are inputs for a wide spectrum of industries. Consequently there is little reason to believe that these exports will be unduly focused on certain countries. There is, however, some difficulty with Ontario's and Quebec's manufactured goods, which are likely primarily demanded by the more industrialized countries.

2.2.3.3 Correlation Between Possible Biases and Immigration or Language

If these variations in production mixes and importers' preferences are not correlated with immigration, the variations will not distort our results. Unfortunately, it is easy to envision a correlation. For example, among the provinces manufactured goods tend to be produced in urban Ontario and Quebec. Immigrants tend to settle in large cities. Industrialized countries may be more likely to trade in industrialized products. If we count all immigrants (rather than only recent immigrants), most immigrants come from industrialized countries. As a result, our statistical tests may attribute to immigrant links what is really attributable to variations in production mixes and importers' preferences.

However, there are several reasons to believe that the above relationship will not materially bias our results. First, there are quite a few industrialized countries with varying levels of emigration into

Canada. These variations among many industrialized countries help diffuse this problem. Second, our use of fixed country effects further helps to diffuse this problem. If industrialized countries trade more in industrial goods, this extra trade is reflected in the fixed country effects.

Obviously, we cannot list every possible correlation one could envision. Nevertheless, after considering whether there may be patterns of trade that are correlated with immigration, and after considering fixed country effects, we believe that our statistical tests will not be materially affected by variations in production mixes and importers' preferences.

2.2.4 Province-Level Data

2.2.4.1 Benefits of Fixed Effects

In a previous study, Head and Ries looked at the immigration effects on Canada's trade. They did so by employing a standard logged gravity model and including as an independent variable the log of the number of immigrants in Canada from the particular trading partner. Their analysis shows that increasing immigration from a particular country increases trade with that country, with elasticities of approximately 0.1 for exports and 0.3 for imports. As with any regression analysis, there is a risk that Head & Ries' study does not measure what it appears to measure. There are alternative hypotheses that might explain their results. One hypothesis is that a handful of countries with high levels of immigration into Canada (such as Great Britain and Hong Kong) drive the result. There may be other relationships between Canada and the foreign country that influence both immigration and trade.

The risk that we are measuring factors other than immigration can be greatly reduced by introducing fixed country effects into the model. We can do this because we have provincial level immigration and trade data. For example, consider trade with Hong Kong. Most immigrants from Hong Kong settle in Ontario or British Columbia.[5] If there is an immigration effect, Hong Kong's trade with these provinces (after accounting for province size and distances) should be greater than its trade with other provinces. By using provincial data and fixed country effects, we eliminate the possibility that we attribute to immigration an increase in trade that really is attributable to some other special relationship between Canada and another country. Of course, there remains a risk that there are special relationships between the other country and a particular province, but we believe such relationships are less significant. The primary special relationships normally involve issues at the national level, such as political relationships and tariff levels.

In addition to enabling us to use fixed country effects, the use of provincial level data is appealing for another reason. We can consider language variation, since Quebec is predominantly French speaking while the other provinces are predominantly English speaking. We consider 28 other languages as well to capture the first language of most immigrants. Appendix 2.1 lists these languages.

2.2.4.2 Measurement Problems

Unfortunately, we have a trade measurement problem in our provincial data; some of the import trade data is allocated to the wrong province. Statistics Canada says the following about its import statistics:

> Import statistics by province of clearance indicate the province in which goods were cleared by Customs either for immediate consumption or for entry into a bonded Customs warehouse. This may not always coincide with the province in which goods are consumed.[6]

For our purposes, we would have liked the imports to be reported in the province in which the goods are consumed.

[5] In 1995 about 250,000 immigrants from Hong Kong were living in Canada. About 50% of those immigrants lived in Ontario and 32% lived in British Columbia.

We analyzed the trade patterns of some specific products to see whether we could identify any potential problems. Automobile imports were clearly not reported on the basis of consumption, but on the basis of the port of entry. (Virtually all automobiles imported from Europe are reported as Nova Scotia's imports; virtually all automobiles imported from the United States show up as Ontario's imports; and virtually all automobiles from Japan are included in British Columbia's imports.) Other products (such as wine) appear to be reported on the basis of the province of consumption.

In the case of *exports*, Statistics Canada says:

> ... for most commodities, trade data are presented by province of origin which represents the province in which the goods were grown, extracted or manufactured.

We have not noted any industries in which the export trade patterns look unreasonable.

We have no practical way of countering this problem. All we can do is remain cognizant of these errors when interpreting the results of our statistical tests.

2.2.4.3 Small Provinces

Some of the provinces are quite small. To avoid attaching too much weight to the effects of small provinces, we group them into regions. Our groupings follow Statistics Canada's normal groupings of the provinces. The "provinces" we use are:

(1) British Columbia,

(2) the prairie provinces (Alberta, Saskatchewan and Manitoba),

(3) Ontario,

(4) Quebec, and

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[6] Statistics Canada, *Imports by country, Jan. – Dec. 1994*, No 65-006, pg xiv.

(5) the maritime provinces (New Brunswick, Nova Scotia, Prince Edward Island and Newfoundland).

Throughout this paper, the term "provinces" refers to these three provinces and two regions. For each foreign country, there are 5 observations per year. Consequently, each of the 160 foreign countries will have 20 observations (5 provinces times 4 years).

2.2.5 Zeros

Zeros in trade data should not be ignored. The zeros may bias our coefficient estimates if we do not take steps to counter this problem. The reason for this bias relates to the functional form we use to run our statistical tests. The dependent variable in the standard logged gravity equation is $\ln(T_{ei})$, so when $T_{ei} = 0$, then $\ln(T_{ei})$ is undefined. Computerized regression programs typically ignore observations with undefined values. Casting out these observations triggers a selection bias. Some researchers try to sidestep this problem by making the dependent variable $\ln(T_{ei}+1)$. But, this form still exaggerates the significance of trade level fluctuations that are at zero. For example, in real life there is little difference between trade of 0 and trade of \$1,000, even between small trading partners. Yet this difference will affect our regressions markedly – much more than the difference between \$10 million and \$1 billion.[7]

We use a Probit estimation to compute each observation's probability that there will be zero trade, and use those probabilities to compute Mills' Ratio for each observation to correct the bias that would otherwise arise.

[7] ln(1,001)-ln(1)=6.9, but ln(1,000,000,001)-ln(10,000,001)=4.6.

2.3 CANADIAN IMMIGRATION AND WHY WE EXPECT A LINK TO TRADE

2.3.1 Description of the Immigration Environment in Canada

At the time of the 1996 census, immigrants comprised 17% of Canada's population. (The US's immigrant population is 8% of its total.) In the 1950's, 80% of immigrants came from Europe. Now most come from Asia; 66% of immigrants came from Asia in 1996-97. The most common sources of recent arrivals (since 1980) are Hong Kong, India and China, while the most common sources across all immigrants (whether recent or not) are Great Britain, Italy and the United States.

The most popular destination provinces for immigrants are Ontario (where 25% of the 1996 population were immigrants) and British Columbia (where 24% of the 1996 population were immigrants).

Between 1992 and 1995 the major classes of immigrants were as follows:

- Family assisted - 52.1% (individuals sponsored by family members who are Canadian citizens or residents);
- Independent - 19.8% (includes assisted relatives and self-employed individuals);
- Refugee - 12.0%;
- Entrepreneur - 6.4% (Entrepreneurs must establish a business in Canada that employs at least one person other than a family member);
- Investor - 4.2% (An investor must make an investment of $250,000-$500,000 (depending on various factors)[8]); and
- Other Categories - 5.5%.

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[8] In April 1999 these amounts were increased to $400,000-$800,000.

Independent, entrepreneur and investor immigrants are all admitted on economic grounds. Admission into Canada is based on a point system that is intended to measure the degree to which the prospective immigrant can contribute to the Canadian economy. The primary objective of the economic immigration program is to increase employment in Canada, particularly in regions of high unemployment. However, one of the secondary objectives is to improve Canada's trade position. Immigration officials look more favourably on entrepreneurs who will export, or who will produce in Canada goods that were previously imported.

2.3.2 Why We Expect an Immigration Link

2.3.2.1 Theoretical

There are two primary theoretical reasons why we might expect a positive correlation between immigration and trade.

The first reason applies only to imports, not exports. Immigrant consumers may have a preference for certain goods that are produced in their country of origin. For example, immigrants may prefer ethnic foods produced in the country from which they immigrated.

The second reason applies both to imports and exports. There are some barriers to international trade that might not apply to immigrants, or that might be less costly to immigrants. Such barriers might include unfamiliarity with business laws in the other country; unfamiliarity with business customers in the other country; unfamiliarity with supply sources in the other country; difficulty in establishing business networks in the other country; unfamiliarity with the special needs and preferences of customers in the other country; and unfamiliarity with the language of the other country. Immigrants from the particular country would be less affected by these barriers - or might not be affected by some of them at all. Thus immigrants may be in a better position than other people to conduct trade with their countries of origin.

2.3.2.2 Informal Observation

In Canada it is widely believed that there is a link between immigration and trade.[9] It is easy to find anecdotal evidence of immigrant businesspeople who conduct trade with their countries of origin. In addition, the substantial immigration from Asia, coupled with the rise in trade with Asian countries, appears to support this belief.

2.3.2.3 Immigration Policy

Canada's immigration policy favours entrepreneurs and other people who will expand Canada's economy. Immigrants of the entrepreneur classification must persuade immigration officials that they can and will set up a suitable business. Very often, such businesses involve exporting, since one of the competencies of an immigrant will usually be knowledge of the home country's market. In a sense, then, expansion of exports is almost a condition of immigration for some individuals.

2.4 LANGUAGE

Since it is testable, we hypothesize that part of immigrants' advantage in carrying out trade with their countries of origin is knowledge of the language. To carry out this test, we computed a common language variable. For a particular country-province pair, this variable equals the probability a randomly chosen person from the foreign country and a randomly chosen person from the province, can speak the same language. For example, suppose the foreign country is Morocco and the province is Quebec. In Morocco, 20% of the population speaks French and 74% speaks Arabic. In Quebec 92.4% speaks French and 1.1% speaks Arabic. As a result, the common language variable

[9] See, for example, Time (Canadian edition) – "Asia's New Capital", pg 31, Nov 17/97.

for Morocco-Quebec observations would equal 0.193 (20% x 92.4% + 74% x 1.1%).[10] (See Appendix 2.1 for a description of our sources of language data.)

2.5 STATISTICAL RESULTS

We test the links between immigration and trade using three different models. The first model is the simple log model that Head and Ries used in their 1998 paper. We then derive two new models to predict how immigration might affect trade. These two models are based on the reasoning described in section 2.3.2 above. We then run statistical tests using specifications based on each of these models.

We have obtained data on each province's trade with each foreign country and on immigration to each province from each country. Appendix 2.1 reports our data sources.

For each specification we run six tests – three involving exports and three on imports. Out of the three tests, the first test considers neither fixed country effects nor language effects. The next test considers fixed country effects, but still does not consider language effects. The last test constitutes the full model – with fixed country effects and language effects.

2.5.1 Head & Ries Specification

Head and Ries used a simple log specification. In this paper we adapt their specification to incorporate provincial data and adopt a dependent variable of $\ln(G_{fp})$ as described in section 2.2.2. Our specification is as follows:

No fixed country effects: Reported in columns 1 and 4 of table 2.1

$$\ln G_{pf} = \beta_1 \ln IMM_{pf} + \beta_2 NID_{pf} + \beta_3 \ln D_{pf} + \beta_4 MILLS_{pf} + \beta\Lambda_{pf} + \varepsilon_{ei}$$

[10] This formula double counts individuals who speak both languages. We do not adjust for this double counting because we do not have the data necessary to do so. This measurement error is likely immaterial.

With fixed country effects: Reported in columns 2 and 5 of table 2.1

$$\ln G_{pf} = \beta_1 \ln IMM_{pf} + \beta_2 NID_{pf} + \beta_3 \ln D_{pf} + \beta_4 MILLS_{pf} + \beta\Lambda_{pf} + FE_f + \varepsilon_{ei}$$

With fixed country effects and language effects: Reported in columns 3 and 6 of table 2.1.

$$\ln G_{pf} = \beta_1 \ln IMM_{pf} + \beta_2 NID_{pf} + \beta_3 \ln D_{pf} + \beta_4 MILLS_{pf} + \beta\Lambda_{pf} + FE_f + \beta_5 COMLANG_{pf} + \varepsilon_{ei}$$

where

IMM_{fp} = the number of immigrants from foreign country f living in province p,

$\ln IMM_{fp}$ is overridden to equal 0 if the number of immigrants is less than or equal to 5,

NID_{fp} = "no immigrant dummy" and equals 1 if the number of immigrants in province p from country f is no more than 5, but equals 0 if the number of immigrants exceeds 5.[11]

D_{pf} = the distance between the province and the foreign country,

β = the row vector of coefficients for the respective elements in Λ_{pf},

Λ_{pf} = the column vector of dummy variables for provinces, years and the constant,

$MILLS_{fp}$ = Mills' ratio,

FE_f = fixed effect for country f, and

$COMLANG_{fp}$ = the common language variable described in section 2.4.

Head and Ries chose the log specification because it is simple and it has the attractive feature that β_1 can be interpreted as an elasticity. However, these attractive features come at the expense of consistency with theory. There really doesn't seem to be any theoretical justification for believing that trade should be proportional to the number of immigrants from that country to the power of some coefficient. Yet, this specification produces very strong results.

[11] The "no immigrant dummy" is needed to cover the case where there are no immigrants. If IMM_{fp}=0, then $\ln IMM_{fp}$ becomes undefined. A province is considered to have no immigrants if there are 5 or less, because Citizenship and Immigration Canada altered some figures by plus or minus 5 to protect the anonymity of the immigrants in the data set.

Table 2.1
Simple Log Specification

	Exports			Imports		
	No Fixed Country Effects and No Language Effects	With Fixed Country Effects, but No Language Effects	With Fixed Country Effects and Language Effects	No Fixed Country Effects and No Language Effects	With Fixed Country Effects, but No Language Effects	With Fixed Country Effects and Language Effects
$\ln IMM_{fp}$	0.131 se=.015;p=.000 *se*=.023; p*=.000*	0.088 se=.028;p=.002 *se*=.040; p*=.029*	0.081 se=.030;p=.008 *se*=.045; p*=.073.*	0.429 se=.029;p=.000 *se*=.048; p*=.000*	0.273 se=.048;p=.000 *se*=.081; p*=.001*	0.253 se=.052;p=.000 *se*=.086; p*=.003*
NID_{fp}	-0.31 se=.16;p=.055	0.33 se=.16;p=.047	0.32 se=.16;p=.051	1.62 se=.30;p=.000	0.74 se=.28;p=.010	0.72 se=.28;p=.011
COMLANG			0.16 se=.25;p=.509			0.42 se=.41;p=.312
$\ln(D_{ei})$	-1.16 se=.07;p=.000	-1.85 se=.12;p=.000	-1.85 se=.12;p=.000	-0.95 se=.11;p=.000	-1.42 se=.21;p=.000	-1.43 se=.21;p=.000
MILLS	0.57 se=.18;p=.001	-0.15 se=.17;p=.362	-0.15 se=.17;p=.381	-0.64 se=.39;p=.097	0.77 se=.28;p=.006	0.78 se=.28;p=.006
Adj. R^2	0.190	0.460	0.460	0.252	0.514	0.514
Root MSE	1.51	1.24	1.24	2.56	2.06	2.06
# of obs.	2,556	2,556	2,556	2,545	2,545	2,545

Notes: Figures shown in large fonts are the coefficient estimates. In small fonts beneath each estimate we report the estimate's estimated error and the corresponding probability that we would get such an estimate if the true coefficient were 0. For $\ln IMM_{fp}$, the robust standard error (se*) is also reported (using the robust/cluster feature in STATA), along with the corresponding probability statistic. Statisitcs for the dummy variables and the constant are not shown.

As reported in table 2.1, the coefficients for $\ln IMM_{fp}$ are very similar to those of Head and Ries (1998). For exports, we obtain a coefficient of 0.131 when fixed effects are not considered (column 1), which is close to Head & Ries' result of 0.099. When we introduce fixed country effects the coefficient estimate decreases to the 0.081 to 0.088 range (columns 2 and 3). According to the standard ordinary least squares (OLS) regression, these immigration effects are statistically significant at a 99% significance level even with fixed country effects.

However, within province-country pairs of trading partners, residuals from year to year are correlated with each other. Regressing residuals on one-year-lagged residuals yields a coefficient of 0.67. Switching the independent variable to a two-year-lag yields a coefficient of 0.60 and a three-year-lag yields 0.55. All these residual regressions are highly significant. Since these coefficients decline somewhat as the years grow farther apart, there appears to be some serial correlation and possibly some other correlation amongst observations involving the same trading partners. Consequently, we reran the regression without assuming the observations are independent within province-country pairs by using the robust/cluster feature of STATA to compute robust errors. The robust errors (reported in italics in table 2.1) are about 50% larger than the estimated errors produced by the OLS regression. Using these estimated errors reduces the significance of the immigration variable. Without fixed effects the variable is still significant at the 99% level, but with fixed effects the significance level falls to 95% before considering language, and falls below 95% when considering language.

The coefficient for $\ln IMM_{fp}$ can be interpreted as an elasticity. A coefficient of 0.08 implies that a 10% increase in immigrants from a country will be associated with a 0.8% increase in exports to that country.

We now turn to imports. Without fixed country effects we estimate the coefficient for $\ln IMM_{fp}$ at 0.429 (column 4). This result is again fairly close to Head and Ries' result of 0.309. The introduction of fixed country effects reduces our coefficient estimate to about 0.25 (columns 5 and 6). These coefficient estimates are statistically significant at the 99% level even with fixed effects and even using robust errors. A coefficient of 0.25 implies that a 10% increase in immigrants from a country is associated with a 2.5% increase in imports from that country.

Columns 3 and 6 introduce language effects. For both exports and imports, language effects are not statistically significant. This finding surprised us, since our prior expectation was that language differences pose a major barrier to trade. This result differs from the findings of other researchers using the gravity model (and my own findings in chapter 1), who find that common languages significantly increase trade levels. We carried out some other tests and found that our insignificant language results are restricted to the case where the regression includes both fixed effects and immigration. If we remove fixed effects from the regression, the common language variable is statistically significant with a coefficient of 0.89 and a standard error of 0.11 when predicting for exports, and with a coefficient of 0.80 and a standard error of 0.18 when predicting for imports. (These results are not reported in a table.) If instead we retain fixed effects but remove immigration variables, then the common language variable has strong prediction value for imports, but is not a particularly strong predictor for exports. In export predictions, the coefficient is 0.38 with a standard error of 0.23, making the variable statistically significant only at the 90% level; in import predictions, the coefficient is 1.16 with a standard error of 0.38, making the variable statistically significant at the 99% level. The lower coefficient for exports may be attributable to Canada's high export levels of homogenous goods, where common languages may be less important to trade.

These results *may* be suggesting that the immigration effect dominates the language effect. Thus the prime benefit of immigrants might be their access to networks rather than their knowledge of foreign languages. However, as shown in Appendix 2.2, the common language variable is highly correlated with immigration stocks.

Another surprising result is that the introduction of fixed effects increases the magnitude of the coefficient estimate for the log of distance. This result surprised us because it implies that distance affects trade levels within Canada more than it does between Canadian shores and a foreign country. This inference follows from the fact that the effect of distance between Canada and a foreign country is fully captured by the fixed country effects. For example, fixed country effects fully reflect the fact that Poland is farther than Ireland from Canada. The distance variable therefore reflects the trade resistance associated with the fact that, for example, Ireland is farther from British Columbia than from Quebec. There are several possible explanations for the increased magnitude in the distance coefficient.

Inaccurate Data

As stated in section 2.2.4.2. of the paper, our data do not always attribute imports to the province in which the goods were consumed. If a significant proportion of imports is reported in the province nearest to the exporter, rather than in the province of consumption, the absolute value of the distance coefficient would naturally be inflated. This explanation would seem rather convincing except for the fact that the unusual distance coefficient occurs not only in the import regressions, but also the export regressions. As reported earlier, we found no reason to believe reporting errors exist in the export data.

We conducted a simple little test to see whether the inaccurate reporting of auto imports might be inflating our distance parameters. (Auto trade comprises about 20% of Canada's imports.) In our simple test we removed the primary auto-exporting countries from the regression (the United States, Japan and Germany). When we did this, we surprisingly found that the absolute value of the distance coefficient actually increased slightly.[12] So it appears that the magnitude of the distance coefficient is not fully explained by this measurement error.

Cross-Canada Transportation Costs

Another explanation is that shipping to Canada by sea is not very costly, while shipping within Canada (by rail or truck) may be costly.

Substitution Effects

Another possibility (which only applies to exports) is that the products of the west coast and the products of the east coast are substitutes of each other. The main industries at both ends of the country are forestry and fisheries. Thus it would seem unlikely that Europe would buy lumber from British Columbia when it can be obtained from the maritime provinces. Similarly, it would seem unlikely that the maritime provinces would export to Asia goods that can be purchased from BC. But the high distance parameter occurs both for imports and exports. So this explanation does not satisfy us.

Our best explanation is that all of the above reasons contribute to the high distance coefficients.

These regressions also employ dummy variables for the provinces, and in columns 2, 3, 5 and 6 employ fixed country effects. These dummy variables and fixed country effects capture the effects of the trading partners' levels of development on trade volumes. Many researchers use GDP-per-capita variables to control for this effect, but because our tests already controlled for this effect we excluded these commonly used variables. Had these tests included GDP-per-capita variables, the outcome would not have changed materially. For example, when modifying the regression reported in column 2 (with fixed country effects) to include logged GDP-per-capita variables, the coefficients for those variables are not statistically significant and the coefficient for the log of immigrants decreases from 0.088 to 0.087.

[12] The results of this regression are not shown. The absolute value of the coefficient estimate increased marginally from 1.42 (Table 2.1, column 5) to 1.44. Meanwhile the coefficient on the log of immigrants

As discussed earlier, the dependent variable equals the logged "proportion of potential trade that is realized", rather than the conventional logged exports (or imports). Had the conventional model been used, with the trading partners' logged GDPs on the right hand side of the equation, the coefficients for the logged immigrants variable change very little, particularly with fixed country effects. Using the more conventional approach the immigrant coefficient for column 1 would be 0.156 instead of the 0.131 reported in Table 2.1. With fixed effects in columns 2 and 3, the coefficients would be 0.088 and 0.080, which are virtually identical to the results reported in Table 2.1. Similarly for imports the revised coefficients would be 0.410, 0.273 and 0.253 (versus 0.429, 0.273 and 0.253 reported in Table 2.1).

In addition, many researchers include the lagged dependent variable as an independent variable. This variable is recommended by Eichengreen and Irwin (1996), who contend that the gravity model is often used without adequate care for considering omitted variables, which can lead to erroneous conclusions. Omitted variables can involve persistent errors for a pair of trading partners or can involve historical relationships. Eichengreen and Irwin conclude that the gravity model should always have the lagged dependent variable as a regressor. However, missing variables can often be handled in other ways that permit a more natural interpretation of the coefficient estimates. In this paper we use fixed country effects to capture omitted variables.

For comparison, we introduced the lagged dependent variable to check the robustness of our results reported in Table 2.1. In the case of exports without fixed effects, the coefficient on $\ln IMM_{fp}$ remains statistically significant at the 99% level, but decreases from 0.131 (reported in Table 2.1) to 0.033 (not reported in a table). According to Head and Ries (1998), using the lagged dependent variable allows immigration to affect trade in two ways – directly and

decreased from 0.273 to 0.262.

indirectly. The direct effect comes from the immigration variable itself while the indirect effect comes through the lagged dependent variable, because immigration affected prior years' trade, which affects the current year's trade. Based on this reasoning, Head and Ries computed the total effect of immigration to be $\beta/(1-\lambda)$, where β is the coefficient estimate for the immigration variable and λ is the coefficient estimate of the lagged dependent variable. In this case $\beta=0.033$ and $\lambda=0.671$, producing a total effect of 0.100, which is somewhat lower than the coefficient estimate reported in Table 2.1. With imports, the coefficient estimate for immigration again remains statistically significant and the computed total elasticity of immigration amounts to 0.247, which is lower than the 0.429 reported in Table 2.1. The use of the lagged dependent variable yields lower, but significant results.

2.5.2 "Propensity" Specification

2.5.2.1 Derivation

We now introduce two new specifications and provide some theoretical justification for them. The first of the two specifications assumes that a representative immigrant has a propensity to trade that is 1+z times as much as a representative non-immigrant. We would expect an immigrant to be more likely to conduct trade with his/her country of origin for the reasons described in section 2.3.2.

1. Immigrants enjoy a lower cost of trade than non-immigrants do. Because immigrants face lower costs, they can carry out some trade that non-immigrants cannot carry out.
2. For imports, we would expect that the immigrant would have a higher propensity to trade than a non-immigrant would because the immigrant might have a preference for goods produced in his/her country of origin.
3. Canada's immigration policy seeks entrepreneurs who are more likely to trade than the average person.

Based on the above, we would assume that total trade would be

$$T_{pf} = [(IMM_{pf}/POP_p)\,(Y_p\,Y_f/\,Y_w)\,R_{pf}\,(1+z) + (1 - IMM_{pf}/POP_p)\,(Y_p\,Y_f/\,Y_w)\,R_{pf}] \text{ x } e^{\varepsilon fp}$$

where

POP_p = the population of province p, and

$R_{pf} = D_{pf}^{\beta 1} \text{ x } e^{FEf + \beta\Lambda + C}$

This equation simplifies to the specification of

$$G_{pf} = R_{pf}\,[1+z(IMM_{pf}/POP_p)] \text{ x } e^{\varepsilon pf}$$

where

$$G_{pf} = T_{pf}\,/\,(Y_p\,Y_f/\,Y_w).$$

Unfortunately, this specification cannot be transformed into a linear relationship. This specification must either be solved iteratively (using a maximum likelihood estimation) or using an approximation. $[1+z(IMM_{pf}/POP_p)]$ can be approximated by $e^{z(IMMfp/POPp)}$ provided z and (IMM_{pf}/POP_p) are sufficiently small. Unfortunately, for major sources of immigrants, these numbers are not sufficiently small. For example, our estimates of z range from 18 to several thousand, and (IMM_{pf}/POP_p) can be as high as 0.046 in the case of British Columbia/United Kingdom observations. If we considered a z of say 50 and the case where (IMM_{pf}/POP_p)=.046, the approximation $e^{z(IMMfp/POPp)}$ is 3 times as big as $[1+z(IMM_{pf}/POP_p)]$.

We run our statistical tests for this specification in two ways. We first use the approximation so that we can use an ordinary linear regression and later we use a maximum likelihood estimate to calculate z in the term $[1+z(IMM_{pf}/POP_p)]$.

2.5.2.2 Regression Results – Using an Approximation

As indicated above, the first set of tests for this specification employs the approximation. Table 2.2 reports the results of the regressions run using the following specifications:

Columns 1 and 4:

$$\ln(G_{pf}) = \beta_1(IMM_{pf}/POP_p) + \beta_2 \ln D_{pf} + \beta\Lambda_{pf} + \beta_3 MILLS_{pf} + \varepsilon_{pf}$$

Columns 2 and 5:

$$\ln(G_{pf}) = \beta_1(IMM_{pf}/POP_p) + \beta_2 \ln D_{pf} + \beta\Lambda_{pf} + \beta_3 MILLS_{pf} + FE_f + \varepsilon_{pf}$$

Columns 3 and 6:

$$\ln(G_{pf}) = \beta_1(IMM_{pf}/POP_p) + \beta_4 COMANG_{fp} + \beta_2 \ln D_{pf} + \beta\Lambda_{pf} + \beta_3 MILLS_{pf} + FE_f + \varepsilon_{pf}$$

Columns 1 and 4 report results when neither fixed country effects nor language effects are considered. Columns 2 and 5 introduce fixed country effects (FE_f), and columns 3 and 6 add the language effect.

For exports without fixed country effects, the estimated coefficient for IMM_{fp}/POP_p is 39.8. This estimate implies that an immigrant is about 40 times more likely to export to his/her country of origin than other people are. This result is significant at the 99% level, even using robust errors. Table 2.6 shows how these results translate into dollars. The results of this statistical test suggest that immigrants account for $16 billion (US) out of $150 billion (US) of Canada's annual exports.

The estimate for imports is even higher at 94.5, and is also statistically significant at the 99% level. These results suggest that immigrants import from their home countries about 100 times as much as other Canadians do. As reported in Table 2.6, this test suggests that $51 billion (US) out of $139 billion (US) of Canada's imports are attributable to immigrants.

Table 2.2
Approximation of "Increased Propensity" Specification

	Exports			Imports		
	No Fixed Country Effects and No Language Effects	With Fixed Country Effects, but no Language Effects	With Fixed Country Effects and Language Effects	No Fixed Country Effects and No Language Effects	With Fixed Country Effects, but no Language Effects	With Fixed Country Effects and Language Effects
IMM_{fp}/POP_p	39.8 se=9.5;p=.000 *se*=13.8; p*=.003*	26.4 se=12.4;p=.033 *se*=13.5; p*=.051*	23.3 se=12.6;p=.065 *se*=14.1; p*=.098*	94.5 se=16.5;p=.000 *se*=28.4; p*=.001*	19.3 se=20.8;p=.355 *se*=17.7; p*=.277*	6.9 se=21.2;p=.746 *se*=17.7; p*=.698*
COMLANG			0.29 se=.231;p=.216			1.13 se=.39;p=.004
$\ln(D_{ei})$	-1.19 se=.07;p=.000	-1.89 se=.12;p=.000	-1.89 se=.12;p=.000	-1.02 se=.12;p=.000	-1.63 se=.20;p=.000	-1.62 se=.20;p=.000
MILLS	-0.31 se=.13;p=.020	-0.12 se=.16;p=.440	-0.11 se=.16;p=.487	-2.76 se=.24;p=.000	0.95 se=.28;p=.001	0.97 se=.28;p=.000
Adj. R^2	0.161	0.459	0.459	0.190	0.508	0.510
Root MSE	1.54	1.24	1.24	2.66	2.08	2.07
# of obs.	2,556	2,556	2,556	2,545	2,545	2,545

Notes: Figures shown in large fonts are the coefficient estimates. In small fonts beneath each estimate we report the estimate's estimated error and the corresponding probability that we would get such an estimate if the true coefficient were 0. For $\ln IMM_{fp}$, the robust standard error (se*) is also reported (using the robust/cluster feature in STATA), along with the corresponding probability statistic. Statisitcs for the dummy variables and the constant are not shown.

These coefficient estimates are large. However, since we obtained these results without considering fixed country effects, there is a risk that these relationships are attributable to factors that increase both immigration and trade. Incorporating fixed effects reveals that the initial results may be inflated. With fixed effects the results weaken in two ways. First, the coefficient estimates decrease. For exports they decrease from the about 40 to the 20's (columns 2 and 3). For imports they decrease from about 100 to less than 20 (columns 5 and 6). Second, the level of significance weakens. For exports the coefficient estimate remains statistically significant at the 90% level, but for imports the coefficient estimate is not statistically significant.

In columns 3 and 6, we add in the language effect. This modification brings the coefficient for the immigration effect down even further, and also weakens the significance of the results somewhat.

To summarize, the introduction of fixed country effects eliminates the significance of immigration on imports, but not on exports. The common language coefficient estimate is not statistically significant for exports, but is for imports.

2.5.2.3 Results - Maximum Likelihood Specification

The "propensity" specification was rerun using a maximum likelihood estimate method of estimating the parameters. Table 2.3 shows these results for exports and imports.

The equation for columns 1 and 4 is[13]:

$$\ln(G_{pf}) = (1 + \beta_1 IMM_{pf}/POP_p) + \beta_2 \ln D_{pf} + \beta\Lambda_{pf} + \varepsilon_{pf}$$

Introducing fixed countries effects for columns 2 and 5 gives us:

$$\ln(G_{pf}) = (1 + \beta_1 IMM_{pf}/POP_p) + \beta_2 \ln D_{pf} + \beta\Lambda_{pf} + FE_f + \varepsilon_{pf}$$

Finally, the inclusion of language effects for columns 3 and 6 yields:

$$\ln(G_{pf}) = (1 + \beta_1 IMM_{pf}/POP_p) + (1 + \beta_4 COMLANG_{pf}) + \beta_2 \ln D_{pf} + \beta\Lambda_{pf} + FE_f + \varepsilon_{pf}$$

We immediately observe that the immigration parameter increases and its significance decreases. The results are so large that they appear implausible.[14] It is difficult to believe that the average immigrant has a propensity to import that is hundreds of times that of a non-immigrant (let alone several thousand times).

[13] Mills' Ratio was dropped in the MLE computations due to the complexity involved in doing MLE's.

[14] The fact that the estimates have increased should not be surprising. For table 2.2 we used $e^{z(IMM_{fp}/POP_p)}$ as an approximation of $[1+z(IMM_{pf}/POP_p)]$. The approximation tends to exaggerate the effect of observations where the immigrant per capital ratio is large. Therefore, z is depressed to offset this exaggeration and get the closest fit. Figure 2.1 shows a graph comparing these specifications.

Table 2.3 MLE of "Propensity" Specification						
	Exports			Imports		
	No Fixed Country Effects and No Language Effects	With Fixed Country Effects, but no Language Effects	With Fixed Country Effects and Language Effects	No Fixed Country Effects and No Language Effects	With Fixed Country Effects, but no Language Effects	With Fixed Country Effects and Language Effects
IMM_{fp}/POP_p	104.9 se=29.2;p=.000	56.6 se=34.6;p=.102	45.1 se=31.7;p=.155	7,271 se=1416;p=.000	569 se=360;p=.113	332 se=283;p=.240
COMLANG			0.30 se=.30;p=.315			0.80 se=.72;p=.269
$\ln(D_{ei})$	-1.19 se=.07;p=.000	-1.88 se=.12;p=.000	-1.88 se=.12;p=.000	-0.95 se=.12;p=.000	-1.44 se=.21;p=.000	-1.47 se=.21;p=.000
σ^2	2.36 se=.07;p=.000	1.43 se=.04;p=.000	1.43 se=.04;p=.000	6.90 se=.19;p=.000	4.04 se=.11;p=.000	4.03 se=.11;p=.000
Log Likelihood	-2,374.3	-1,735.1	-1,734.5	-3,730.2	-3,047.9	-3,046.8
# of obs.	2,556	2,556	2,556	2,545	2,545	2,545

Notes: Figures shown in large fonts are the coefficient estimates. In small fonts beneath each estimate we report the estimate's estimated error and the corresponding probability that we would get such an estimate if the true coefficient were 0.

The implausible immigration results in table 2.3 are not statistically significant when we consider fixed country effects.

2.5.3 "Probability" Specification

2.5.3.1 Derivation

Our last specification is the probability specification. This specification is rooted in the idea that an immigrant may face lower costs to trade than non-immigrants, due to connections to business networks. Not all immigrants would have such connections, so we assume each immigrant has a probability p of having the connections necessary to advantageously carry out a trade. The probability specification assumes that there are a multitude of trading opportunities between a country and a province. The size of the trading opportunities is assumed to be proportional to the GDP of the exporting province (or country) and proportional to the GDP of the importing country (or province). These trading opportunities can be divided into two classes – the easy opportunities and the hard opportunities. There are α times as many hard opportunities as easy opportunities. The easy opportunities do not require the facilitation of an immigrant. We assume that trade is carried out for all the easy opportunities.

The hard opportunities require the facilitation of an immigrant. Moreover, the immigrant needs to have skills, knowledge or connections for a particular industry both in the country of origin and in the Canadian province. For a specific hard trade opportunity, one immigrant has a probability p of being able to facilitate the trade. So the probability that there exists at least one immigrant who can facilitate a potential hard trade opportunity is $1-(1-p)^{IMM_{pf}}$. This specification implies that there are diminishing returns on immigration.

The full model is:

$$G_{pf} = R_{pf}\,[1+\alpha(1-(1-p)^{IMM_{pf}})] \text{ x } e^{\varepsilon_{pf}}$$

Like the previous specification, this model requires a maximum likelihood estimation.

2.5.3.2 Results

The "probability" specification was run using a maximum likelihood estimate, and is reported on table 2.4. The equations for columns 1 and 4, columns 2 and 5 and columns 3 and 6 are as follows:

$$\ln(G_{pf}) = [1 + \alpha(1-(1-p)^{IMM_{pf}})] + \beta_2 \ln D_{pf} + \beta\Lambda_{pf} + \varepsilon_{pf},$$

$$\ln(G_{pf}) = [1 + \alpha(1-(1-p)^{IMM_{pf}})] + \beta_2 \ln D_{pf} + \beta\Lambda_{pf} + FE_f + \varepsilon_{pf}, \text{ and}$$

$$\ln(G_{pf}) = [1 + \alpha(1-(1-p)^{IMM_{pf}})] + (1 + \beta_4 COMLANG_{pf}) + \beta_2 \ln D_{pf} + \beta\Lambda_{pf} + FE_f + \varepsilon_{pf}.$$

Our results for both variables (α and p) are statistically significant at the 90% level or better in all six columns. (Robust errors are not computed due to programming problems associated with the maximum likelihood estimation computation. Based on robust errors computed in the OLS regressions, it appears reasonable to believe that in this test the robust errors would not exceed 150% of the estimated errors reported in table 2.4. If robust errors are 150% of the reported estimated errors, then variable p would not be statistically significant in the fixed effect models.)

Table 2.4
MLE of "Probability" Specification

	Exports			Imports		
	No Fixed Country Effects and No Language Effects	With Fixed Country Effects, but no Language Effects	With Fixed Country Effects and Language Effects	No Fixed Country Effects and No Language Effects	With Fixed Country Effects, but no Language Effects	With Fixed Country Effects and Language Effects
α	1.33 se=.20;p=.000	0.77 se=.25;p=.002	0.73 se=.24;p=.003	19.96 se=3.61;p=.000	3.48 se=1.32;p=.008	3.00 se=1.18;p=.011
P	0.00200 se=.00066 p=.002	0.00048 se=.00025 p=.058	0.00048 se=.00026 p=.061	0.00039 se=.00009 p=.000	0.00015 se=.00007 p=.028	0.00016 se=.00007 p=.030
COMLANG			0.20 se=.26;p=.453			0.70 se=.60;p=.246
$\ln(D_{ei})$	-1.16 se=.07;p=.000	-1.85 se=.12;p=.000	-1.85 se=.12;p=.000	-1.03 se=.11;p=.000	-1.46 se=.20;p=.000	-1.46 se=.20;p=.000
σ^2	2.29 se=.06;p=.000	1.42 se=.04;p=.000	1.42 se=.04;p=.000	6.54 se=.18;p=.000	4.00 se=.11;p=.000	4.00 se=.11;p=.000
Log Likelihood	-2,336.7	-1,728.9	-1,728.6	-3,662.9	-3,036.4	-3,035.3
# of obs.	2,556	2,556	2,556	2,545	2,545	2,545

Notes: Figures shown in large fonts are the coefficient estimates. In small fonts beneath each estimate we report the estimate's estimated error and the corresponding probability that we would get such an estimate if the true coefficient were 0.

On the export side, we estimate α to be about 0.75 (in the fixed effects model). This implies that there are fewer hard trading opportunities than easy opportunities. The estimate for p is 0.00048, which implies that a province needs 1,444 immigrants from a country to achieve half of its potential hard exports and 4,800 immigrants to achieve 90% of its potential hard exports.[15]

On the import side we have a larger α and a smaller probability, p. With fixed country effects, estimates for α range between 3.00 and 3.48. Thus α is about 4 to 5 times larger for imports than exports. The estimate for the imports p is about 1/3 of the size of the exports p.

We can partially explain the differences between the export and import results by considering our theories on why we expect a link between immigration and trade. As outlined in section 2.3.2, we expect immigrants to export more because they have lower transaction costs. Meanwhile we expect immigrants to import more for the same reason plus they have tastes for goods produced in their country of origin. The lower α and higher p we get on exports suggests that trading opportunities become saturated more quickly in exports than in imports. However, the imports generated from taste differences would not get saturated, because additional immigrants will want more goods from the home country.

As shown in Table 2.6, this specification suggests that over one third of imports are attributable to immigrants.[16] This result is large. We need to stress that these results do not imply that without immigrants Canadian imports would be only two-thirds of their present level. Much of the amount attributable to immigrants may arise from a substitution effect; immigrants may divert trade away from other trading partners.

[15] $1-(1-0.00048)^{1,444} = 0.5$; $1-(1-0.00048)^{4,796} = 0.9$.

As in previous tests, language effects are not statistically significant.

2.5.4. Developed versus Less Developed Countries

Up to this point we assumed the immigration effect is the same for all types of immigrants, but there are reasons to believe that immigrants from a developed country might affect trade differently from immigrants from less developed countries. Developed countries tend to have a business environment and infrastructure that resembles those of Canada more closely than those of other countries do. As a result the value of being an immigrant may be diminished for conducting trade with developed countries. In addition, developed countries tend to have a stronger legal framework. Trade with less developed countries requires more familiarity with the reliability of other parties, since there tends to be less recourse to the courts. In addition, the tastes of immigrants from developed countries may more closely resemble those of non-immigrant Canadians, making it more likely that Canadian firms supply desired goods.

Based on this reasoning, we ran tests to see what happens when we allow the coefficients for developed countries to differ from those of less developed countries. For the purposes of our test we treated members of the OECD as developed and non-members as less developed. Table 2.5 reports our results and compares them with our previous results.

Most (but not all) of these statistical tests produced results that accorded with expectations. The immigration coefficient for OECD countries tends to be less than that of non-OECD countries. The exception occurs in the first two lines of table 2.5 reporting the elasticity specification on exports.

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[16] When fixed effects are employed in the probability specification, the amount was computed as $51 billion out of total annual imports of $139 billion.

Table 2.5
Immigration Coefficients when Differentiating between OECD and non-OECD Countries

	Fixed Country Effects		Per Tables 2.1-2.4 Coefficient	Est. Error	OECD countries Coefficient	Est. Error	Non-OECD countries Coefficient	Est. Error
"Simple Log"								
Exports	No		0.131	0.015	0.158	0.033	0.155	0.017
Exports	Yes		0.088	0.028	0.097	0.051	0.087	0.029
Imports	No		0.429	0.029	0.140	0.057	0.494	0.033
Imports	Yes		0.273	0.048	0.085	0.085	0.299	0.049
"Propensity" – Approx.								
Exports	No		39.8	9.5	12.8	10.8	115.7	17.3
Exports	Yes		26.4	12.4	15.3	14.9	49.7	21.2
Imports	No		94.5	16.5	54.5	18.6	205.5	30.1
Imports	Yes		19.3	20.8	-14.6	25.0	90.9	35.7
"Propensity" – MLE								
Exports	No		104.9	29.2	20.1	16.3	314.3	68.0
Exports	Yes		56.6	34.6	18.5	24.0	109.6	57.3
Imports	No		7271	1416	4731	1339	8120	1586
Imports	Yes		569	360	-6	26	1753	801
"Probability" - MLE								
Exports	No	α	1.33	0.20	0.68	0.18	1.79	0.26
		P	0.00200	0.00066	0.00211	0.00067		
Exports	Yes	α	0.77	0.25	1.11	0.59	0.69	0.25
		P	0.00048	0.00025	0.00057	0.00051		
Imports	No	α	19.96	3.61	24.29	5.11	18.05	3.42
		P	0.00039	0.00009	0.00045	0.00011		
Imports	Yes	α	3.48	1.32	0.62	0.71	5.65	2.26
		P	0.00015	0.00007	0.00012	0.00005		

Differentiation between OECD & non-OECD: OECD countries and Non-OECD countries columns.

Note: This table only provides the results that correspond with columns 1, 2, 4 and 5 from tables 2.1 to 2.4. The results for the other columns follow a similar pattern to those reported above.

Few of the OECD results are statistically significant. The non-OECD results are more likely to be statistically significant, since there are many more non-members than members of the OECD. For imports, the "propensity" model actually produces negative results for OECD countries when fixed country effects are employed, but these estimates are not statistically significant.

2.5.5. Comparison of the Specifications

Figure 2.1 and Table 2.6 provide a comparison of the specifications. Figure 2.1 shows how predicted trade responds to immigration.[17] The steeper, straight line is the MLE of the "Propensity" specification. The flatter curve (which is almost straight in the range shown in this figure) is a convex curve that represents the approximation of the "Propensity" specification. The white concave curve shows the relationship for the "Probability" specification, where there are decreasing returns to scale.

Table 2.6 shows the total trade associated with immigration for each specification. These amounts were computed using the coefficients obtained when we distinguished between OECD and non-OECD countries. The table reveals several things. First, the effect on trade is enormously affected by the specification. In the case of exports, the increased propensity model allocates just over 10% of Canada's $150 billion annual exports to the immigration link. Meanwhile, the probability model allocates about 50%. The reason for this discrepancy can be seen by observing the shape of this specification's curve in Figure 2.1. A large amount of trade is attributed to the first few immigrants. As a result, the model suggests that removing all immigrants removes much trade.

Secondly, the trade per immigrant is much larger for immigrants from OECD countries than for immigrants from non-OECD countries, despite the fact that the coefficients for the non-OECD

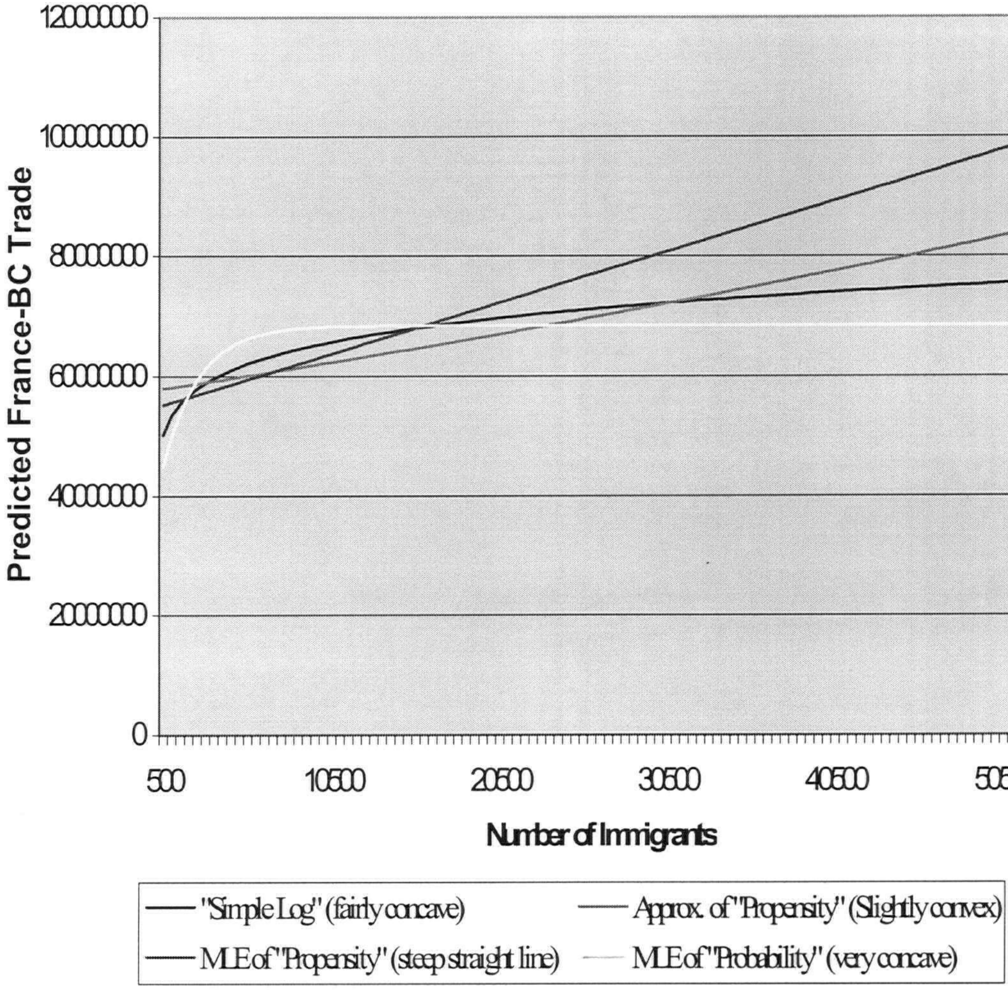
[17] The figure uses the actual independent variables and their estimated coefficients for exports from BC to France.

Table 2.6
Exports and Imports Associated with Immigrants

Specification	Fixed Country Effects	Total Annual Trade Associated with Immigrants ($US)	Average Annual Trade per OECD Immigrant ($US)	Average Annual Trade per Non-OECD Immigrant ($US)
Exports:				
Simple Log	No	$126 billion	$55,600	$3,800
	Yes	$75 billion	$33,900	$1,600
"Propensity" - Approx.	No	$16 billion	$6,100	$1,400
	Yes	$17 billion	$7,300	$550
"Propensity" - MLE	No	$20 billion	$8,300	$1,300
	Yes	$18 billion	$7,800	$600
"Probability" - MLE	No	$62 billion	$26,500	$2,900
	Yes	$76 billion	$34,100	$1,600
Imports:				
Simple Log	No	$15 billion	-$1,500	$7,300
	Yes	$29 billion	$6,300	$6,400
"Propensity" - Approx.	No	$51 billion	$19,600	$4,700
	Yes	-$13 billion	-$6,500	$1,600
"Propensity" - MLE	No	$128 billion	$52,900	$5,700
	Yes	-$4 billion	-$2,500	$3,400
"Probability" - MLE	No	$133 billion	$54,600	$6,700
	Yes	$51 billion	$20,100	$4,600

	Total	OECD Countries	Non-OECD Countries
Canada's Annual Exports	$150 billion	$138 bill.	$12 bill.
Canada's Annual Imports	$139 billion	$120 bill.	$19 bill.
Immigrants	4.6 million	2.1 mill.	2.5 mill.

Figure 2.1 - Pictures of Specifications



countries are larger. The OECD opportunities appear much larger than those of non-OECD countries, so an OECD immigrant can carry out more trade than a non-OECD immigrant, despite having a smaller comparative advantage over non-immigrants. As shown at the bottom of table 2.6, about 92% of Canada's exports and 86% of Canada's imports are derived from OECD countries.

A third observation is that results for imports are generally either very large or negative. One should not attach too much weight to the results of this table, since many of the results (particularly the OECD immigration coefficients) are not statistically significant. For example, the 95% confidence intervals for the total amount of trade associated with immigrants computed with the simple log specification are:

Exports – without fixed effects:	\$104 billion to \$138 billion
Exports – with fixed effects:	-\$59 billion to \$122 billion
Imports – without fixed effects:	-\$258 billion to \$100 billion
Imports – with fixed effects:	-\$504 billion to \$119 billion

The one specification that produces a statistically significant outcome is without fixed country effects and likely omits important variables. Given the wide confidence intervals on the other specifications, the point estimates reported in Table 2.6 cannot be relied upon. The calculations of total dollar values of trade are too dependent on a few large countries.

Nevertheless, we can use the outcomes in Tables 2.1 to 2.4 to consider the responsiveness of trade to immigration levels, since those measurements are not driven by only a few countries. Based on those outcomes, the specification that produces the most significant results is the simple log specification despite its lack of theoretical support. The probability model also produces fairly strong results, while the "propensity" model produces results that are neither plausible nor significant.

2.6 CONCLUSION

We set out to determine whether previous studies' findings of links between immigration and trade can survive the introduction of fixed country effects. We also sought to derive theory-based models to describe these links. In addition we introduced language effects into our tests to determine the interaction between language and immigration effects.

The simple log specification from Head and Ries (1998) performed very well, even with fixed country effects. Our results with new data correspond closely with their results. The newly-derived "probability" model also produces somewhat significant results.

There continues to be strong evidence that a link between immigrants and trade exists. Moreover, the relative success of the simple log model and the "probability" model suggests that an individual immigrant contributes less to trade, the larger the province's population of immigrants from the same country. Such decreasing returns could suggest that unexploited trading opportunities become harder to find as immigrants increase. Thus, there is some evidence that the reason immigrants expand trade is that they exploit trading opportunities that non-immigrants cannot.

CHAPTER 3: DO TAX DIFFERENCES CONTRIBUTE TOWARD THE BRAIN DRAIN FROM CANADA TO THE U.S.?

3.1 INTRODUCTION

"Canadian taxes are too high, they are driving out top talent, the Gretzkys of high tech." – John Roth, Nortel's CEO[1]

"Do I believe that some people leave Canada and that these are the kind of people we should not lose? Yes ... Do I believe that they leave because of taxes? No." - Paul Martin, Canada's Minister of Finance[2]

Over the last year Canada's brain drain to the U.S. has received much media attention in Canada. The debates reported in the newspapers have centred on two issues – whether the brain drain is big enough to be a concern to policy-makers, and whether Canada's higher taxes contribute toward Canada's loss of talent to the U.S. A number of political, business and academic leaders have expressed opinions on both of these issues. The opinions these leaders express are predictable, based on whether those individuals generally advocate tax reductions or advocate government spending. Those who seek tax reductions argue that the brain drain is an important problem and that taxes are an important reason for the drain. In politics, these arguments have come from the former Reform Party and the Progressive Conservative Party[3], which are the parties that campaign for tax reductions. Business leaders, such as John Roth (CEO of Nortel Networks Corp.)[4] and billionaire Paul Desmarais Sr.[5] have also argued that high taxes are driving Canada's best engineers and entrepreneurs out of the country. These claims are contradicted by people who favour government spending and perhaps see reduced taxes as a threat to that spending. In politics, the ruling liberal party has publicly argued that the Canadian brain drain is

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[1] *The National Post.* "'Gretzkys of high tech' flee taxes: Nortel CEO" (by Paul Bagnell), November 12, 1999, A1.

[2] *The National Post.* "How Paul Martin learned everyone has an agenda" (by Paul Wells), November 4, 1999, A7.

[3] See, for example, *The Globe and Mail*, "Martin says no brain-drain proof" (by Shawn McCarthy), May 1, 1999, A4.

[4] *The Globe and Mail*, "Nortel seen suffering from brain drain", (by Lawrence Surtees), April 30, 1999, B7. and *The National Post*, "'Gretzkys of high tech' flee taxes: Nortel CEO", (by Paul Bagnell), November 12, 1999, A1-A2.

small[6], based on a report by Statistics Canada (1998), and the party denies that Canada's high taxes drive Canadians south of the border. Similarly, the Canadian Association of University Teachers has argued that "the perception that Canada's finest brains are draining south to the United States is a myth being pushed as part of a right-wing tax-cutting agenda"[7].

The first of the two main issues – measurement of the significance of Canada's brain drain – has received some academic attention. The primary studies have been carried out by Statistics Canada (1998, 1999 and 2000), DeVoretz and Layrea (1999), Helliwell (1999) and Iqbal (1999). The conclusions of these studies are varied as well. DeVoretz and Layrea and Iqbal conclude that the brain drain is a very significant problem, while Helliwell and Statistics Canada conclude that Canada's brain drain to the U.S. is relatively small compared to historical levels and compared to Canada's brain gain from the rest of the world.

This paper addresses the second issue – whether taxes contribute to the brain drain. I consider this issue by examining a sample of Canadians living in Canada and a sample of Canadians living in the US. I have information on these individuals' income levels, tax levels and various other personal attributes. Using this information I estimate the amount each of these individuals would earn in the opposite country and estimate the taxes they would pay. Based on these results I test the effects of income and taxes by checking whether the people who have the most to gain in income or tax-savings are the most likely to choose to live in the U.S.

This paper is organized as follows. Section 3.2 examines in more detail the issue of whether the brain drain is significant. Section 3.3 compares the two countries' tax rates and tax systems, and

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[5] *The National Post*, "Canadians forced to U.S. by taxes, Desmarais says" (by Diane Francis), March 11, 1999, A1-A2.
[6] *The Globe and Mail*, "Martin says no brain-drain proof" (by Shawn McCarthy), May 1, 1999, A4.

identifies what sort of people have the most to gain in tax-savings by moving to the U.S. Section 3.4 reports how hypothetical incomes and taxes were determined, and also provides some observations on the attributes of people who have the most to gain in income by choosing the U.S. Section 3.5 presents the specification for my statistical tests and section 3.6 reports the results of those tests. Section 3.7 concludes.

3.2 Review of Previous Research

3.2.1 Research on the Significance of the Brain Drain

A study of the role of taxes on the brain drain would not be worthwhile unless the brain drain actually exists. There are six well-publicized studies that examine the significance of Canada's brain drain to the U.S. – three by Statistics Canada and three by other academic researchers. The studies by DeVoretz and Layrea (1999) and by Iqbal (1999) conclude that the rate of emigration is substantial, while the Statistics Canada studies (1998, 1999, and 2000) and Helliwell's study (1999) compute more modest levels of emigration. The former studies rely heavily on U.S. immigration data published by the U.S. Immigration and Naturalization Service (INS). This database tracks how many people are granted permanent or temporary visas in the U.S. These figures are substantial. For example, Iqbal used this data to compute that in 1997 total emigration of highly skilled Canadians to the United States amounted to 98,000.

Statistics Canada and Helliwell contend that the INS data should not be used to estimate Canadian emigration because many people are counted more than once as they renew their short-term visas. Statistics Canada's most recent paper (2000) uses three alternative methods[8] to

[7] *The National Post*, "Brain drain a myth created by business, academic group says" (by Eric Beauchesne), July 14, 1999. Page A7.

[8] Those three methods are: (1) a year-to-year comparison of the number of people living in the U.S. who were born in Canada as estimated from the U.S.' annual Current Population Survey; (2) the use of Statistics Canada's "Reverse Record Check" database, which comprises a sample of people included in the 1991

estimate emigration and concludes that approximately 22,000 to 35,000 Canadians move to the U.S. each year. Helliwell corroborates Statistics Canada's estimates. He bases his conclusions on the U.S. Current Population Survey as well as records of University of British Columbia graduates.

The first study by Statistics Canada (1998) tries to put the size of Canada's brain drain into context. This study finds that the brain drain is small relative to the brain drain of the late 50's and 60's, that the drain in most knowledge occupations is small relative to the stock of individuals in those occupations, that the drain is small relative to the supply of individuals entering those professions, and that the drain is substantially smaller than the brain gain from the rest of the world. (See Table 3.1 for a summary of some of Statistics Canada's findings.)

The second report by Statistics Canada (1999) reports extensive survey data from the 1995 graduates of post-secondary institutions. This study reports that 1.5% of the 1995 graduates moved to the U.S. by March 1999 (18% of whom had moved back to Canada by that date). The graduates most likely to move were the most educated ones. By degree type, the proportions of graduates who moved were as follows:

College graduates	1.4%
Bachelor's degree holders	1.7%
Master's degree holders	3.2%
Ph.D. graduates	12.0%

The study also finds evidence that those who move tend to be among the brightest students. 44% of those who moved indicated that they were in the top 10% of their graduating class, and 80% reported being in the top 25%. Those who moved also tended to win more scholarships when

Canadian Census that Statistics Canada tracks until the next census; and (3) inferences from Canadian tax returns on which taxpayers report if they are ceasing Canadian residence.

Table 3.1
Canada's Brain Drain to the U.S.
Some Data from the Statistics Canada Reports (1998 and 1999)

Historical Context:		
	1955-69	1990-96
Average annual emmigration from Canada to the U.S.	42,265	21,731
Average annual immigration into Canada	143,651	230,581

Educated Immigrants and Emigrants (1990-96):	
Average annual flow of university graduates from Canada to U.S.	8,512
Average annual flow of university graduates into Canada	32,829

Emmigrants in Selected Occupations Relative to New Supply (1990-96):

	Annual avg. emmigration to the U.S.	New supply to the Cdn labour force	Emigration as % of new supply
Physicians and health diagnosing	360	7,000	5.1%
Nurses	816	38,200	2.1%
Engineers	527	28,600	1.8%
Computer scientists	139	25,900	0.5%
Managerial occupations	2,347	388,700	0.6%

Reasons movers cited for going to the U.S. [1]	
Work-related	57%
Furthering Education	23%
Marriage/Relationships	17%

Specific aspects about work-related reasons [1,2]	
Greater availability of jobs in a particular field	44%
Higher salaries in the U.S.	39%
Greater availability of jobs in general	35%
Chance to gain or develop skills	21%
Better career advancement opportunities[3]	16%
Better employment benefits/perks[3]	11%

Median Salaries (in 1999 Canadian dollars, US amounts translated at PPP=1.25) [1]

	In Canada	In U.S.
College graduates	$24,200	$42,600
University graduates	30,500	43,400

(1) Sample was taken from individuals who graduated from a post secondary school in 1995.
(2) This was an open-ended question (with multiple answers allowed) asked to individuals who moved to the U.S. for work-related reasons.
(3) Coefficient is between 16.6% and 33.3%.

Sources: Statistics Canada (1998) and (1999) they had been in school.[9] The survey also asked the movers their primary reasons for moving. These results are reported in Table 3.1. Very few interviewees cited lower taxes as a reason for moving, though as the report indicates, tax differences may be implicit in the "higher salaries" response. The main conclusions of this report are that the brain drain is relatively small, but those who move tend to be among the highest achievers. Moreover, job opportunities are the main reason people move.

The third report by Statistics Canada (2000) shows that high-income households are more likely to move to the U.S. than lower income households.

There is a brain drain to the U.S. The drain is not at an alarming level that demands desperate measures, but it is significant enough to warrant study that could potentially lead to improvements in Canadian policy.

3.2.2 The Role of Taxes

Many people contend that lowering taxes would curb the outflow of Canadian talent. This claim is well supported by the theoretical literature. For example, Wilson (1992) finds that if a population has a high migration elasticity (i.e. the willingness to migrate is very responsive to changes in government benefits minus tax costs), then it is hard for a government to use taxes to redistribute wealth. Thus the theory suggests that the degree to which Canada can impose high taxes on high income earners will depend upon those people's migration elasticity.

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[9] Many respondents would not know their precise standing within their graduating class, and some respondents may have over-estimated their class performance. The information on scholarships is likely more objective.

Iqbal sought to quantify emigration levels (for several key occupation groups) as a function of income and tax gaps between Canada and the U.S. He found a positive relationship, but he cautions that one cannot read too much into his results given that he only had 46 observations. With a limited number of observations he was unable to place year dummies into his regression, and thus his regression would tend to show positive results as long as income and tax gaps and emigration all grow from year to year.

Aside from Iqbal's work, empirical evidence on the role of taxes on Canada's brain drain has been limited to survey data. Statistics Canada's 1999 report indicates that few people who moved cited taxes as a reason, though the structure of the survey might be partially responsible for this. It appears that respondents' only opportunity to cite taxes was in an open-ended question within the work-related category, but many respondents might not have considered taxes as work-related.[10]

Another survey, reported in *The National Post* on September 1, 1999[11], indicated that 59% of Canadians surveyed would consider taxes as one reason to move. However, the poll's sample was drawn from people who have chosen to remain in Canada, and their responses might be influenced by a desire to have the poll results imply that Canadian taxes should be cut.

This paper will examine evidence based not on what people say, but on what decisions they actually make, given their own tax circumstances.

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[10] The question was "What aspects of the job or other work-related factors attracted you to the United States after graduation? Be as specific as possible." (Statistics Canada, 1999, page 14).

[11] *The National Post*. "82 per cent say brain drain is really happening" (by Andrew McIntosh), September 1, 1999, A1,A8.

3.3 TAX DIFFERENCES

There are big differences between Canadian and U.S. taxes – both in effective rates and in the types of individuals that qualify for favourable tax treatment.

Canadian income tax rates are much higher than U.S. rates. Sometimes one sees comparisons of Canadian top marginal rates (46% to 54% in 1996 depending on the province) with California's or New York City's top marginal rates (47% and 48%)[12] that suggest the tax differences are small. However these comparisons fail to take into account the differences in how quickly marginal tax rates rise as a taxpayer moves up the earnings scale. In the U.S., the highest marginal rates only apply to taxpayers with earnings in excess of about $250,000(US), whereas the highest Canadian marginal rates begin between C$60,000 and C$100,000 (US$45,000 to US$75,000 in 1996). Moreover, Canadian marginal rates already reach 40% at the C$30,000 income level ($22,000 US).

Figures 3.1 and 3.2 compare the *average* tax rates for income levels between 0 and US$150,000. The first graph shows the rates that apply to unmarried individuals, while the second graph shows the rates that a married couple with two children faces if the family income is all earned by one of the partners. Both graphs show three lines for each country – a high tax jurisdiction (Quebec for Canada and New York City for the U.S.), the most populous jurisdiction (Ontario and California), and a low tax jurisdiction (Alberta for Canada and any no-tax state for the U.S., such as Texas or Florida).

[12] These rates were computed assuming state and city taxes are claimed as itemized deductions. Social Security taxes (FICA) are also included in these rates.

Figure 3.1 - Comparison of US and Canadian Tax Rates (1996) - Single Person

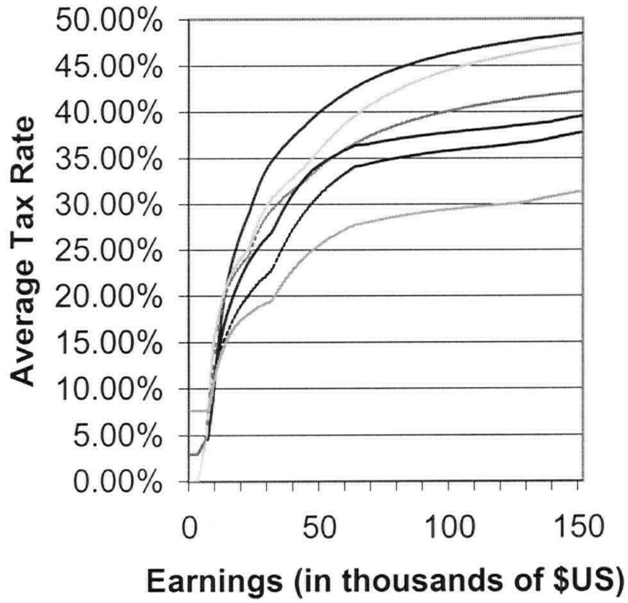
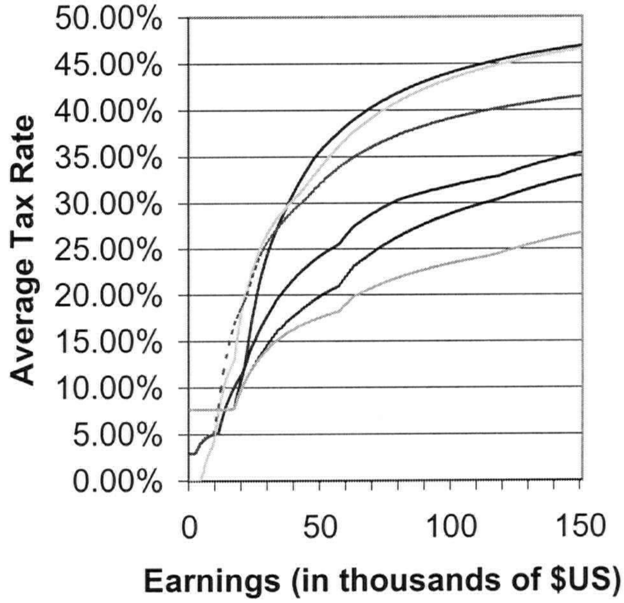


Figure 3.2 - Comparison of US and Canadian Tax Rates (1996) - Married Couple - One Income Earner



Legend for both figures:

Top line (at most earning levels): Canadian high tax jurisdiction (Quebec)
Second highest line (at most earning levels): Canada's largest jurisdiction (Ontario)
Third highest line (at most earning levels): Canada's low tax jurisdiction (Alberta)
Third lowest line (at most earning levels): U.S. high tax jurisdiction (NY City)
Second lowest line (at most earning levels): U.S.'s largest jurisdiction (California)
Bottom line (at most earning levels): U.S. low tax jurisdiction (any no-tax state)

Notes:

The taxes computed for these figures include no deductions or credits other than those available to all taxpayers of that profile. The Canadian calculation assumes claims for personal, married, CPP and EI credits. The U.S. calculations assume the appropriate exemption claims and the greater of the standard deduction and itemized deductions (comprising state and city taxes). Unique provincial, state and city deductions and credits are also considered. In figure 3.2, the married couple is assumed to have two children.

The Canadian taxes considered in these figures include federal and provincial income taxes, as well as Canada Pension Plan (CPP) and Employment Insurance (EI) levies. Correspondingly, the U.S. taxes include federal, state and city income taxes plus Social Security taxes (FICA).

Besides having lower tax rates, the U.S. tax system also targets its tax breaks differently from Canada. There are a legion of factors that can affect the size of a household's tax-savings if it moved from Canada to the U.S. Some of the more important ones are the following.

1. *Ability to file joint returns*: The U.S. allows married couples to file joint tax returns, while Canada does not. Under the U.S. system married couples move up to higher tax brackets at higher income levels than single individuals. As a result, taxpayers who marry partners who earn little income can gain a significant tax savings in the U.S., while corresponding Canadian taxpayers gain very little tax-savings. By comparing figures 3.1 and 3.2, one can easily see the relative tax benefits of being married in the U.S. versus Canada. The Canada-US gap for married couples is widest where only one of the partners earns income; the gap narrows if both partners contribute approximately equal amounts to family income. In summary, couples where one partner earns most of the income have more to gain from moving to the U.S. than single individuals or couples with near equal earnings.
2. *Mortgage interest and real estate tax deductions*: U.S. taxpayers may claim mortgage interest and real estate tax expenses on their home as itemized deductions, whereas Canadian taxpayers have no such deductions available. In 1996, U.S. deductions for mortgage interest amounted to 4.9% of adjusted gross income (AGI) and deductions for real estate taxes amounted to 1.6% of AGI.[13] Therefore, homeowners – particularly those with debt – may have more to gain than renters.[14]

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[13] These data were computed from IRS statistics provided on the internet on spreadsheets 96in14.si.xls and 96in21id.xls.

[14] In the U.S., home mortgage interest and real estate tax expenses are itemized deductions, which a taxpayer would only claim if in aggregate they exceed the standard deduction. Many homeowners do not

3. *Retirement plan contributions*: Canada offers much more generous deductions for contributions to retirement plans. Canadian taxpayers may deduct contributions up to the lesser of 18% of earned income and $13,500. Meanwhile, U.S. deductible contributions to an IRA are limited to at most $2,000 per person and in 1996 this deduction was eliminated for individuals who earned more than $35,000 or couples who earned more than $50,000. In 1996, deductible contributions to retirement plans amounted to 4.3% of total income in Canada and 0.2% of AGI in the U.S.[15] Taxpayers who cannot or choose not to contribute much to retirement plans have more to gain from moving to the U.S. than those who contribute the maximum deductible amount. In particular, indebted homeowners may tend to fit this profile, preferring to pay down their mortgage rather than contribute to a registered retirement plan.
4. *Jurisdictions*: Taxpayers who come from high-tax provinces in Canada, such as Quebec and British Columbia, or taxpayers who move to no-tax states, such as Texas, gain more from moving than other taxpayers.
5. *Income Range*: The Canada-U.S. tax rate differences vary across income levels. The biggest differences lie in the $50,000 to $150,000 ($US) range.

Evidently there is substantial variation amongst taxpayers in how much tax-savings they can realize by moving to the U.S. My statistical tests exploit this variation by considering whether those who have the most to gain are the most likely to move.

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benefit from these deductions, since their standard deduction exceeds their itemized deductions. State taxes are also itemized deductions, so taxpayers in high-tax states, such as California, are more likely to benefit from itemized deductions than other taxpayers, since their itemized deductions are more likely to exceed the standard deduction.

[15] The Canadian data is taken from Revenue Canada statistics provided on the internet (Tables 1 and 11 at http://www.rc.gc.ca/menu/EmenuKVE.html) and the U.S. data is taken from the IRS's spreadsheet 96in14si.xls.

3.4 MEASUREMENT OF INCOME AND TAXES – ACTUAL AND HYPOTHETICAL

My statistical tests involve comparing people's Canadian and U.S. pre-tax income opportunities and comparing their Canadian and U.S. taxes on those respective incomes. The tests use a sample of households in Canada and the U.S. where at least one member of the household has lived in Canada in the past. Survey data on people's income and taxes are readily available for the country in which they actually live, but not for the country in which they do *not* live. These latter amounts must be estimated.

Below, I describe the survey data used for actual income and taxes and then describe how this survey data is used to estimate people's income opportunities and hypothetical taxes for the opposite country. I also profile which people have the most to gain in income by moving to the U.S.

3.4.1 Survey Data

For people residing in Canada, the data comes from an annual survey conducted by Statistics Canada, called "Individuals – Aged 15 Years and Over With and Without Income". This survey provides fairly detailed economic and demographic data on about 75,000 individuals each year. In the U.S. the Current Population Survey (CPS) provides similar data on about 140,000 individuals each year. Beginning with 1992 the CPS also reports each individual's country of birth, and since 1994 the person's previous country of residence, allowing researchers to identify which people came from Canada. Over the five years used in my study (1992-1996), there are 2,331 Canadians in the CPS sample, 422 of whom arrived in the 1990s.

The unit of observation for my statistical tests is the household. The sample included those households whose highest income earner is of a normal working age – 18 to 64, and which had a positive income. A household residing in the U.S. is treated as Canadian if at least one of the spouses was born in Canada or last resided in Canada. Using these units of observation, my tests have 182,276 observations, 1,259 of which are residing in the U.S. Observations are weighted according to the sample weighting factors provided by the CPS and Statistics Canada.

3.4.2 Estimation of Hypothetical Income

A comparison of people's Canadian and U.S. income opportunities requires an unbiased estimate of the amount people could earn if they lived in the opposite country from which they actually live. The potential source of bias that concerns us most is the skills effect, because there is strong evidence (reported in section 3.2 of this paper) that people who move to the U.S. tend to be more skilled than those who remain in Canada. Therefore it is important that the hypothetical income calculations account for a person's skill levels, otherwise Canadians living in the U.S. would appear to have a bigger U.S./Canada earnings ratio than might really be the case.

One useful measure of skill levels is a person's education achievements. I therefore predict income with a regression that includes education variables among the regressors. The return on education can vary by occupation type, so I take advantage of the large number of observations by running a separate regression for each of 48 occupation classes.[16] The specification is as follows:

$$\log Y = \beta_1 AGE + \beta_2 AGE^2 + \beta_3 SEX + \beta_4 MARRIED_M + \beta_5 MARRIED_F + \beta_6 MWC_M + \beta_7 MWC_F + \beta_8 E_{HS} + \beta_9 E_{PS} + \beta_{10} E_{BACH} + \beta_{11} E_{GRAD} + \beta_{12} YR_{93} + \beta_{13} YR_{94} + \beta_{14} YR_{95} + \beta_{15} YR_{96} + C + \varepsilon$$

where:

Y = total income (including investment income, government assistance, etc.);

AGE = a person's age in years;

SEX = 0 if male, 1 if female;

$MARRIED_M$ = 1 if married and male, 0 if not;

$MARRIED_F$ = 1 if married and female, 0 if not;

MWC_M = 1 if male and married with children, 0 if not;

MWC_F = 1 if female and married with children, 0 if not;

E_{HS} = 1 if the person graduated from high school or its equivalent, 0 if not;

E_{PS} = 1 if the person graduated from a post-secondary school, 0 if not;

E_{BACH} = 1 if the person has a university bachelor's degree, 0 if not;

E_{GRAD} = 1 if the person has a graduate degree or graduated from a professional school, 0 if not;

YR_{93} to YR_{96} = year dummies;

C = constant; and

ε = error term.

Thus a Canadian resident in the health diagnosing profession, for example, would have a hypothetical U.S. income based on the coefficients derived from running the above regression on people in the same profession in the U.S.

The results of the regressions are not reported in detail, because there are 96 regressions (48 occupation codes times 2 countries) and the derived coefficients are not the central point of the paper; however, a summary of the results is presented in Appendix 3.1 for interest's sake.

Unfortunately, there is evidence that the use of education variables may not adequately capture the skills effect. Recall Statistics Canada's survey results that found that 44% of those who moved reported being in the top 10% of their class and 80% reported being in the top 25%.

[16] I use the occupation codes used by Statistics Canada in the survey data. The U.S. data has a different coding system with 500 occupation classes, so their occupations were recoded into the Canadian

While some respondents may have exaggerated their scholastic achievements, the data does suggest that movers tend to be more skilled than stayers with identical education backgrounds. I therefore take the additional step of using an individual's earning capabilities at home as an indicator of skill level beyond those predicted by education and other measured attributes. This is done by using residuals from regressions in the home country to estimate an individual's earning capabilities beyond those predicted from his/her measured attributes. An individual's final hypothetical income is computed as follows:

$$E(\log Y_{f,p}) = \log\hat{Y}_{f,p} + (\log Y_h - \log\hat{Y}_{h,p})(s_{f,p}/s_{h,p})$$

where subscript *h* refers to the person's country of residence, subscript *f* refers to the opposite country and subscript *p* refers to the individual's profession, and:

$E(\log Y_{f,p})$ = the individual's hypothetical logged income,

$\hat{Y}_{i,p}$ = the individual's predicted income in country *i* using coefficients from regressions run with country *i* data on individuals in profession *p*,

Y_h = the individual's actual income, and

$s_{i,p}$ = the standard error from the regression involving country *i* and profession *p*.

This formula uses the regressions described above as a base, but then adjusts the predicted earnings based on home country residuals. Those residuals are rescaled by $s_{f,p}/s_{h,p}$ to reflect the different degrees of income dispersion in the two countries.

3.4.3 Income Profiles of Movers and Stayers

Table 3.2 reports the average ratios of U.S. income to Canadian income (translated at PPP rates). Overall, the average U.S./Canadian pre-tax income ratio is 1.13. The ratio for households that chose to remain in Canada is also 1.13 while the ratio for households that chose to live in the U.S.

classification system.

Table 3.2
Average Ratios of U.S. Income to Canadian Income(1)

	Households in Both Countries(2)	Households in Canada Only	Households in the U.S. Only(2)
Overall	1.13	1.13	1.19
Selected High-Ratio Occupations(3)			
Scientists	1.35	1.35	1.36
Architects and engineers	1.32	1.33	1.25
Health diagnosing and treating	1.72	1.69	1.87
Nursing, therapy and related	1.41	1.41	1.45
Artistic, literary, recreational	1.50	1.51	1.31
Selected Low-Ratio Occupations(3)			
Forestry and logging	0.86	0.86	0.73
Processing (other than food and beverage)	0.70	0.70	0.66
No occupation	0.88	0.87	1.06
By Income Ranges (using U.S. income)			
0 – 20,000 ($US)	1.07	1.07	1.10
20,000 - 50,000	1.12	1.12	1.15
50,000 - 100,000	1.21	1.21	1.22
over 100,000	1.36	1.35	1.37
By Education(3)			
No high school diploma	0.89	0.89	0.89
Only high school diploma	1.19	1.20	1.12
Non-university post secondary diploma	1.17	1.17	1.16
Bachelor's degree but no grad. degree	1.24	1.23	1.28
Graduate degree	1.34	1.33	1.41

(1) In the case of a household located in Canada, the ratio's numerator is hypothetical U.S. income and the denominator is actual Canadian income converted to U.S. dollars at the PPP rate. In the case of a household located in the U.S. the numerator is actual income and the denominator is hypothetical Canadian income converted to U.S. dollars at the PPP rate.
(2) U.S. households used for these statistics only include those with at least one member who lived in Canada in the past.
(3) The higher-income spouse's occupation and education level are used for married couples.

is 1.19. These statistics provide some preliminary evidence that households that have the most to gain in income are more likely to choose to live in the U.S. Later, this paper presents results from tests that control for family traits and education levels, to produce a better analysis of how before-tax income affects households' decisions on which country to live in.

The ratio of U.S. income to Canadian income varies across occupations. As shown in table 3.2, some of the biggest income differentials are in professions where the brain drain is most acute – amongst doctors, nurses, scientists and engineers. On the other end of the spectrum, there are some professions where Canadian earnings are more attractive than U.S. earnings – such as in forestry and logging, and various blue collar professions. Individuals not in any profession also have more attractive earnings in Canada than in the U.S., probably on account of Canada's more generous social welfare programs.

The analysis by income ranges in table 3.2 shows that the gains from living in the U.S. tend to be larger for high income earners than for low income earners, reflecting a greater income dispersion in the U.S. than in Canada. It is interesting to note that within each income category, there is only a small difference in ratios between Canadian households and U.S. households. These ratios differ only by 1 to 3 percentage points, whereas the overall difference is 6 percentage points (1.19-1.13). This can be explained by the fact that households that choose to live in the U.S. tend to earn more income. The proportions of households in each income class are as follows:

U.S. Income Range ($US)	Households in Canada (%)	Households in the U.S. (%)
0 – 20,000	43	23
20,000 – 50,000	37	34
50,000 – 100,000	17	30
> 100,000	3	13

Finally, the earnings benefits of living in the U.S. rise as education levels increase. People who have not finished high school tend to be better off in Canada, while people with university education generally can earn substantially more in the U.S. than in Canada.

3.4.4 Estimation of Hypothetical Tax

This paper's tests consider three types of taxes – federal income taxes, state/provincial income taxes and payroll taxes. The methodology used to predict taxpayers' hypothetical taxes for the country in which they do not live, is described below. The description focuses primarily on the computation of hypothetical U.S. taxes. Since the prediction of hypothetical Canadian taxes follows a similar pattern, the discussion of those computations is limited to issues unique to those predictions.

Hypothetical U.S. *federal* taxes were estimated as follows:

1. Using the U.S. survey data, each taxpayer's "net non-personal tax deductions" (NNPDs) were inferred using the following steps:
    a. First I determined the taxpayer's most likely filing status. I assumed that married people filed "married filing joint" (MFJ), single individuals without children filed "single", and single individuals with children filed "head of household". (Readers unfamiliar with the U.S. tax rules can refer to Appendix 3.2, which provides a very brief overview of the structure of how U.S. taxes are computed.)
    b. Next, I imputed the taxpayer's taxable income using the reported taxes and the tax rates and brackets that apply to the taxpayer's filing status for the particular year.

c. To the imputed taxable income I added the amounts the taxpayer would be able to deduct for exemptions and the standard deduction for the taxpayer's filing status. I assumed that all children under age 18 are claimed as dependents.[17]

d. The amount determined in step c was subtracted from the taxpayer's income reported in the survey to get the taxpayer's NNPDs. (For married people the taxpayer's income is the joint income of both partners.)

For most taxpayers, the NNPD is the sum of net deductions[18] claimed in arriving at adjusted gross income and any excess of itemized deductions over the standard deduction.

2. Next, a regression was run using the U.S. data to produce an equation that can predict a taxpayer's NNPDs. The regression's specification was as follows:

$$NNPD_u = \beta_1 Y_u + \beta_2 I_u + \beta_3 MARRIED + \beta_4 PARENT + \beta_5 AGE + \beta_6 AGE^2 + \beta_7 YR_{93} + \beta_8 YR_{94} + \beta_9 YR_{95} + \beta_{10} YR_{96} + \beta_{11} MILLS + C + \varepsilon,$$

where:

I_u = investment income (interest, dividend and rental income) in the U.S.;

PARENT = 1 if the taxpayer has children under age 18, 0 otherwise; and

MILLS = Mills' ratio.

Investment income was included since deductions that offset investment income differ from deductions that offset other earnings. Mills' ratio was included because a unique NNPD cannot be inferred when taxes are zero; only a lower bound can be inferred. The ratio was computed based on probabilities that a positive federal tax is observed. Table 3.3 reports results from this regression.

3. Finally, the above information was used to determine the hypothetical U.S. federal taxes for taxpayers in the Canadian data set, as follows:

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[17] The U.S. data had enough information to permit a more accurate prediction of exemption claims, but this cutoff was chosen so that the same assumption could be used when estimating hypothetical exemptions for Canadian residents, for whom the data is less detailed.

[18] These deductions are net of any taxable benefits and other taxable income not reported in the survey.

Table 3.3
Predictions Used to Compute Hypothetical Taxes

Prediction of "Net Non-Personal Deductions"

Variable	U.S. Coefficients	Canadian Coefficients
Y_U	0.249	0.135
I_U	-0.185	0.210
MARRIED	-3,559	-711 if male; -1,094 if female
PARENT	1,138	-47 if male; 1,655 if female
AGE	-249	-36
AGE^2	3.22	0.93
YR_{93}	-185	-74
YR_{94}	-755	-216
YR_{95}	-956	-232
YR_{96}	-1,068	-406
MILLS	1,820	230
C	796	-1,039
No. of obs.	198,340	196,199
R^2	0.431	0.298

Note: All results are statistically significant at the 99.9% level.

Weighted Average of State Tax Rates

Income Range		Single	Married
\$0 -	10,000	0.79%	0.44%
\$10,000 -	20,000	2.96%	1.47%
\$20,000 -	40,000	4.48%	3.61%
\$40,000 -	80,000	5.32%	5.01%
\$80,000 -	160,000	5.95%	5.65%
\$160,000 and up		6.71%	7.56%

a. A U.S. filing status was determined using the decision rule described in step 1a above.

b. The taxpayers' net non-personal deductions were estimated using the coefficients derived in step 2. Hypothetical U.S. income was used as the taxpayers' income. Since the level of taxpayers' investment income may depend on the country in which they reside, hypothetical U.S. investment income (I_u) was computed as follows:

$$I_u = (I_c / Y_c) \times (\bar{I}_u / \bar{I}_c) \times Y_u$$

where:

I_c = actual investment income earned in Canada (in Canadian dollars),

Y_c = actual income earned in Canada (in Canadian dollars),

$\bar{I}_u$, $\bar{I}_c$ = average investment income as a fraction of total income earned by individuals in the U.S. sample and the Canadian sample respectively, and

Y_u = hypothetical U.S. income (in U.S. dollars).

c. The hypothetical U.S. *taxable* income was computed by deducting from hypothetical U.S. income the NNPDs computed in step b, the exemption deduction based on the number of children under age 18, and the standard deduction.

d. Hypothetical taxes were computed using this taxable income and the prevailing tax rates.

Hypothetical U.S. *state* taxes were computed differently. Rather than working with the tax rate schedules and permitted deductions for each of the 51 states[19] in each of the 5 years, I instead predicted state tax based only on income and marital status. For each state and each marital status (married and single) I ran the following regression:

$$\begin{aligned} STATETAX = {} & \beta_1\max\{\min\{Y_u,10000\},0\} + \beta_2\max\{\min\{Y_u,20000\}-10000,0\} + \\ & \beta_3\max\{\min\{Y_u,40000\}-20000,0\} + \beta_4\max\{\min\{Y_u,80000\}-40000,0\} + \\ & \beta_5\max\{\min\{Y_u,1600000\}-80000,0\} + \beta_6\max\{Y_u-160000,0\} + \varepsilon. \end{aligned}$$

In effect, I computed an average tax rate for the income ranges of 0-\$10,000, \$10,000-20,000, \$20,000-40,000, \$40,000-80,000, \$80,000-160,000 and \$160,000 plus. This method allowed for the progressive nature of states' tax rate schedules.[20] I then computed a weighted average of these coefficients in proportion to the number of people from Canada residing in each state. The

[19] The 51st "state" is the District of Columbia. Seven of the states impose no income taxes.

proportion of Canadians in the state served as a proxy for the probability that a Canadian would choose that state if he/she were to move to the U.S. Table 3.3 reports the results. These results were used to estimate Canadian residents' hypothetical U.S. state taxes on hypothetical U.S. income.

U.S. *social security taxes* are the payroll taxes of the U.S. They do not qualify for deductions, so they were computed directly both for U.S. residents and Canadian residents. For U.S. residents, I simply applied the prevailing rates to the employment and self-employment income reported in the U.S. survey. For Canadian residents I applied the rates to hypothetical employment and self-employment income. Hypothetical U.S. employment income (EMP_u) and self-employment income (SE_u) were computed as follows:

$$EMP_u = (EMP_c / Y_c) \times (\bar{e}mp_u / (\bar{e}mp_c) \times Y_u \text{ and}$$

$$SE_u = (SE_c / Y_c) \times (s\bar{e}_u / s\bar{e}_c) \times Y_u$$

where:

$\bar{e}mp_u$, $\bar{e}mp_c$ = average employment income as a fraction of total income earned by individuals in the U.S. sample and the Canadian sample respectively, and

$s\bar{e}_u$, $s\bar{e}_c$ = average self-employment income as a fraction of total income earned by individuals in the U.S. sample and the Canadian sample respectively.

U.S. *city* taxes were not considered in my statistical tests since the U.S. data does not report respondents' city taxes. Thus, city taxes are in neither the actual U.S. tax amounts nor the hypothetical U.S. tax amounts. The differences between Canadian and U.S. average tax rates are therefore mildly exaggerated, but this mismeasurement likely does not distort my results. The

[20] Colorado, Illinois, Indiana, Massachusetts, Michigan and Pennsylvania have a flat tax rate, but the

slightly inflated differences between Canadian and U.S. taxes applies both to people who choose to live in the U.S. and those who live in Canada, so a distortion could only arise if a disproportionately low or high number of Canadians choose to live in cities that impose income taxes. The vast majority of cities do not impose city income taxes, and the rates are low in the cities that do impose them.

A taxpayer's total hypothetical U.S. tax *rate* (t_u) is thus the sum of the hypothetical federal taxes, state taxes and social security taxes all over hypothetical U.S. income.

The computation of hypothetical *Canadian* taxes follows a similar pattern, but there are a few differences that need to be highlighted. These differences arise due to the amount of information provided in the data sets, and due to differences in the tax structures. (Readers not familiar with Canada's tax system can refer to Appendix 3.3, which provides a very brief overview of the Canadian tax system for individuals.)

1. The Canadian survey data does not provide separate figures for federal and provincial taxes, but just provides the total. As a result, the methodology used for U.S. federal taxes is the methodology I use to estimate the combined Canadian federal and provincial tax. That is, I use the total taxes to infer net non-personal deductions. Since the rates differ across the ten provinces, this approach required that I take into consideration the tax rules for each province for each of the five years.
2. Whereas the U.S. system provides tax relief primarily with deductions, the Canadian system relies on a mixture of deductions and credits. As a result, the NNPD amount computed is really the sum of net deductions and the "deduction equivalent" of credits. A "deduction equivalent" can best be described with an example. Suppose an individual in the 26% bracket (before surtaxes and provincial taxes) claims $1,000 of tuition expenses. The credit

availability of deductions still makes the effective tax rate somewhat progressive.

(before surtaxes and provincial taxes) is 17% of $1,000, so the "deduction equivalent" is 17% / 26% x $1,000 = $654.

3. The coefficients for predicting Canadian NNPDs are reported in table 3.3.
4. The Canadian payroll taxes are the Canada Pension Plan and Employment Insurance levies.

3.5 SPECIFICATION

In theory, a household with rational decision-makers will choose to live in the country that yields the highest expected utility. Assume a household's expected utility from living in country *i* is:

$$U_i = Y_i^{\beta 1} (1-t_i)^{\beta 2} \theta_i$$

where:

Y_i = the pre-tax income the household can earn in country *i*;

t_i = the average rate of income taxes the household would have to pay if it lived in country *i* and earned Y_i;

θ_i = the product of all non-income factors that affect utility for country *i*; and

β_1, β_2 = parameters.

Note that β_1 would equal β_2 if a household's decision makers do not care whether an extra dollar of after-tax income comes from a reduction in the average tax rate or from an increase in pre-tax income.

This paper allows β_1 to differ from β_2 both for theoretical and practical reasons. The first theoretical reason is that there may be compensating factors associated with earning more pre-tax income. For example, a person may derive less utility from earning an extra x% through a change in Y_i than through a change in $(1-t_i)$ because he/she may have to work harder or face poorer work conditions to earn that extra amount, whereas a change in taxes would not involve a change in effort level.[21] Alternatively, people might weight pre-tax earnings differently from taxes because they do not really understand the tax system. For example, if their before-tax income increases by x%, they may not know how much that increase changes their after-tax income. That is, β_1 may differ from β_2 because of a systematic difference between perceived and actual tax effects. Rupert and Fischer (1995) provide U.S. evidence that a high proportion of people do not know their marginal tax rates, though they find no evidence that people systematically over-estimate or under-estimate their rates.

The practical reason for allowing β_1 to differ from β_2 is that the purpose of this paper is to distinguish whether pre-tax earnings, tax-savings or other factors are motivating people to move to the U.S. Without allowing β_1 to differ from β_2 the tests would not be able to distinguish whether higher salaries or lower taxes are driving the decision.

A household's expected utility will also be based in part upon non-income factors. To the extent that such information is available, I use other factors to serve as control variables. The log of the non-income factor of a household's expected utility associated with living in country *i* is assumed to have the following structure:

$$\log(\theta_i) = \Lambda_i + \Gamma_i$$

$$= \beta_{3i}\log(AGE)+\beta_{4i}MARRIED+\beta_{5i}KIDS+\beta_{6i}E_{HS}+\beta_{7i}E_{PS}+\beta_{8i}E_{BACH}+\beta_{9i}E_{GRAD}+C_i+\Gamma_i$$

where:

Λ_i = log of the measured non-income factors;

Γ_i = log of the unmeasured non-income factors;

[21] The same argument *cannot* be made about compensating factors associated with paying taxes. Many Canadians believe Canada's higher taxes contribute towards free health care, lower crime and more generous social benefits. However, the benefits do not accrue in greater measure to those who pay high taxes than those who do not. Consequently, government provided benefits enter the utility function as part of θ_i, not $(1-t_i)^{\beta 2}$.

β_{3i} to β_{9i} = parameters for country i;

AGE = the age of the person in the household who earns the highest income;

MARRIED = 1 if married, 0 otherwise;

KIDS = 1 if the family has children under age 18, 0 if not;

E_{HS}, E_{PS}, E_{BACH}, E_{GRAD} = the education variables of the person in the household who earns the highest income; and

C_i = constant.

In this case, the betas and the constant are allowed to vary across countries to allow for the fact that the utility derived from being married, having kids, or being educated can differ between the two countries.

The age, marital and kids variables serve as control variables to allow for the possibility that individuals may prefer to raise their families in one country over the other.

The educational variables allow for the possibility that academic or professional careers may be more rewarding in non-monetary ways in one country than in another. They also serve as a proxy for professional mobility. The more specialized a job is, the further afield employers look to recruit individuals to fill the position.

Γ_i covers all non-measured non-income variables. Factors that might affect an individual's Γ_i include specific job opportunities, depth of personal ties to his/her home country, and political preferences.

Logging the expected utility function yields the following transformed utility function:

$$\log(U_i) = \beta_1 \log Y_i + \beta_2 \log(1-t_i) + \beta_{3i}\log(AGE) + \beta_{4i}MARITAL + \beta_{5i}KIDS$$

$$+ \beta_{6i}E_{HS} + \beta_{7i}E_{PS} + \beta_{8i}E_{BACH} + \beta_{9i}E_{GRAD} + C_i + \Gamma_i$$

Since a family will choose to live in the country that yields the highest utility, it will move to the U.S. if $\log(U_u)>\log(U_c)$, where subscript u refers to the U.S. and subscript c refers to Canada. Restating this, the family will live in the U.S. if:

$$\beta_1 \log(Y_u/Y_c) + \beta_2 \log((1-t_u)/(1-t_c)) + (\beta_{3u}-\beta_{3c})\log(AGE) + (\beta_{4u}-\beta_{4c})MARITAL$$

$$+ (\beta_{5u}-\beta_{5c})KIDS + (\beta_{6u}-\beta_{6c})E_{HS} + (\beta_{7u}-\beta_{7c})E_{PS} + (\beta_{8u}-\beta_{8c})E_{BACH} + (\beta_{9u}-\beta_{9c})E_{GRAD}$$

$$+ (C_u-C_c) > \Gamma_c - \Gamma_u$$

If I assume that $\Gamma_c - \Gamma_u$ is normally distributed with mean zero, I can use a probit test to estimate my parameters. A family will therefore choose to live in the U.S. if:

$$\beta_1 \log(Y_u/Y_c) + \beta_2 \log((1-t_u)/(1-t_c)) + \beta_3 \log(AGE) + \beta_4 MARITAL + \beta_5 KIDS$$

$$+ \beta_6 E_{HS} + \beta_7 E_{PS} + \beta_8 E_{BACH} + \beta_9 E_{GRAD} + C > \Gamma_c - \Gamma_u$$

where:

$\beta_k = \beta_{ku} - \beta_{kc}$; and

$C = C_u - C_c$.

Before moving on to the results of these tests, it is important to note that my methods of estimating hypothetical incomes and taxes lead to some unavoidable biases. Appendix 3.4 briefly describes three such biases and argues that each pushes the coefficient estimates toward zero.

3.6 RESULTS

Table 3.4 presents the main results of this paper. The first column, labeled as the "economic person" model, ignores all measures of non-economic factors, relegating all those factors into the constant. The probit test on this model finds that both income differences and tax differences are

Table 3.4
Statistical Tests on the Significance of Income Differences and Tax Differences

Variable	"Economic Person" Model	Full Model	Full Model (92,94,96)	Full Model (only 90s immigrants)	Full Model (only 90s immigrants) (92,94,96)
$\log(Y_u/Y_c)$	0.42 (se=.03;P=.000)	0.38 (se=.04;P=.000)	0.41 (se=.05;P=.000)	0.33 (se=.09;P=.000)	0.47 (se=.12;P=.000)
$\log[(1-t_u)/(1-t_c)]$	1.52 (se=.18;P=.000)	0.83 (se=.20;P=.000)	0.82 (se=.27;p=.002)	1.21 (se=.40;P=.003)	1.38 (se=.57;P=.015)
log(AGE)		0.39 (se=.06;P=.000)	0.41 (se=.07;P=.000)	-0.71 (se=.12;P=.000)	-0.68 (se=.14;P=.000)
MARRIED		0.27 (se=.04;P=.000)	0.26 (se=.05;P=.000)	0.51 (se=.09;P=.000)	0.50 (se=.11;P=.000)
PARENT		-0.01 (se=.04;P=.712)	-0.01 (se=.05;P=.825)	-0.11 (se=.07;P=.123)	-0.12 (se=.09;P=.178)
E_{HS}		0.55 (se=.05;P=.000)	0.63 (se=.07;P=.000)	0.25 (se=.11;P=.029)	0.22 (se=.14;P=.119)
E_{PS}		-0.78 (se=.05;P=.000)	-0.77 (se=.06;P=.000)	-0.64 (se=.12;P=.000)	-0.60 (se=.14;P=.000)
E_{BACH}		0.91 (se=.06;P=.000)	0.88 (se=.07;P=.000)	1.11 (se=.12;P=.000)	1.01 (se=.14;P=.000)
E_{GRAD}		0.09 (se=.05;P=.105)	0.11 (se=.07;P=.103)	0.07 (se=.09;P=.453)	0.05 (se=.11;P=.631)
Constant	-1.84 (se=.02;P=.000)	-3.72 (se=.22;P=.000)	-3.87 (se=.28;P=.000)	-0.54 (se=.42;P=.203)	-0.60 (se=.50;P=.227)
Lik. Ratio Index	0.019	0.103	0.107	0.130	0.124
No. of obs.	182,276	182,276	111,242	181,246	110,616
Obs. in U.S.	1,259	1,259	769	226	141

Note: The figures in large fonts (other than the number of observations) are coefficient estimates. Underneath these coefficient estimates, the estimated error is reported in small fonts, as well as the P>|t| amount.

positive and significant at the 99.9% level. Thus, both income differences and tax differences affect a household's decision on whether to live in the U.S.

Somewhat surprisingly, the tax coefficient is substantially higher than the income coefficient, which seems to imply that households value gains in their after-tax income through tax savings more than through pre-tax earnings increases. This difference in coefficients is explored further later in this paper.

The constant term in the "economic person" test is –1.84, which implies that if income and taxes were equal in both countries, households with individuals who have ever lived in Canada have a 3.3% probability of choosing to live in the U.S.[22] If the earnings and after-tax ratios are both 1.1, this probability shifts to 4.9%.[23]

The ensuing columns present results for specifications with additional variables and/or subsets of the data. Note that the tax and income coefficients remain positive and significant.

The second column presents results when non-economic variables are considered. In this case the tax effect drops considerably, but the income and tax effects both remain significant at the 99.9% level.

The coefficients for the non-economic variables, while not the central focus of this paper, are nevertheless interesting. A person is more likely to choose the U.S. the older he/she is. This reflects the fact that a large proportion of Canadians in the U.S. moved there in the 1950s and 1960s, so those people would naturally tend to be older.

[22] This probability is computed by taking the cumulative normal function at -1.84.
[23] This probability is the cumulative normal function at $(0.42)\log(1.1) + (1.52)\log(1.1) - 1.84$.

Choosing to live in the U.S. is positively correlated with being married, which may be surprising since one might think unmarried people are more mobile and therefore more likely to move out of Canada. On the other hand, some people may move to the U.S. because they have found a marriage partner there.

Having children is irrelevant to the decision, which provides some evidence against the notion that some people systematically remain in Canada because that is where they want to raise their children.

The coefficients for the education variables are positive for E_{HS}, E_{BACH} and E_{GRAD}, but negative for E_{PS}. Note that income is controlled for, so this variable likely measures non-monetary professional recognition or a probability of U.S. recruitment. Note also that the variables are cumulative. For example, a person whose highest education achievement is a university bachelor's degree will have $E_{HS}=1$, $E_{PS}=1$, $E_{BACH}=1$ and $E_{GRAD}=0$. The negative result for variable E_{PS} is surprising because it implies that a person with a non-university post-secondary diploma is less likely to move than someone without such a diploma (after controlling for income and tax differences). Indeed the non-university post-secondary graduate is less likely to move than someone who did not finish high school, since the coefficient for E_{HS} (0.55) plus the coefficient for E_{PS} (-0.78) is less than zero. In general, though, the higher the education, the more likely a Canadian chooses the U.S., even after controlling for income and tax differences.

Many Canadians associate Canada's higher taxes with Canada's government-provided health insurance (though in fact the U.S. governments spend more per capita on health care than the

Canadian governments[24]). In the U.S. some people bear their own health care costs either by buying private health insurance or by paying their health costs as they are incurred; others receive health insurance from the government or from employer-financed health plans. If one could reasonably predict the health costs a household would bear in the US, it would be interesting to add a variable of $\ln[(1-h_u)/(1-h_c)]$ to the regression, where h_i is the health care costs in country *i* divided by the household's after-tax income. Such a ratio would capture the effect of health coverage costs in a household's residency decision. If the ratio is correlated to tax differences or income differences, then the income or tax ratio coefficient predictions could be affected. At present I do not have data that would permit consideration of this factor.

Near the bottom, Table 3.4 reports the likelihood ratio index, which equals one minus the ratio of the maximized value of the log likelihood function to the log likelihood computed where all coefficients are zero. The possible values of the likelihood ratio index can range between 0 and 1. However, the values have no interpretive significance other than to compare one model with another.[25] This index rises from 0.019 to 0.103 when switching from the "economic person" model to the full model.

The third test, reported in column 3, drops the years 1993 and 1995. The reason for doing so is that the U.S. survey includes respondents two years in a row to the extent possible. For a particular year, a little less than half the respondents are in the previous year's sample and a little less than half are in the following year's sample. Thus there is a certain amount of autocorrelation between consecutive years. Taking a sample only from every second year eliminates this autocorrelation. This step reduces the number of observations, yet the income effect and the tax effect remain statistically significantly at the 99% level.

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[24] Canadian Institute for Health Information and Statistics Canada, *Health care in Canada: A first annual report*, Ottawa: Canadian Institute for Health Information, 2000, page 15.

The fourth and fifth columns only consider individuals who lived in Canada in 1990 or later. That is, U.S. residents were only included in the tests if they immigrated to the U.S. in the 1990s. This test was run because the political and popular discussions on the brain drain centre on people who have left Canada recently. Despite the lower number of observations, the income and tax effects remain statistically significant.

Interestingly, the sign for the log(AGE) reverses to negative in the final two tests. Amongst people who lived in Canada in 1990 or later, younger people are more likely to choose the U.S. than older people.

Table 3.5 reports two variations on my statistical tests. The first modified test is motivated by Statistics Canada's finding that high income individuals are more likely to move to the U.S. than low income individuals. This modified test examines whether individuals with a high absolute income are more likely to move after controlling for relative income differences and other effects used in the "full model" from table 3.4. Accordingly, this test adds a variable equal to the log of the average of the US and Canadian incomes. As shown in column 2 of table 3.5, the coefficient for the absolute income variable is not significantly different from zero. This result suggests that the reasons that high income individuals move to the U.S. are captured by the other variables – such as the relative income variable and the education factors.

The last column in table 3.5 differentiates between university graduates and non-university graduates. Interestingly, university graduates have virtually the same coefficient for their income and tax effect. Their combined income and tax effect has a coefficient estimate of 0.85. Another variable that captures only their tax effect has a coefficient estimate of 0.07, which is not

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[25] Greene (1990), page 682.

Table 3.5
Variations on the Statistical Tests

Description of Variable	Variable	Full Model (only 90s immigrants) (as reported in Table 3.4)	With Absolute Income Effect	Separating Out Univ. Graduates from Non-Univ. Grads
Relative income effect	$\log(Y_u/Y_c)$	0.33 (se=.09;P=.000)	0.36 (se=.10;P=.000)	
Relative tax effect	$\log[(1-t_u)/(1-t_c)]$	1.21 (se=.40;P=.003)	1.35 (se=.41;P=.001)	
Absolute income effect	$\log[(Y_u+Y_c)/2]$		-0.05 (se=.04;P=.181)	
University Graduates:				
Relative after-tax income effect	$E_{BACH} \times \log[Y_u(1-t_u)/(Y_c(1-t_c))]$			0.85 (se=.14;P=.000)
Relative tax effect	$E_{BACH} \times \log[(1-t_u)/(1-t_c)]$			0.07 (se=.54;p=.896)
Non-University Graduates:				
Relative income effect	$(1-E_{BACH}) \times \log(Y_u/Y_c)$			0.06 (se=.07;P=.510)
Relative tax effect	$(1-E_{BACH}) \times \log[(1-t_u)/(1-t_c)]$			1.52 (se=.50;P=.002)
Likelihood Ratio Index		0.130	0.131	0.140

(Other Variables in the "Full Model" not shown.)

Note: The figures in large fonts (other than the number of observations) are coefficient estimates. Underneath these coefficient estimates, the estimated error is reported in small fonts, as well as the P>|t| amount.

statistically different from zero. Thus, the tax effect by itself is not significantly different from the combined income and tax effect for university graduates. This is the result one would expect if households do not care whether they earn an extra dollar of after-tax income through an increase in pre-tax earnings or through a tax savings.

However, for non-university graduates, the statistical tests produce perplexing results. The income effect becomes insignificant while the tax effect remains significant. No convincing explanation has been found for this result. One explanation might be that non-professional movers tend to be households that earn investment income that does not change by switching countries. This hypothesis was indirectly tested by considering age and occupation factors, but these tests provided no support for such an explanation. Another explanation may be that low-skilled individuals face compensating factors when earning income in the U.S. In Canada they may benefit from generous social assistance if they do not work, or only work seasonally. Having to work harder in the U.S. may reduce the appeal of earning a U.S. income. However, it is difficult to believe that compensating factors fully explain the elimination of the income effect.

While there is some variation in estimates across specifications, they all indicate that both income and tax differences do influence emigration. The statistically significant results are quite remarkable, given the small sample size of people who moved to the US, and given the biases towards conservative estimates inherent in the test structure. (These biases arise because income and taxes for the country in which a person does not live, must be estimated and therefore fail to capture the full variation in income and taxes across people. Appendix 3.4 describes these biases in more detail.)

In addition to carrying out these tests, I computed how much smaller the migration would have been in the early 1990s if Canadian taxes had been lower. I considered two experiments. The first experiment involved checking how many less households would have moved if taxes had been identical in both countries. Based on the estimates in column 4 of Table 3.4, the number of households migrating to the US would have declined by 12% and the number of university-graduate households moving south would have declined by 15%. (If I instead used the estimates in the last column of Table 3.5, the number of university-graduate households migrating to the US would be estimated to have declined by 10%.) Since the coefficients are likely underestimated due to the biases described in Appendix 3.4, the true effect of tax differences is likely larger. On the other hand these computed effects of tax differences ignore any corresponding cut in government services.

If *both taxes and income* opportunities had been equal in Canada and the US, the number of households migrating south would have declined by 18% and the number of university graduate households moving south would have declined by 22%. (This last figure would be modified to 41% if the coefficients reported in the last column of Table 3.5 were used.) These figures are also conservative, and are based on data from the early 1990s, before the substantial drop in the Canadian dollar that widened the earnings gap between Canada and the U.S.

The second experiment involved checking how many less households would have moved if Canada's average taxes had been reduced by various percentages. Some results are reported below:

Reduction in Canadian Taxes	Reduction in Migration to the U.S. Across All Households	Households with University Graduates (using Table 3.4 coefficients)	Households with University Graduates (using Table 3.5 coefficients)
5%	4%	5%	3%
10%	8%	9%	6%
15%	12%	13%	9%
20%	15%	17%	11%
25%	19%	20%	14%

As a generalization, a 1% reduction in Canadian average taxes results in a reduction in migration to the US of almost 1%.

An additional, appealing test would have been to check the most talented people's responsiveness to tax differences. However, the sample size is too small to allow this test, and it would be difficult to identify which observations fit that profile. Nevertheless, there appears to be no compelling reason why the most talented individuals would respond to tax savings differently from the average university graduate.

It is remarkable how few people move despite the economic incentives. In the early 1990s, the average university-graduate household could earn 27% more in the U.S. and could pay 18% less taxes for a given income level. Yet, only 2.5% of university-graduate households moved.

3.7 CONCLUSION

Until now, no comprehensive empirical test has informed the debate on whether taxes affect Canadians' decision to move to the US. This paper reports the methodology and results of an empirical test of the hypothesis that taxes play a role.

The tests carried out are fairly demanding, because the tests involve estimating taxes for the country in which people do not live. These estimates contain a certain amount of artificial noise (i.e. mismeasurements of the tax differences that taxpayers really face) and also do not capture all of the variations that taxpayers may benefit from or suffer from if they lived in the opposite country. These factors and others tend to bias my tests towards conservative estimates and statistical insignificance.

Nevertheless, both income differences and tax differences prove to be statistically significant. I also estimate that tax differences account for about 10% to 15% of Canada's migrants to the U.S.

Do tax differences contribute toward Canada's brain drain to the U.S.? Yes.

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Appendix 1.1
Countries in Sample

Donor Countries in Sample

Australia
Austria
Belgium
Canada
Denmark
Finland
France
Germany
Ireland
Italy
Japan
Netherlands
New Zealand
Norway
Portugal
Spain
Sweden
Switzerland
United Kingdom
United States

Recipient Countries in Sample

Albania
Algeria
Angola
Argentina
Bahamas
Bahrain
Bangladesh
Barbados
Belize
Benin
Bermuda
Bhutan
Bolivia
Brazil
Brunei
Burkina Faso
Burundi
Cambodia
Cameroon
Cen African Rep
Chad
Chile
China
Colombia
Comoros
Congo, Dem Rep
Congo, Rep.
Costa Rica
Cyprus
Côte d'Ivoire
Djibouti
Dominican Rep.
Ecuador
Egypt
El Salvador
Equ. Guinea
Ethiopia
Fiji
Gabon
Gambia
Ghana
Guatemala
Guinea
Guyana
Haiti
Honduras
Hong Kong
India
Indonesia
Iran
Iraq
Israel
Jamaica
Jordan
Kenya
Korea, Rep.
Kuwait
Laos
Liberia
Libya
Madagascar
Malawi
Malaysia
Maldives
Mali
Malta
Mauritania
Mauritius
Mexico
Morocco
Mozambique
Nepal
Nicaragua
Niger
Nigeria
Oman
Pakistan
Panama
Papau N. Guinea
Paraguay
Peru
Philippines
Rwanda
Saudi Arabia
Senegal
Seychelles
Sierra Leone
Singapore
Solomon Islands
Somalia
South Africa
Sri Lanka
Sudan
Suriname
Syria
Tanzania
Thailand
Togo
Trin. & Tobago
Tunisia
Turkey
Uganda
U Arab Emirates
Uruguay
Vanuatu
Venezuela
Viet Nam
Zambia
Zimbabwe

Appendix 2.1
Sources of Data

Trade Data

The 1992-1996 trade data was obtained from Industry Canada's Strategis website, currently located at http://strategis.ic.gc.ca/sc_mrkti/tdst/engdoc/tr_homep.html.

Immigration

We obtained from Citizenship and Immigration Canada the number of immigrants to each province from each country for each year. This data was provided by immigrant category. We also obtained the number of immigrants living in each province from the 1986 and 1991 long form census. Based on the data from 1986-1991, we estimated an attrition rate. (The number of immigrants in 1986 plus the number of new immigrants from 1986 to 1991 exceeds the number of immigrants in 1991, due to departures and deaths.) Based on this data, we estimated the stock of immigrants in each year in each province. We calculated the attrition rates for each province separately. The attrition rates varied substantially between provinces, likely reflecting the fact that immigrants move from province to province, and that some provinces are more popular than others. BC showed a very low net attrition rate, while the Maritime Provinces showed a relatively high net attrition rate.

Languages

The common language variable for a country-province observation equals the probability that a randomly selected person from the foreign country and a randomly selected person from the province can speak a common language. Our data on languages spoken in the Canadian provinces was obtained from the 1991 Canadian (short form) census report. The languages considered were those that were listed in the Canadian census. They include the following:

English	Punjabi	Hebrew
French	Arabic	Urdu
Italian	Greek	Persian (Farsi)
German	Tagalog	Croatian
Chinese	Vietnamese	Japanese
Spanish	Hindi	Korean
Portuguese	Hungarian	Tamil
Ukrainian	Russian	Finnish
Polish	Gujarati	Armenian
Dutch	Yiddish	Romanian

Unfortunately, it is difficult to get reliable, consistent information on what languages are spoken in various countries. We had to use a variety of sources and sometimes used our judgment when the sources did not agree with each other. Our country by country data, and our sources for each data entry are available on request.

The following were our sources:

(1) *Ethnologue: Languages of the World* (edited by Barbara Grimes),

(2) *The Encyclopedia of Language and Linguistics* (edited by R.E. Asher),

(3) *The Cambridge Encyclopedia of the English Language* (by David Crystal),

(4) The CIA world factbook country reports (on the world wide web) where we assumed that knowledge of languages corresponds with ethnic groups. This last source was only used for countries that were formerly parts of the USSR, Czechoslovakia or Yugoslavia.

Unfortunately, our data on English speakers is not as consistent as we would have liked. For example, our sources provided no information on how many people in Germany can speak English. This is unfortunate, since clearly a large proportion of Germans can speak English. In general, we had no information on knowledge of English on continental Europe, but we do have information on knowledge of English in other parts of the world, especially Africa and Asia. In the interest of keeping our data as objective as possible, we did not make any adjustments for European countries where from our own personal experience we know there are many English-speakers.

GDP Data

The GDPs of foreign countries were obtained from the World Development Indicators CD, produced by the World Bank. Countries for which no such data was available, were excluded from the data. The most significant excluded countries were Afghanistan, Cuba, French Guiana, Myanmar, and North Korea. Taiwan was missing from the World Bank's data, but is too large an economy to exclude, so we obtained data for Taiwan from the Asian Development Bank's website (http://www.adb.org). The GDPs of the Canadian provinces were obtained from Statistics Canada. The data were on the CANSIM matrices 2623-2631 and 6949.

Population Data

We obtained the populations of foreign countries from the World Development Indicators CD, produced by the World Bank. For Taiwan, we obtained the populations from the Asian Development Bank web page. The populations of the Canadian provinces were obtained from the Statistics Canada web page. (http://www.statcan.ca)

Distances

The distance between a country and a province is the distance between the foreign country's major city and the province's major city. (For the U.S. we use Chicago as the major city, since it is more central than New York.)

Distances between cities were measured as follows:

(1) For countries in continental North America we use the great circle distance between the major city of the particular country and the major city of the particular province. Shipping from these countries can occur by land, and the land distance would not differ substantially from the great circle distance.

(2) For countries that would use an eastern port, we use the great circle distance between Halifax and the foreign country, and then add the road distance between Halifax and the major city of the particular

province. We use this measure in place of strictly using the great circle distance because for Canada's biggest trading partners (apart from the U.S.) the great circle route passes through arctic territory, where no surface shipping can occur. For example, the distance between Great Britain and British Columbia is taken to be the great circle distance between London and Halifax (2,883 miles) plus the road distance between Halifax and Vancouver (3,842). Thus the total distance is 6,725 miles. Had we used the great circle distance between London and Vancouver, it would have been 4,721 miles. So the London-Vancouver distance would have been 1.64 times the London-Halifax distance. However, using our composite distance, the London-Vancouver distance is 2.33 times the London-Halifax distance, which is more realistic in terms of shipping distance. Many goods do not actually get shipped through Halifax, but are brought into Quebec or Ontario through the St. Laurence Seaway. The distance ships travel along this seaway approximately equals the road distance. The countries using the east port include all European countries, all African countries, all South American countries on the east side, all Caribbean countries, and all Middle Eastern countries with access to the Mediterranean Sea.

(3) For countries that would use a western port, we use the great circle distance between Vancouver and the foreign county, and then add the road distance between Vancouver and the major city in the particular province. The countries we regard as using the western port include all Asian countries (other than those with access to the Mediterranean Sea), all Pacific islands and all western countries in South America.

(For Colombia we just use the great circle distance, since it has coastal access both to the Pacific Ocean and the Atlantic Ocean.)

When we tried using strictly the great circle distance as our distance measure, we obtained distance parameter estimates between –1 and –1.5 when we excluded fixed country effects from our model. (These estimates correspond with normal results.) But the parameter estimates jumped to about -2.5 when we introduced fixed country effects. The introduction of fixed country effects means that the distance variable is no longer reflecting the distance between a province and a foreign country, but is reflecting a province's distance relative to the other provinces.

It turns out that the choice of distance measurements has little effect on the parameter estimates of immigration, and little effect on the regression's root mean squared error.

Appendix 2.2
Regression with the log of Immigrants as a Dependent Variable

Variable	With Fixed effects	Without Fixed Effects
COMLANG	1.97	3.60
	se=.14;p=.000	se=.17;p=000
$\log(POP_p)$	0.66	0.57
	se=4.00;p=.870	se=2.01;p=.776
$\log(POP_f)$	0.94	2.33
	se=.02;p=.000	se=1.17;p=.047
$\log(Y_p/POP_p)$	-1.19	-1.22
	se=3.67;p=.747	se=1.84;p=.508
$\log(Y_f/POP_f)$	0.69	-0.05
	se=.02;p=.000	se=.16;p=.774
$\log(D_{pf})$	-0.80	-0.84
	se=.08;p=.000	se=.09;p=.000
BC_{DUMMY}	0.33	0.30
	se=1.17;p=.780	se=.59;p=.605
$ONTARIO_{DUMMY}$	1.00	1.06
	se=3.22;p=.756	se=1.62;p=.513
$QUEBEC_{DUMMY}$	0.12	0.18
	se=1.78;p=.948	se=.89;p=.844
$MARITIME_{DUMMY}$	-0.21	-2.22
	se=2.87;p=.458	se=1.44;p=.125
Adj. R^2	0.62	0.90
Root MSE	1.93	0.97
# of obs.	2,935	2,935

Notes: Figures in large fonts are the coefficient estimates. In small fonts beneath each estimate we report the estimate's estimated error and the corresponding probability statistic. Statistics for the year dummy variables and the constant are not shown.

Appendix 3.1

Hypothetical Income Predictions

As reported in section 3.4 of this thesis, 96 regressions were used to compute hypothetical income. A separate regression was run for each occupation class within each country. There are 47 occupation classes plus one class for people with no occupation. Since some readers may be curious about these coefficients and how Canada and the U.S. differ, I report results from regressions that combine all the occupation classes, but exclude the "no occupation" class.

Specification:

$$\log Y = \beta_1 AGE + \beta_2 AGE^2 + \beta_3 SEX + \beta_4 MARRIED_M + \beta_5 MARRIED_F + \beta_6 MWC_M + \beta_7 MWC_F + \beta_8 E_{HS} + \beta_9 E_{PS} + \beta_{10} E_{BACH} + \beta_{11} E_{GRAD} + \beta_{12} YR_{93} + \beta_{13} YR_{94} + \beta_{14} YR_{95} + \beta_{15} YR_{96} + C + \varepsilon$$

	Canadian Coefficients	U.S. Coefficients
AGE	.165	.118
AGE^2	-.00183	-.00123
SEX	-.099	-.165
$MARRIED_M$	.395	.332
$MARRIED_F$	-.100	-.104
MWC_M	-.024	.045
MWC_F	-.089	-.192
E_{HS}	.194	.478
E_{PS}	.128	.244
E_{BACH}	.233	.238
E_{GRAD}	.160	.302
Y_{93}	-.014	.036
Y_{94}	.001	.068
Y_{95}	.000	.122
Y_{96}	.068	.174
C	6.194	6.660

All of the above results are significantly different from zero at the 99.9% level except the 93-95 year dummies in the Canadian column.

The most interesting coefficients are the gender variables and the education variables. The coefficients for variable SEX (0 if male, 1 if female) imply that on average an unmarried female in Canada earns ($e^{-.099}$=) 91% of the amount an unmarried male with identical education earns. The U.S. figure is ($e^{-.165}$=) 85%. The gap widens considerably for married individuals. Among married individuals with no children the ratio of female to male earnings holding education constant is 55% in both Canada and the U.S. ($e^{-.099-.100-.395}$ in Canada and $e^{-.165-.104-.332}$ in the U.S.) Since it is not the purpose of my tests to compare male and female earnings, my tests do not control for other factors, such as hours worked at a paying job. Readers interested in pay inequity between the genders are referred to the literature on the subject.[1]

The education coefficients are also interesting. The difference in earnings between more educated and less educated people is much larger in the U.S. than in Canada. Earnings differences expressed as ratios are as follows:

	Relative to no high school	Relative to high school graduate	Relative to post-secondary graduate	Relative to bachelor's degree holder
Canada:				
High School	1.21			
Post-secondary	1.38	1.14		
Bachelor degree	1.74	1.43	1.26	
Graduate degree	2.04	1.68	1.48	1.17
U.S.				
High School	1.61			
Post-Secondary	2.06	1.28		
Bachelor degree	2.61	1.62	1.27	
Graduate degree	3.53	2.19	1.72	1.35

Brojas, Bronars and Trejo (1992) find that individuals with high skill levels tend to move toward regions that offer higher rewards for skills. Their finding is corroborated by the above data combined with Statistics Canada's finding that higher educated Canadians are more likely to move south (as reported in section 3.2.1).

[1] See, for example, Jacobs, 1992.

Appendix 3.2

A Very Brief Description of U.S. Tax Rules

(Note: The descriptions in this appendix and Appendix 3.3 are only intended to provide a general overview of the structure of the taxation of individuals in the U.S. and Canada. The actual rules are quite complex and there are numerous exceptions to the rules described below.)

Federal Taxes

In the U.S. a taxpayer's tax brackets and the standard deduction are dependent on the taxpayer's filing status. The filing statuses are:

- *Single*: Single individuals with no dependents,
- *Married filing joint (MFJ)*: married couples who report their income together on a single tax return,
- *Married filing separately*: married couples who do not file MFJ, either by choice or some prohibition,
- *Head of household*: single individuals with one or more dependents.

Next, "adjusted gross income" (AGI) is determined. AGI includes employment, business, investment and other income. Certain deductions, such as deductible contributions to an Individual Retirement Account (IRA) are also permitted in arriving at AGI.

Taxable income equals AGI minus the following:

1. *The greater of itemized deductions and the standard deduction.* Itemized deductions are allowed for expenses such as medical costs, state and city income taxes, real estate taxes, home mortgage interest and donations. The alternative standard deduction is a fixed amount for a given year and filing status. In 1996, the standard deductions were as follows:

Single:	$4,000
Married filing joint	6,700
Married filing separately	3,350
Head of household	5,900

2. *Exemptions*. A taxpayer gets a deduction ($2,550 in 1996) for each "exemption". An exemption can be claimed for oneself, the spouse (if filing jointly), and each qualifying dependent.

Federal tax is computed from taxable income at the following rates (in 1996) for the single, MFJ and head of household filing statuses:

Bracket	Range of Taxable Income		
	Single	MFJ	Head of Household
15%	0 - 24,000	0 - 40,100	0 - 32,150
28%	24,000 - 58,150	40,100 - 96,900	32,150 - 83,050
31%	58,150 - 121,300	96,900 - 147,700	83,050 - 134,500
36%	121,300 - 263,750	147,700 - 263,750	134,500 - 263,750
39.6%	263,750 +	263,750 +	263,750 +

State and City Taxes

Several states impose no income tax. Most other states impose taxes on their own taxable income computations, often using the federal AGI as a starting point. The marginal state tax rates range from 0-12%. A few cities, such as New York, also impose income taxes.

Payroll Taxes

The U.S. imposes a social security tax on employees and employers. The rates are 7.65% on earnings up to $62,700 (in 1996) and 1.45% on earnings above this threshold. Employers must match the employees' payments. Self-employed taxpayers must pay both the employee's and employer's portion.

Appendix 3.3

A Very Brief Description of Canadian Tax Rules

(Note: See the disclaimer at the beginning of Appendix 3.2.)

Federal Taxes

Canadian personal income taxes apply to individuals; married couples cannot file jointly as they can in the U.S. The first step to computing Canadian federal taxes is to compute "net income". Net income includes employment income, business income (net of expenses), investment income (net of expenses) and other income. In computing net income individuals may also deduct contributions to registered retirement savings plans (RRSPs), union dues, moving expenses, certain child care expenses and certain other expenses. Net income minus certain other deductions (such as losses carried over from other years) equals taxable income. The taxable income is used to compute a gross tax amount at the following rates:

Taxable income of	0 to $29,590:	17%
	$29,590 to $59,180:	26%
	$59,180 and up:	29%

This gross amount is reduced by certain credits. The most common credits are 17% of the following amounts:

- Basic amount: $6,456
- Married amount: 5,380 (applies where the spouse earns $538 or less; the amount is fully phased out where the spouse earns more than $5,918)
- Children (until 1992): 417 per child plus an additional $417 for each child after the second one
- Certain amounts for tuition, education, blindness, being 65 or older, medical expenses, and donations up to $200.

In addition, a credit of 29% of donations in excess of $200 is allowed. The gross tax less these credits (and a few other, less common adjustments) is the "basic federal tax". The federal government then imposes a 3% surtax (4.5% in 1992) on basic federal tax plus another 5% surtax of basic federal tax in excess of $12,500.

Provincial Taxes

All provinces except Quebec compute their income taxes with reference to the basic federal tax. The basic provincial tax rates range from 45.5% to 69% of basic federal tax. Three provinces also levy an additional "flat tax", ranging from ½% to 2% of income. Some provinces offer some tax relief for low-income individuals. Unlike the other provinces, Quebec bases its provincial income taxes on its own rules for computing income, deductions and credits.

Payroll Taxes

Canada requires people earning employment and/or business income to contribute to the Canada Pension Plan (CPP). The rates and thresholds change each year. In 1996 employees had to contribute 2.8% of earnings in excess of $3,500, up to a maximum contribution of $893. The employer contributes the same amount. Self-employed individuals contribute both the employee and employer shares.

Employees are also required to contribute to Canada's Employment Insurance (EI) program. In 1996 the employee's contribution was 2.95% of employment earnings up to a maximum contribution of $1,151.

Appendix 3.4

Biases

My statistical results are statistically significant, yet the income and tax coefficients are likely underestimated due to three biases that push these coefficient estimates down.

The first bias arises because variation in the income and tax rates are only fully captured in the actual data, not the hypothetical data. As a result, the *measured* income and tax ratios only capture part of the *true* variation and hence the income and tax coefficients are biased towards zero.

In a related problem, the *measured* tax ratios may have a larger random fluctuation than the *true* tax ratios, because some deductions and credits claimed in one country may also be claimable in the other country. For example, an individual in Canada may have his/her taxes substantially reduced by a deduction for alimony payments. This deduction will not be fully captured in the hypothetical U.S. taxes; effectively only the expected value of alimony deductions is incorporated into all taxpayers' hypothetical U.S. taxes. Therefore, Canadian residents paying alimony will tend to have a substantially understated after-tax ratio $(1\text{-}t_u)/(1\text{-}t_c)$ while Canadian residents not paying alimony will tend to have a slightly overstated ratio. Overall, this creates artificial fluctuations in the ratio, reducing the t-statistic for the coefficient estimate.

A second bias, which applies only to the income coefficient, arises because I use the home country residual to estimate the foreign country income. I chose this method of estimation to reduce the noise in the income ratio, because I believe that a substantial part of the home country residual relates to factors that affect a person's earning capabilities in both countries. Recall that estimated income for the country in which a person does not live is as follows:

$$E(\log Y_{f,p}) = \log\hat{Y}_{f,p} + (\log Y_h - \log\hat{Y}_{h,p})(s_{f,p}/s_{h,p})$$

We can think of $(\log Y_h - \log\hat{Y}_{h,p})$ as equaling $(\mu + \varepsilon_h)s_{h,p}$, where μ is the component of the residual that is common to both countries and ε_h is the component that only applies to the home country. In effect, then, I use $(\mu + \varepsilon_h)s_{f,p}$ to estimate $(\mu + \varepsilon_f)s_{f,p}$, so the logged foreign income is overstated by $(\varepsilon_h-\varepsilon_f)s_{f,p}$. Due to selection bias, we expect this amount to tend to be positive. Consequently, the income ratio (Y_u/Y_c) for Canadian residents will tend to have an overestimated numerator while the income ratio for U.S. residents will tend to have an overestimated denominator. These mis-measurements produce a high ratio for stayers (Canadian residents) and a low ratio for movers (US residents), resulting in a downward bias in the income ratio's coefficient.

A third bias, which applies only to the tax coefficient, arises because my computation of the tax ratio is based on an estimate of foreign taxes using the expected foreign income at a *single* point. This computation does *not* produce an unbiased estimate of the tax ratio, because there is in fact a distribution of incomes that might apply to a particular individual. Since the relationship of taxes on income is generally convex, using a single income point produces a lower tax estimate than using a probability-weighted distribution of income points. Thus, foreign taxes are underestimated. Consequently, a Canadian resident's tax ratio $(1-t_u)/(1-t_c)$ gets overstated because t_u is understated, while a US resident's tax ratio gets understated because t_c is understated. As a result, the estimate of the tax coefficient is biased downwards. If the income variance $(\varepsilon_f\, s_{f,p})$ were known, this bias could be corrected. While I can approximate $(\mu + \varepsilon_f)s_{f,p}$, I cannot determine how much is attributable to μ and how much is attributable to $\varepsilon_{f,p}$. Thus, I cannot correct this bias.

Fortunately, all the biases identified here push the coefficient estimates towards conservative results. This fact suggests that we can be confident that the true income and tax effects are at least as strong as reported in table 3.4.