Università Commerciale “Luigi Bocconi” Milano
XIX Cycle
Ph.D. in Economics

ESSAYS IN DEVELOPMENT
AND LABOUR ECONOMICS

SILVIA REDAELLI
Matr. N° 935379

Thesis Committee:
Prof. Eliana La Ferrara, Bocconi University
Prof. Tito Boeri, Bocconi University
Prof. Imran Rasul, University College London

Anno Accademico 2007-2008

Index

Introduction

The present thesis investigates topics in Development and Labour Economics.

In the first chapter, joint with Gunther Fink, "Determinants of International Emergency Aid: Humanitarian Need Only?", we develop and use an original data set covering more than four hundred recent natural disasters to analyze the determinants of international emergency aid. Although we find that humanitarian need is a major determinant of emergency relief payments, our results imply that political and strategic factors play a crucial role in the allocation of emergency aid. On average, donor governments favor smaller, geographically closer and oil exporting countries, and display a significant bias in favor of politically less aligned countries as well as towards their former colonies. Furthermore, we find strong evidence for bandwagon effects in humanitarian assistance, a result which supports recent developments in the humanitarian policy field going in the direction of a more centralized management of emergency aid's funds.

The second chapter, "Be as careful of the books you read as of the company you keep. Evidence on peer effects in educational choices", is a joint work with Giacomo de Giorgi and Michele Pellizzari. The main focus of this paper is on the empirical identification of endogenous peer effects. Using administrative data on Bocconi University's students we explore whether peers' behavior has a significant effect on the choice of college major. Taking the advantage of the rich administrative data at hand we show how peer-groups' network structure can provide a natural set of exogenous instruments for the clean identification of peer effects. Our results show the effective importance of peer effects, and how these effects can divert individuals' choice from majors for which they have experienced relative ability advantages hence leading to potentially suboptimal outcomes.

Finally, in the third and last chapter, "Sibling Rivalry and Early Marriage: Evidence from Rural Malawi", I analyze the institution of early marriage. My goal is to understand how marriage institutions affect the age at which women get married. In particular, using data from Malawi's Demographic and Health Survey, I investigate how dowry and brideprice

traditions interact with sibship structure. The identification strategy relies on the coexistence of matrilineal and patrilineal kinship systems, respectively used to dowry and brideprice transfers at marriage, and exploits within groups variations of the exogenously determined gender composition of siblings and of birth order. After controlling for the demographic composition of the natal family, for cohort and district variations, I find that, on average, women in matrilineal groups tend to marry relatively younger the higher the number of older sisters, whereas older brothers reduce average age at marriage for women in patrilineal groups.

Chapter 1

Determinants of International Emergency Aid: Humanitarian Need Only?

Günther Fink

Silvia Redaelli

1 Introduction

The magnitude and impact of recent disasters like Hurricane Katrina and the December 2004 Tsunami have brought natural emergencies into the international spotlight. Rapid population growth, urbanization, environmental degradation and climate variability have increased the vulnerability to, and impact of, natural hazards, especially in less developed countries (Abramovitz, 2001). As a result, natural disasters have caused an average loss of 63,500 human lives annually, and affected more than 212 million people per year in the period from 1990 and 2005.[1]

Despite several initiatives towards disaster prevention[2], humanitarian relief remains the principal channel of support for countries hit by natural disasters. With a growing range of issues falling into the humanitarian agenda, and rising attention from national governments, total bilateral emergency aid has increased from US$ 3.2 billion to US$ 8.5 billion between 1995 and 2005 (OECD, 2007). The increasing importance of emergency aid is also apparent in the size of emergency aid relative to total official development assistance (ODA), which has shifted from 5% in 1989 to 10.5% in 2000 (Macrae, 2002).

From a theoretical perspective, the objectives and criteria of humanitarian aid are well defined. The United Nations General Assembly resolution 46/182 states that emergency assistance shall "*..be provided in accordance with the principles of humanity, neutrality, impartiality and independence*" (United Nations RES/46/182, 1991, page 1). Humanitarian assistance is designed to alleviate human suffering in emergency situations, independent of race, citizenship and other political considerations. Despite these principles, concerns regarding the allocation of emergency aid have mounted over the last years, and international aid policies increasingly been exposed to criticism from both private aid organizations and the popular press (Darcy and Hofmann, 2003; IFRC, 2003; Olsen et al., 2003; Walker et al., 2005). In particular, humanitarian agencies engaged in relief operations have de-

[1] Source: EM-DAT Emergency Disasters Data Base.

[2] Among others, the UNDP's Disaster Risk Index Project (DRI) was designed to improve the understanding of the relationship between development and disaster risk, and to provide country vulnerability analysis during the International Panel on Climate Change (IPCC) in 2001. More recently, the international community has recognized disaster risk management as an integral part of the development agenda (2005 World Conference for Disaster Reduction, Kobe, Japan).

nounced the existence of "forgotten" or "silent" emergencies receiving little or no help from the international community, while other emergencies receive disproportionate amounts. With emergency aid determining not only the immediate fate of affected populations, but likely also affecting the medium to long run development of countries, these concerns are serious, and demand a closer analysis of international humanitarian aid.

In this paper, we provide the first large scale analysis of emergency aid. Using a sample of more than 400 calamities occurring worldwide over the last 15 years, we analyze how the international community responds to humanitarian crises triggered by natural disasters, and evaluate the degree to which international aid flows reflect the humanitarian principles they are officially based upon. Narrowing the scope of our analysis to *rapid onset* natural emergencies[3], we take advantage of natural disasters as exogenous shocks allowing us to clearly distinguish humanitarian from politically or strategically based motivations. Once we control for disaster impact as measured by the number of people killed and affected, it becomes straightforward to test whether political and strategic factors affect the allocation of emergency aid. Since disaster related needs may depend on country specific conditions, we allow for a large set of socioeconomic factors in all of our empirical specifications.

Our empirical work is divided into three parts. In the first part, we pool all donors to assess the average performance of donor governments. We find that a one standard deviation increase in the number of people affected increases the likelihood of receiving aid by 10 to 13 percentage points, while a one standard deviation increase in the number of people killed by a calamity increases the likelihood to receive aid by about 25 percentage points. Our results indicate that bilateral and strategic factors play a crucial role in the allocation of emergency aid. On average, donor governments provide significantly more aid to oil exporting countries, and give disproportionately more to geographically closer and politically less affine countries, as well as to their former colonies.

In a second step, we take a closer look at the five most active donors in emergency aid, namely the US, Japan, Germany, the UK and Norway, and

[3] As explained in Section 3 of the paper, rapid onset emergencies last only for very short periods of time, thus limiting potential feedback effects from aid on the actual impact of the hazard.

compare the aid patterns of these donors to private, non-governmental aid flows. We find that the factors driving the participation decision (selection) and the actual amounts of aid provided (allocation) vary substantially across donors. The US and the UK provide significantly more aid to oil exporting countries, a bias that cannot be detected in private emergency aid flows. Germany displays a significant "home bias", preferring closer emergencies to more distant ones. All of the five major donors except Japan seem to be more generous towards countries less politically aligned in their recent UN voting history, suggesting that emergency aid is used by donors for bridging the gap to countries with diverging foreign policy objectives.

As a last step, we use our data set to analyze the degree of strategic interaction among donors. Instrumenting other donors' aid responses with bilateral distance variables, we find strong evidence of bandwagon effects in the international allocation of emergency aid. On average, the likelihood to provide aid after a natural emergency increases by 15-30 percentage points when any other major donor participates in the aid process.

The work presented in this paper naturally complements and builds on the existing literature on the allocation of development aid. Starting with the pioneering works by McKinlay and Little (1977), a large number of studies have attempted to separate recipient needs (RN) from donors interests (DI) in the allocation of development aid. Alesina and Dollar (2000) find strong evidence for strategic biases towards former colonies and political allies, while Neumayer (2003) finds civil and political rights to be a major factor in aid allocation. Neumayer (2003a) analyzes the aid allocation of development banks and United Nations agencies and finds that most regional development banks focus exclusively on economic need of the recipient, while UN agencies also take human development aspects into account. Berthélemy and Tichit (2004) use a three-dimensional panel to test and reject the equality of aid criteria across donors, and stress the increasing importance of trade connections in the allocation of aid.

Tarp et al. (1999) and Berthélemy and Tichit (2004) also estimate interactions among donors using total (per capita) commitments provided by other bilateral donors in their empirical specifications. While the first study points towards aid coordination among donors, Berthélemy and Tichit (2004) find that these results are not very robust to model specification[4].

[4] The estimated coefficients on other donors' aid are positive in Tobit estimates, but

Round and Odedokun (2004) measure "peer pressure" as the total aid effort of all other donors as a fraction of their total GDP, and find peer pressure to have a positive and significant impact on the aid given by each donor.

Closely related to this paper is also recent work by Eisensee and Strömberg (2007) on US disaster relief payments. The authors show that disaster types differ in terms of their news coverage or "newsworthiness", and highlight the significant and large effects of this media channel on the disaster aid allocation by US government agencies.

The rest of the paper is structured as follows. In section two, we briefly discuss the role and size of emergency aid in the domain of international aid. We present the data in section three, our main empirical results in section four, and conclude with a short discussion and a summary.

2 Emergency aid vs. Development assistance

International aid is broadly divided into two categories: Official Development Assistance (ODA) and Humanitarian Assistance, commonly referred to as emergency aid. ODA consists of financial flows to developing countries aimed at the promotion of their economic development and welfare. To qualify for receiving this kind of assistance - which is by definition concessional and has a grant element of at least 25% - countries have to be classified as potential recipients by the Development Assistance Committee (DAC).[5] The main objective of ODA is the elimination of poverty and its principal causes, which implies considerable involvement of recipient countries in the negotiation and implementation of intermediate to long term programs.

Humanitarian assistance, on the other hand, is meant to provide rapid assistance and distress relief to populations temporarily needing support after natural disasters, technological catastrophes or conflicts, generally classified as "complex emergencies"[6]. Historically, humanitarian assistance has

negative when only the initial selection equation is estimated using Probit models.

[5] The DAC list is reviewed every three years. As of 2005, this list includes all low and middle income countries, except those that are members of the G8 or the European Union (or countries with a firm date for EU admission, i.e. Bulgaria and Romania).

[6] The official definition of a complex emergency is "a humanitarian crisis in a country, region or society where there is total or considerable breakdown of authority resulting from internal or external conflict and which requires an international response that goes beyond the mandate or capacity of any single agency and/ or the ongoing United Nations

been considered a distinct form of aid mostly due to its ethic foundations in humanitarian law. The principles governing humanitarian assistance were to be reflected in the fact that donor governments perceive emergency aid as politically unconditional, while development assistance has always been conditional. Humanitarian aid does not target nations or states and their development, but individuals, independent of race, country or citizenship.

In practice, the distinction between humanitarian and development aid is not always straightforward. Frequently, emergencies like civil wars or droughts spread over months, if not years; it is not clear, how medical facilities established during these kinds of events can be distinguished from generic investment into health infrastructure typically part of ODA programs.

In the case of natural disasters, this distinction is generally less of an issue. As we will show in the following section, the vast majority of natural disasters are classified as "rapid onset", i.e. emergencies triggered by short lived causal phenomena requiring immediate, and only temporary assistance. The short time horizon in which aid has to be delivered limits the room for negotiations between recipient and donor countries, and requires a serious (humanitarian) commitment of donors, who are generally also directly responsible for the coordination of the aid interventions.

3 The Data

3.1 Data Sources

The main source of emergency data is the Emergency-Events Database (EM-DAT) maintained by the Centre for Research on the Epidemiology of Disasters (CRED)[7]. The EM-DAT database covers over 15,900 natural and technological disasters since the beginning of the twentieth century. A disaster is defined as "*a situation or event which overwhelms local capacity, necessitating a request to the national or international level for external assistance, or is recognized as such by a multilateral agency or by at least two sources, such as national, regional or international assistance groups and*

country program." (IASC, 1994).

[7] http://www.em-dat.net/.

the media".[8]

The entry criterion for an event to be classified as natural disaster is to either have caused at least 10 fatalities, affected at least 100 people, to have triggered a declaration of state of emergency, to have led to an appeal for international assistance, or a combination of any of the above criteria. EM-DAT draws from a variety of public sources, including reports by governments, insurance companies, press and aid agencies. In 2003, about 27.9% of the data came from various US Government disaster agencies, 27% from insurance companies, 20% from UN organizations, 13.1% from press agencies, and the remaining 7% from various humanitarian organizations (Guha-Sapir et al., 2004). The EM-DAT database contains information on the severity of each disaster in terms of the total number of people killed (persons confirmed dead or missing and presumed dead) and affected (people requiring immediate assistance during the emergency period, including displaced or evacuated people).[9] From the EM-DAT we also get information on disaster type, country of occurrence and the timing of each emergency, which we use to merge disaster characteristics with funding records.

The funding data we use in this paper, together with donors breakdown, come from the UN's Office for the Coordination of Humanitarian Affairs (OCHA) Financial Tracking System (FTS)[10]. FTS data on natural disaster funding start in 1992 and include governments', together with private, NGOs' and international agencies' responses to Consolidated Appeals.[11] These data are quite different from the ones in the OECD's DAC system commonly used in the ODA literature. Humanitarian aid as defined in the DAC's reporting scheme ("emergency and distress relief") contains "sudden natural or man made disasters, including war or severe civil unrest, food

[8] http://www.em-dat.net/glossary.htm#D

[9] The EM-DAT database includes figures on the "estimated damage" in US dollars. However, given the absence of a standard procedure to quantify the economic impact, and considering the number of missing values for this variable, we decided to rely exclusively on figures of emergencies' human impact.

[10] The United Nations Office for the Coordination of Humanitarian Affairs (OCHA) is mandated to coordinate the international humanitarian response to a natural disaster or complex emergency acting on the basis of the United Nations General Assembly Resolution 46/182. (http://ocha.unog.ch/fts/index.aspx)

[11] Since its creation in 1998, the OCHA has the responsibility to issue an international appeal for aid when requested by affected governments. The Consolidated Appeal is the reference document on the humanitarian strategy and the funding requirements through which the OCHA coordinates and mobilize humanitarian aid in response to natural disasters and complex emergencies.

scarcity conditions arising from crop failure owing to drought, pest and diseases, as well as support for disaster preparedness" (OECD, 2007). The DAC data on emergency aid does thus not only contain large amounts of complex emergency aid as discussed in the previous section, but also expenses made for disaster prevention and refugee support.

Using the FTS data has three main advantages: first, while the DAC system provides only annual totals for each donor-recipient pair, FTS records aid provided for each appeal separately, hence allowing to directly link aid flows to each individual disaster. Second, as opposed to the DAC system which primarily focuses on OECD donors, the FTS tracks aid flows of multilateral and private donors, providing an interesting alternative dimension to be explored in our empirical section. Third, FTS is not restricted to developing countries, so that the recipient pool covers a much broader spectrum of socioeconomic backgrounds.

One potentially important shortcoming of FTS data is that donors' reporting to the OCHA system is on a voluntary basis. To evaluate the magnitude of potential under-reporting, we compare the FTS data used in this paper with DAC data in Figure 1 below. While the DAC numbers are significantly higher than the numbers from the FTS on aggregate, differences are only minor once we exclude complex emergencies.[12]

[**Figure 1: FTS and DAC Data on Emergency Aid**]

We complete the data set with socioeconomic information on recipient countries from the World Development Indicators (World Bank, 2006) and distance data from Gleditsch and Ward (2001). As proxy for the political ties between donors and recipient countries, we use the Gartzke index of similarity in states' voting patterns in the United Nations General Assembly (Gartzke, 2002). Complete summary statistics and a detailed description of the variables used are provided in Table A.1 in the Appendix.

[12] As pointed out by Randel and German (2002), the bulk of humanitarian assistance has been spent on complex emergencies in recent years. For instance, in 2001, the 20 countries appealing for complex emergencies raised a total pledge of $2.1 bn as opposed to a total contribution of only $311.2m received by the 49 countries hit by natural disasters.

3.2 Data Description

Total aid granted by the international community for the 491 emergencies in our sample amounts to US$ 3.06 billion dollars[13]. Total aid includes bilateral and multilateral aid, as well as donations from private sources. In Figure 2, we show a break down of total aid by state or institution, and rank donors in terms of the total amount granted and in terms of the number of emergencies assisted.

The USA result as the leading donor both in terms of the number of interventions and total aid provided, whereas the UK is second in terms of total aid provided, and Japan is second in terms of the percentage of emergencies assisted.

[**Figure 2: Major Donor Countries and Institutions**]

As shown in Tables 1a and 1b, the degree of coordination within the international community is rather small. The correlation of aid interventions is strictly below 0.5 (Table 1a) whereas the correlation of the actual amount given (Table 1b) ranges from 0.59 between Germany and Japan to only 0.18 between Norway and the United States.

In Figure 3, we summarize total contributions by donor and year. Total contributions vary significantly across years, and do not show a clear time trend for any donor. The aggregate data show little evidence of fixed annual budgets, and the correlation between total expenditure per country and the number of calamities appears fairly low. Another important source of variation in our data set is the geographic distribution of disasters. On aggregate, Asia has the largest number of disasters, with South and South-East Asian countries accounting for 73% of disasters over the entire period. Of the 111 recipient countries in our sample, Indonesia is the most exposed one, with 25 natural disasters, followed by India (18), and the Philippines (16). Floods are the most frequent natural disaster type, representing 49% of the sample, followed by wind storms (21%) and earthquakes (15%).

[**Table 1a: Correlation of Aid Interventions Among Major Donors**]

[13] The numbers are denominated in real 2000 US$ at PPP and do not include the December 2004 Indian Ocean Tsunami. The Tsunami has triggered unprecedented aid flows of over US$ 12 bn - about four times the amount of emergency aid provided to all of the disasters in our sample - and therefore is hardly comparable with the typical disaster in our sample.

[**Table 1b: Correlation of Aid Amounts Among Major Donors**]

[**Figure 3: Total Emergency Aid by Year and Donor**]

[**Table 2: Impact by Disaster Type**]

Strong differences in terms of human impact, measured by the number of people killed and affected[14], are also visible across disaster types. Natural emergencies can be broadly classified into rapid onset emergencies lasting only for short periods of time such as earthquakes or floods, and slow onset emergencies, such as droughts or epidemics, which affect populations for longer time periods, in some cases even years. As shown in Table 2, rapid onset emergencies are on average associated with a higher death toll, whereas slow onset disasters tend to affect larger shares of the population.

4 Empirical Strategy

The main goal of our analysis is to determine the factors driving donors' interventions, and to clearly distinguish the relative importance of disaster impact and aid need from factors reflecting donors' strategic and political considerations. As shown in the previous section, the US as the most "active" donor country provides aid for about half of the emergencies in our sample, and participation probabilities are significantly lower for all other donors. The median number of donors for each emergency is five, with one quarter of all emergencies being assisted by no more than three donors. Given the low average participation rates, we dedicate the first part of our analysis to estimating the initial selection equation only, and then jointly estimate selection and allocation in a second step.

We structure our empirical analysis into three parts. In the first part, we exploit our data set's multidimensionality by taking emergency-donor pairs as unit of analysis in a panel setup similar to previous work on ODA by Berthélemy and Tichit (2004). This approach allows us to estimate the importance of each of our explanatory variables for the average donor in our sample under the assumption that the factors driving aid decisions are the same across donors.

[14] People affected are defined as those requiring immediate assistance during an emergency situation; people killed are persons confirmed dead and persons missing or presumed dead (source: EM-DAT). See Table A1 in the Appendix.

In the second part of our analysis, we loosen this restriction, and allow bilateral effects to differ across donors by switching our analysis to the individual donor level. We focus on the five major donors and separately estimate both selection (Probit model) and allocation (Tobit model) equations. In the last part of our empirical section, we allow for interactions between donors and test the degree to which each donor's participation probability depends on other donors' actions. To deal with the endogeneity concerns arising in the estimation of strategic interaction effects, we use bilateral controls to instrument for other donors' participation decision.

The set of explanatory variables used in our empirical analysis can be divided into five broad categories: measures of disaster impact (DI), measures of socioeconomic background (SE), policy performance variables (PP), measures of bilateral relations between donor and recipient (BR), and other additional controls (OC).

Disaster Impact Measures

Our main measures for humanitarian need and disaster impact are the number of people affected and the number of people killed in each emergency. In the EM-DAT system, a person is registered as killed if confirmed dead, missing or presumed dead. A person is counted as affected if she requires immediate assistance during the emergency period, which includes displaced or evacuated people. One potential empirical concern regards the exogeneity of humanitarian impact. If international support was quickly and effectively disbursed, it could reduce emergencies' human impact, and thus induce a downward bias to our estimates. To minimize this problem, we restrict our sample to rapid onset emergencies (449 observations). Rapid onset emergencies usually last less than one day, so that the direct effect of aid on our disaster impact variables should be negligible. Finally, to control for potential differences in the measurement of impact, we use disaster type dummies (flood, windstorm, fire etc.) in all of our specifications.

Socioeconomic Background

The socioeconomic indicators included are GDP per capita, population (in logs), and population density. While higher per capita income reduces the risk to be affected by natural disasters ex-ante, it is likely to be the most important measure for the degree to which exposed countries can cope with the damage inflicted by natural disasters[15]. In general, as highlighted by the UNDP's report (United Nations, 2004), low levels of development of an economy can amplify the risk that a natural event translates into a disaster, as well as the extent of the severity of the losses incurred.

Larger countries are ex-ante more likely to have disasters, but should generally also be more able to deal with a shock of a given size. More densely populated areas may be prone to suffer more in the aftermath of natural disasters' as evacuation possibilities can be limited and the risk of infectious diseases may be higher. On the other hand, densely populated areas may have better local networks and thus be able to recover more easily after natural hazards.

Policy Performance Variables

To account for structural differences in recipients' ability to cope with natural disasters, we include a set of basic policy variables in our empirical specifications. Poor policy settings may increase local's population need for foreign assistance, but at the same time lower the effectiveness of financial flows and thus the potential to help from a foreign perspective.

The main policy performance indicators we use in our analysis are the Freedom House Index (Freedom House, 2007), trade openness (imports plus exports over GDP) and ethnic fractionalization (Fearon and Laitin, 2003). The Freedom House Index assigns an annual score for civil and political freedom on a scale from 1 (most free) to 7 (least free) to each country. We add both scores to get an overall democracy index.[16] High Freedom House scores are generally associated with "good" institutions such as property rights, individual liberties, free information flows and low corruption. Such institutions may increase the potential of affected countries to deal with disasters themselves, but may also facilitate and encourage the provision of foreign emergency aid.

[15] The 2001 earthquakes in El Salvador and Seattle in the United States resulted in losses of around US$ 2 billion each. While these losses were easily absorbed by the U.S. economy, they represented 15 percent of El Salvador's GDP for that year (United Nations, 2004).

[16] The "political rights" index assesses the right to vote, election meaningfulness, multiple political parties, opposition power, and government independence from foreign or military control. The "civil liberties" index covers the freedoms of speech, assembly, and religion and freedom from terrorism or discrimination.

Higher fractionalization is generally associated with higher risk of internal conflicts, lower provision of public goods and higher inequality levels (Easterly and Levine, 1997; Garcia-Montalvo and Reynal-Querol, 2005). Fractionalization likely decreases the population's capacity to deal with external shocks especially in the case of minority groups, but also limits the degree to which foreign aid can reach its targets.

We also control for trade openness in our specifications, since open countries are generally more integrated into the international financial markets, and should thus be more able to smooth negative shocks relative to less open countries. On the other hand, open economies may have the better infrastructure for foreign aid transfers, making emergency aid particularly efficient in open economies.

Bilateral Relations and Strategic factors:

We define bilateral relations as broadly as possible to test the degree to which economic, historical and political ties shape the allocation of aid after natural disasters. The two most frequently used bilateral measures are distance and prior colonial status. The geographical distance variable we use measures the distance between the capital of the donor and the capital of the recipient (Gleditsch and Ward, 2001). Although distance is commonly used as a proxy for bilateral trade, distance may also capture the relative cost of providing help, especially if aid is provided in kind as it is often the case after natural disasters.

We also add a control for oil exporting countries to capture the potential strategic relevance of recipients, and Gartzke's affinity index measuring bilateral political alignment as the correlation of historical voting patterns in the United Nations General Assembly (Gartzke, 2002). A value of 1 of this index implies that the donor and the recipient always voted the same way, while a value of -1 implies that the two countries never agreed. Both of these measures are intended to capture donors' strategic and political objectives in the aid process.

Other Controls

To control for the total number of disasters in a given year and other exogenous shocks to donor's budget constraints, we include year fixed effects in all of our specifications. In the panel regressions, we also allow for donor fixed effects to control for differences in the average likelihoods of giving.

To limit concerns regarding potential feedback effects from aid to the explanatory variables, we use one year lags of all time-varying recipient specific (SE, PP) and bilateral (BI) variables in all of our empirical specifications.

5 Empirical Results

5.1 Part I: Panel Estimation

Given that emergency aid is by definition left censored at zero the equation to be estimated can be stated as

$$aid_{ij} = \max(0, \mathbf{x}_{ij}\beta + u_{ij}), \tag{1}$$

where aid_{ij} is the amount of aid donor i provides for disaster j, $\mathbf{x}_{ij}$ is the vector of explanatory variables and $u_{ij} \mid \mathbf{x}_{ij} \sim N(0, \sigma)$. Building on the independent variable groupings discussed in the previous section, we can state the model to be estimated as

$$\begin{aligned} aid_{ij} &= \max(0, \alpha + \beta\mathbf{DI}_j + \delta\mathbf{SE}_j + \lambda\mathbf{PP}_j + \vartheta\mathbf{BI}_{ij} + \gamma\mathbf{\Gamma}_{ij} + u_{ij}) \\ i &= 1, ..., 20 \\ j &= 1, ..., 449 \end{aligned}$$

where i refers to the donor[17] and j to the emergency. **DI** are the disaster impact measures, and **SE** and **PP** are the socioeconomic and policy performance indicators of the country affected by disaster, **BI** is our vector of donor-recipient bilateral controls, and $\mathbf{\Gamma}$ is a vector containing donor, year, disaster type and regional fixed effects.

We start our analysis with the participation (selection) equation. Empirically, this involves estimating a binary response model, where the dependent variable is the probability p that donor i provides positive amounts of aid ($give_{ij} = 1$) in response to disaster j, which can be stated as

$$p(\mathbf{x}) \equiv P(give_{ij} = 1 \mid \mathbf{x}) = P(\mathbf{x}_{ij}\beta + u_{ij} > 0 \mid \mathbf{x}). \tag{2}$$

[17] For computational purposes we restrict the analysis to the sample of 20 OECD donors consisting of Austria, Australia, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Japan, Netherlands, Norway, Portugal, Spain, Sweden, Switzerland, UK and the US.

Having estimated the initial selection process, we estimate the actual amounts of aid given in a Tobit model in a second step. While the Tobit estimates are likely to suffer from measurement error in the recorded aid amounts, estimating the aid allocation allows us to determine the actual magnitude of the detected effects under the assumption that the factors driving the probability of giving are identical to the factors driving the actual amount given (Wooldridge, 2002)[18].

The results from the Probit estimation of the selection equation are summarized in Table 3 below. In the first specification (Column1), we pool all donors and include year, regional and disaster type dummies only. In the second column, we add donor fixed effects, which appear highly significant, reflecting the pronounced differences in participation frequencies[19]. Estimating the Probit model in the pooled sample corresponds to treating the full data set as cross-section, thus assuming independence across observations, i.e. that there is no correlation between donors' actions for a given emergency. As this assumption is likely violated in the presence of unobservable disaster specific effects, the estimates from the pooled Probit are consistent but not efficient.

To deal with this problem we fit a Random Effect Probit Model (column 3). The RE Probit model allows to control for unobservable omitted factors specific to each emergency. Disaster specific unobservable effects may include media coverage, physical damage and similar unobservable shocks. The key underlying assumption for the RE estimator to be consistent is the independence of the unobservable effects from the full set of regressors, an assumption which is not necessarily satisfied in our framework[20]. To deal with potential correlations of unobservable effects with the included covariates we apply a conditional Logit model in columns 4 of the table. The functional assumptions underlying the conditional maximum likelihood Logit model

[18] An alternative to the Tobit would be an Heckman selection model. In the Heckman model, the inverse Mill's ratio estimated in a first stage Probit is used to correct for selection in the allocation equation (see Berthélemy and Tichit (2004) for a detailed discussion). In the absence of obvious exclusion restrictions (factors that matter in the selection, but not in the allocation equation) we opt for a Tobit specification of the allocation equation.

[19] We test the null of zero coefficients of all donor fixed effects and reject it at the 99% level. The Wald test for all coefficients equal to zero reports a chi-square statistic of 308.35.

[20] For example, if one assumes that the main unobservable is emergencies' media coverage, it is easy to imagine some positive correlation between the unobservable effect and our disaster impact measures.

allow consistent estimation independent of the distribution of unobserved effects (Wooldridge, 2002). The results are highly consistent with the random effect model, implying that the correlation between unobservable effects and our main covariates seems to be of rather minor importance. As further robustness check, we also estimate a conditional Logit model with emergency specific fixed effects. This specification allows us to perfectly control for emergency specific unobservables, but restricts the estimates to bilateral factors and to those disasters where at least one donor provides aid and at least one donor does not. Since the incidental parameter problem may potentially bias the maximum likelihood Probit estimates, we also estimate the same set of models with ordinary least squares model - the results of the OLS estimation are shown in Appendix A.3 and are nearly identical to the maximum likelihood estimates.

Overall, the results emerging from the panel analysis are highly consistent across estimators and specifications. As expected, both the number of people affected and the number of people killed have a positive and highly significant effect on the aid decision. A one standard deviation increase in the number of people affected (22.9 Million people, 2.6 in logs) increases the likelihood of receiving aid by 10-13 percentage points[21]. The effect of the number of people killed is about twice as large: a one standard deviation increase in the number of people killed (3054 people in levels, 1.9 in logs) increases the likelihood to receive emergency aid by around 20 percentage points.

While density does not seem to play a major role in the aid decision, population size and GDP per capita show the expected negative sign. Larger and richer countries can cope with natural disasters more easily, and are thus less dependent on foreign assistance. In line with our mostly ambiguous priors, none of the policy variables appears to have a significant effect on the final aid allocation.

Most remarkable are the estimated coefficients on geographical distance, oil, former colony status and political affinity. Our point estimates imply that each 1000 km of distance between donor and recipient reduces the likelihood to receive aid by 1-2 percentage points, a magnitude likely too

[21]Note that Table 1 shows Probit and Logit coefficient estimates. To get the marginal effects at the mean of the independent variables coefficients need to be roughly divided by a factor of 2.5 (Probit) and 4 (Logit), respectively.

big to be explained by purely logistical issues. Also, former colonies and oil exporters appear to get significant preferential treatment in the international aid process; on average, being an oil exporter increases the likelihood to receive aid by 10-15 percentage points, while former colonies are 25-30 percentage points more likely to receive aid after natural disasters.

As opposed to previous results in the ODA literature (Alesina and Dollar, 2000), we find that donors are more likely to provide emergency aid to countries traditionally not aligned in their voting patterns. For the average donor, a one standard deviation decrease in the affinity index (0.25) increases the average likelihood to receive aid by 10-12 percentage points. Donors seem to use emergency aid to improve weak diplomatic relations rather than to reward countries with traditionally aligned political interests. If the acquisition of international consensus is on donors' political agenda, emergency aid may well be a more visible, cheaper and more flexible tool to reach such a consensus than traditional development assistance. Emergency donations are significantly smaller in size than typical ODA transfers, and are typically delivered directly by donors' officials providing increased visibility to the donor. The behavior of the US and Australia in the aftermath of the December 2004 Tsunami towards Indonesia is a good examples for such behavior. Indonesia traditionally appears as not aligned to the US voting patterns in the UN General Assembly, with a deteriorating trend in affinity since the 1999 crisis in East Timor strongly condemned by the Clinton administration. Similarly, diplomatic relations with Australia have been very complicated in recent years. Despite this, both Australia and the US provided particularly generous support to Indonesia in the aftermath of the Tsunami. A related statement by the US Secretary of State Colin Powell nicely illustrates the underlying logic: "We'd be doing this regardless of religion, [...] but I think it does give the Muslim world an opportunity to see American generosity, American value in action [...] And I hope that, as a result of our helicopter pilots being seen by the citizens of Indonesia helping them, that value system of ours will be reinforced" (The Economist, 2005).

[**Table 3: Panel: Probit Analysis**]

Table 4 below shows the results for the Tobit estimates. The results are nearly identical with respect to sign and significance of the explanatory variables. A 10 percent increase in the number of agents killed increases the total amount of aid received by about 25 percent, while a 10 percent increase in population has exactly the opposite effect. More importantly, the Tobit estimates strongly underline the relative importance of bilateral factors. Every 1000 kilometers of distance between capitals decreases aid by around 50%. The effects of affinity and colonial origin are even larger. A one standard deviation in affinity (0.25) increases aid by a factor or 50, and the effect of being a former colony is still larger. Even when the marginal effects are calculated conditional on the non-censored range, these effects remain surprisingly large; conditional on non-censored outcomes, the marginal effects of affinity and colonial status are -3.9 and 1.79 respectively, which implies that a one standard deviation decrease in affinity raises aid by about 200 percent, while being a former colony implies aid flows about five times as big as observed for comparable disasters.

[**Table 4 : Panel: Tobit Analysis**]

5.2 Part II: Individual donor analysis

In the previous section, we implicitly assumed that the factors driving bilateral aid decisions were the same across donors. In this section, we determine the factors driving aid for each donor separately, and directly test the restrictions imposed in the panel analysis presented before.

For expositional convenience, we limit our analysis to the five major donors in our sample - the US, Japan, Germany, the UK and Norway, which alone represent more than the 40% of total humanitarian aid - and confront their aid patterns to those of private donors. With scarce disaggregate data on private donations, total non-governmental donations is the only proxy for "private" donations generally available in the FTS data. While this variable is a useful benchmark for the country specific results, its aggregate nature makes the interpretation of the estimated coefficients rather difficult.

Table 5 below reports the coefficients for the Probit models estimated for each donor and, in the last column, the Wald test for the equality of coefficients across them. All donors are more likely to intervene in emergencies characterized by a higher death toll and a larger number of people affected, although these effects are only partially significant for Japan and private donors.

With respect to our socioeconomic controls, all donors are more likely to intervene in favor of less populated potential recipients, even though this effect is not significantly different from zero for the US. This effect is similar to what is found in the aid literature (Berthélemy and Tichit, 2004), and, as discussed in the previous section likely reflects donors' evaluations of the recipient's capacity to deal with the disaster.[22] While Japan seems marginally more likely to provide aid to more densely populated countries, the opposite is true for Norway. Only private donors and the UK are more likely to help poorer countries.

The positive and highly significant coefficient on the oil indicator found in the panel regression applies only to the US the UK and Norway, who are 24, 35 and 39 percentage points more likely to help oil exporting than other countries, respectively.

Among the five major donors analyzed, Norway is the one showing the highest responsiveness to policy performance indicators. In particular, one standard deviation change in trade openness, as measured by total trade value over GDP, reduces Norway's likelihood to provide aid by 14 percentage points. The same negative response is displayed by private donors (16% decrease). Norway also appears to be hesitant to donate to ethnically fractionalized countries. Moving from the least to the most ethnically fractionalized background reduces the likelihood to receive aid from Norway's by a remarkable 38 percentage points[23].

[**Table 5: Individual Donors: Probit Analysis**]

As to the Gastil index, the US are more likely to help more free and democratic countries whereas for Norway the opposite holds. In particular a 3 point increase in the freedom index (i.e. if the recipient is three points less "free") decreases the US giving probability by 10%, while increases Norway's one by 14 percentage points.

Similarly diverging patterns emerge from donors' response to (bilateral) geographical distance. Germany is 66 percentage points more likely to give to the closest recipient with respect to the most remote one, whereas the US are 70 percentage points more likely to give to the most distant as

[22] Trumbull and Wall (1994) argue that smaller populations also imply a higher per capita impact of aid.

[23] The ethical fractionalization index (ELF) ranges from 0 (least fractionalized) to 1 (most fractionalized). See Appendix Table A.2.

compared to the least distant recipient. However, the interpretation of these coefficients is to be taken with caution as all the specifications contain region fixed effects. On the other hand, all donors are more likely to give to less aligned countries, confirming previous results from the panel analysis. The test for equal coefficients among donors cannot be rejected. In Figure 4 we plot the predicted probability of each donor's giving against the respective values for the bilateral affinity index[24]. The variation in bilateral affinity index varies considerably between donors. When computing differences in fully standardized coefficients[25], it turns out that one standard deviation increase in affinity index lowers the probability of providing aid by 0.15 standard deviations for the US and by 0.36 standard deviations for Norway.

[**Figure 4: Donors' Responsiveness and Bilateral Affinity**]

Last, formal colonial ties increase the UK's intervention probability by 29 percentage points.

The patterns emerging from the aid allocation (Tobit) estimation are nearly identical as shown in Table 6 below. All donors respond strongly to the humanitarian need generated by emergencies, even though the estimated coefficients on the death toll vary significantly across donors. As discussed before in the panel regressions, the magnitude of bilateral considerations is considerable.

[**Table 6: Individual Donors: Tobit Analysis**]

5.3 Part III: Strategic Interaction

The last question we address in this paper is the interaction between donors in the international aid process. The literature on ODA allocation has treated other donors' actions as exogenous, finding mixed results on the direction of such interactions (Tarp et. al., 1999, Berthélemy and Tichit, 2004, Round and Odedokun, 2004). While it is conceivable that governments may be exposed to international "peer pressure" or may want to

[24] All other controls are kept to their mean value. Summary statistics in the Appendix (TableA2).

[25] $\beta_k^S = \left(\frac{\sigma_k \beta_k}{\sigma_k} \right)$

profit from economies of scale in the provision of aid, donor governments may also view other donors' donations as substitutes for their own aid and thus reduce their contributions with increasing aid from others.

[**Figure 5: Donor Participation Patterns**]

In our analysis we focus on the interactions between the most active donors analyzed in the previous section: the US, Japan, Germany, the UK and Norway. These five donors are not only the most active ones, but also fairly good predictors of the international aid response as summarized in Figure 5. The average number of other OECD donors responding to each emergency increases from 0.47 when none of the major donors intervenes to 7.07 when all of them respond. The main advantage of focusing only on the five principal donors is that we can build on the results presented in the previous section and use the bilateral variables relevant for each individual donor as instruments in a Two-Stage Least Squares (2SLS) setup.

The results of the 2SLS estimation are summarized in Table 7 below. In addition to the full set of covariates used in the previous section, we now include the number of other main donors providing aid for a given emergency, which we instrument with bilateral distance in the first stage regressions. We test the validity of our instruments with the Sargan/Hansen overidentification test; p-values between 0.14 (Germany) and 0.98 (UK) imply that the null of instrument validity cannot be rejected. Given the large set of controls included in our specification, the predictive power of our instruments is limited[26]. As shown at the bottom of Table 7, the Cragg-Donald F-statistics ranges between 4.56 (Germany) and 7.21 (US); as a result, our estimates are likely to display some of the upward bias expected for basic OLS estimates in our setup. A Cragg-Donald statistic of 6.4 implies a maximum relative IV bias of 20 percent in our setup (Stock and Yogo, 2005). Even though this implies that our point estimates are likely to be upward biased at the margin, our results provide evidence for positive and highly significant interaction among donors. Our point estimates imply that the likelihood to provide aid for a given disaster increases between 19.2 (US) and 33.6 (Germany) percentage points with each other major donor committing to provide help for a given disaster.

[26] For full first stage results, see Table A.4 in the Appendix.

[**Table 7: Donor Interaction. IV Estimates**]

6 Summary and Conclusions

In this paper we have used a sample of over four hundred recent natural disasters to systematically evaluate the degree to which humanitarian need is reflected in international humanitarian aid flows. We have shown that donor governments are on average significantly more generous towards geographically closer, politically less affine and oil exporting countries. We also find significant biases in favor of former colonies, and evidence for herding in the international aid process. While the extent of the various biases varies significantly across countries, the correlation between the current allocation of aid and the actual humanitarian losses associated with natural disasters is surprisingly low. While we have presented some evidence on private donations in this paper, data limitations have prevented us from going further into the details of private aid and its determinants. Given the growing role of the private sector in the humanitarian field, more studies on the interaction of private contributions in general, and the interaction of private preferences with domestic media in particular, appear desirable.

From a policy perspective, our findings do not necessarily imply that government agencies behave suboptimally. Even though the aid patterns detected in this paper stand in stark contrast to the official international commitment to a purely humanitarian use of emergency aid, discretionary choice in the allocation of aid may well reflect the preferences or interests of underlying populations and electorates. Nevertheless, recent developments in the international political sphere indicate that at least some countries have recognized the need for improvements in the allocation of humanitarian aid. In a first meeting in 2003, sixteen of the major donors joined forces in the Good Humanitarian Donorship Initiative working towards more efficiency and higher degrees of accountability within humanitarian assistance. In a related effort, UN Secretary-General Kofi Annan officially launched the Central Emergency Response Fund (CERF) as central tool to provide immediate and impartial humanitarian aid to regions experiencing humanitarian crisis in March of 2006. Both initiatives appear steps into the right direction from a humanitarian policy perspective.

References

[1] Abramovitz, J. , 2001. Unnatural Disasters. Worldwatch Institute Paper 158.

[2] Alesina, A., Dollar, D., 2000. Who Gives Foreign Aid to Whom and Why? Journal of Economic Growth 5, 33–63.

[3] Berthélemy, J.-C., Tichit, A., 2004. Bilateral Donors' Aid Allocation Decisions - a Three Dimensional Panel Analysis. International Review of Economics and Finance 13, 253–274.

[4] Darcy, J., Hofmann, C.A., 2003. According to Need? Needs Assessment and Decision-Making in the Humanitarian sector. HPG Report nr.15, London.

[5] Easterly, W., Levine, R., 1997. Africa's Growth Tragedy: Policies and Ethnic Divsions. The Quarterly Journal of Economics 112(4), 1203–1250.

[6] Eisensee, T., Stromberg, D., 2007. News Floods, News Droughts, and U.S. Disaster Relief. Forthcoming: Quarterly Journal of Economics.

[7] Fearon, J.D., Laitin, D.D., 2003. Ethnicity, Insurgency and Civil War. American Political Science Review 97(1), 75–90.

[8] Freedom House, 2007. Freedom in the World 2006. http://www.freedomhouse.org/.

[9] García-Montalvo, J., Reynal-Querol, M., 2005. Fractionalization, Polarization and Economic Development. Journal of Development Economics, 76, 293-323.

[10] Gartzke,E., Dong-Joon, J., 2002. The Affinity of Nations Index, 1946-1996. http://www.columbia.edu/~eg589/datasets.html.

[11] Gleditsch, K. S., Ward, M. D., 2001. "Measuring Space: A Minimum Distance Database." Journal of Peace Research 38:749-68.

[12] Guha-Sapir, D., Hargitt, D., Hoyois, P., 2004. Thirty Years of Natural Disasters 1974-2003: the Numbers. UCL, Presse Universitaires de Louvain.

[13] IASC, 1994.Consolidated Appeal Process Guidelines. Inter-Agency Standing Committee.

[14] IFRC, 2003. World Disasters Report 2003. International Federation of Red Cross and Red Crescent Societies, Geneva.

[15] Macrae, J. (ed.), 2002. The New Humanitarianism: A Review of Trends in Global Humanitarian Action. HPG Report nr.11. London: ODI.

[16] McKinlay, R.D., Little, R., 1977. A Foreign Policy Model of U.S. Bilateral Aid Allocation. World Politics, Vol. 30(1), 58-86.

[17] Neumayer, E., 2003. Do Human Rights Matter in Bilateral Aid Allocation? A Quantitative Analysis of 21 Donor Countries. Social Science Quarterly, vol. 84(3), 650-66.

[18] Neumayer, E., 2003a. The Determinants of Aid Allocation by Regional Multilateral Development Banks and United Nations Agencies. International Studies Quarterly, 47 (1), 101-122.

[19] OECD, 2007. International Development Statistics (IDS) Online. http://www.oecd.org/dac/stats/idsonline.

[20] Olsen, G.R., Carstensen, N., Høyen, K., 2003. Humanitarian Crises: What Determines the Level of Emergency Assistance? Media Coverage, Donor interests and the Aid Business. Disasters, vol.27, no.2.

[21] Randel, J., German, T. 2002. Trends in the financing of humanitarian assistance. in J. Macrae (ed.), 2002. The new Humanitarianism: a review of trends in global humanitarian action. HPG Report nr.11. London: ODI.

[22] Round, J., Odedokun, M., 2004. Aid Effort and its Determinants. International Review of Economics and Finance 13, 293–309.

[23] Stock, J. H. and M. Yogo (2005). Testing for weak instruments in linear IV regression. Identification and Inference for Econometric Models: Essays in Honor of Thomas Rothenberg. D. W. K. A. a. J. H. Stock. Cambridge, Cambridge University Press.

[24] Tarp, F., Bach, C. F., Hansen, H., Baunsgaard, S., 1999. Danish Aid Policy: Theory and Empirical Evidence, in Gupta, K. (ed.): Foreign Aid: New Perspectives. Norwell MA, 149-69.

[25] The Economist. 2005. More generous than thou. http://www.economist.com. January 8th, 2005.

[26] United Nations, 1991. General Assembly Resolution A/RES/46/182, http://www.un.org/documents/ga/res/46/a46r182.htm.

[27] United Nations, 2004. Reducing disaster risk: a challenge for development. UNDP report.

[28] Walker, P., Wisner, B., Leaning, J., Minear, L., 2005. Smoke and Mirrors: Deficiencies in Disaster Funding. BMJ 2005. 330: 247-50.

[29] Wooldridge, J., 2002. Econometric Analysis of Cross Section and Panel Data, MIT Press, Cambridge, MA.

[30] World Bank (2006). World Bank Development Indicators 2006 CD-ROM.

Figure 1: FTS and DAC Data on Emergency Aid

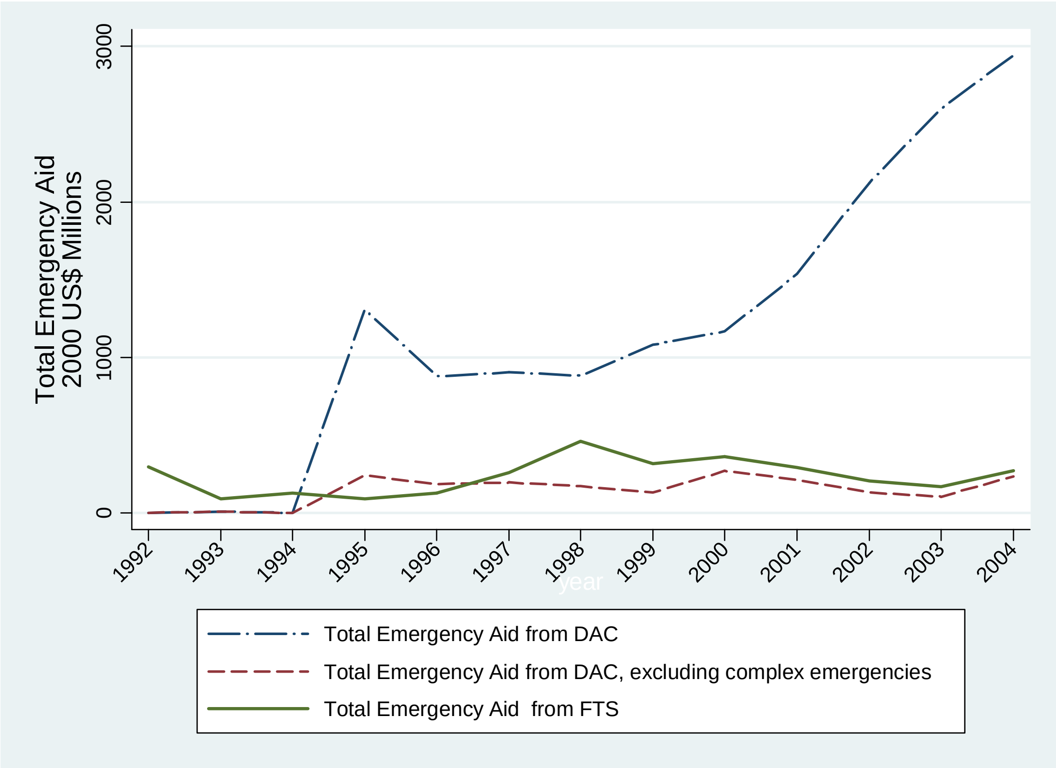

Figure 2: Major Donor Countries and Institutions

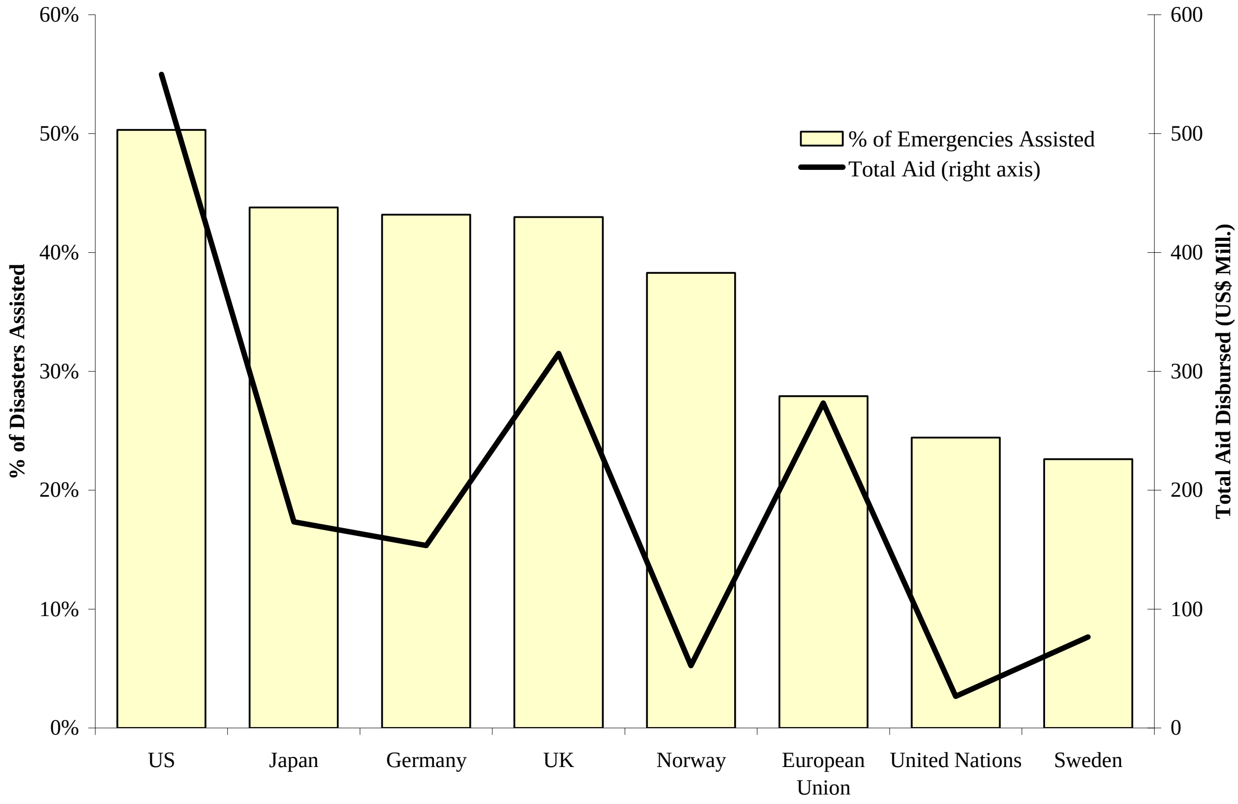

Figure 3: Total Emergency Aid by Year and Donor

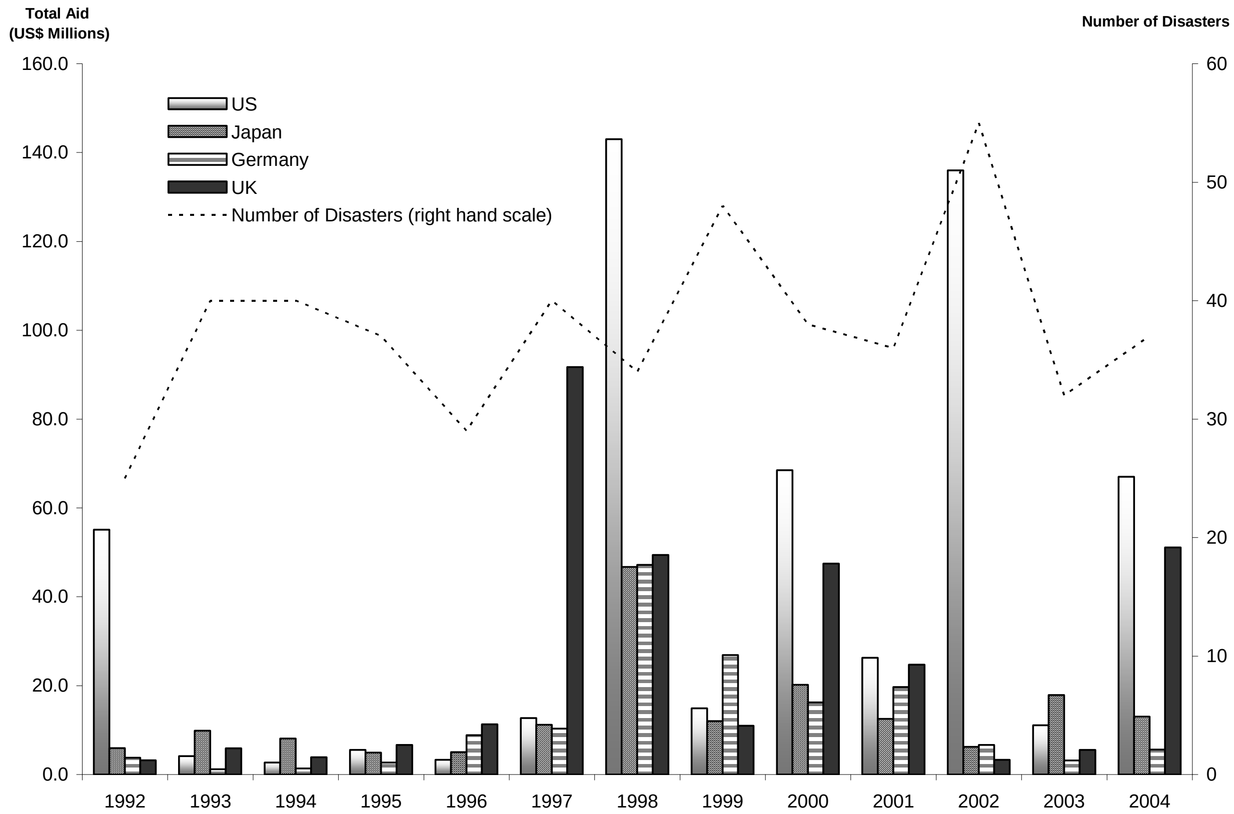

Figure 4: Donors' Responsiveness and Bilateral Affinity

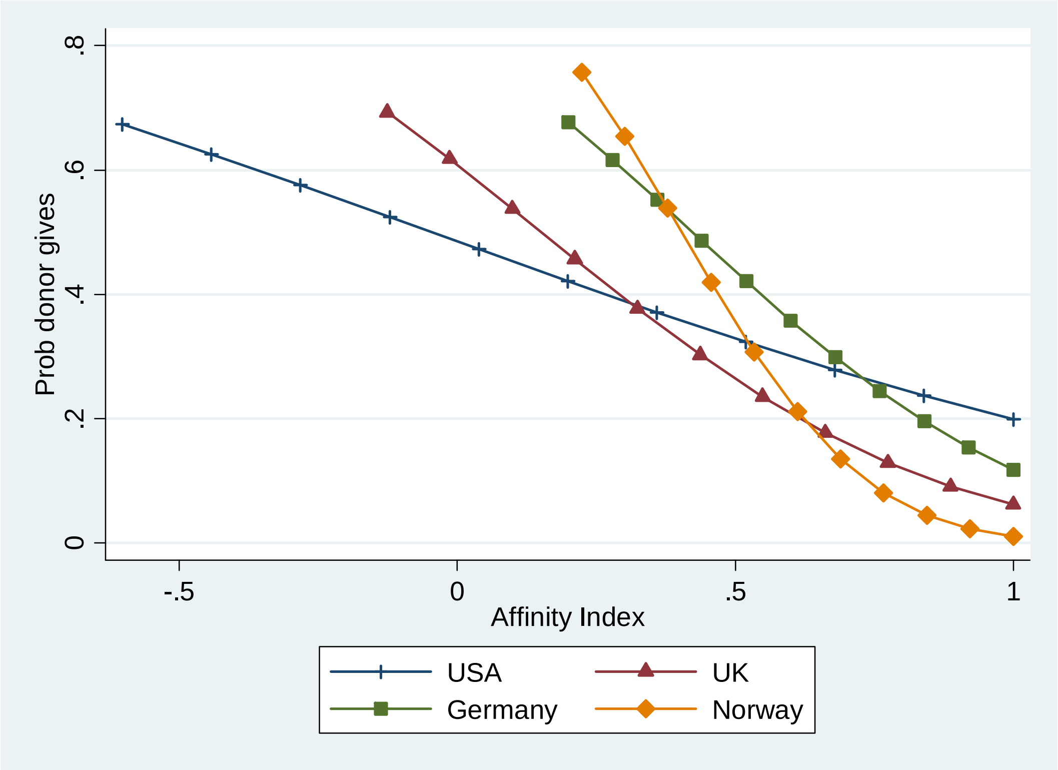

Figure 5: Donor Participation Patterns

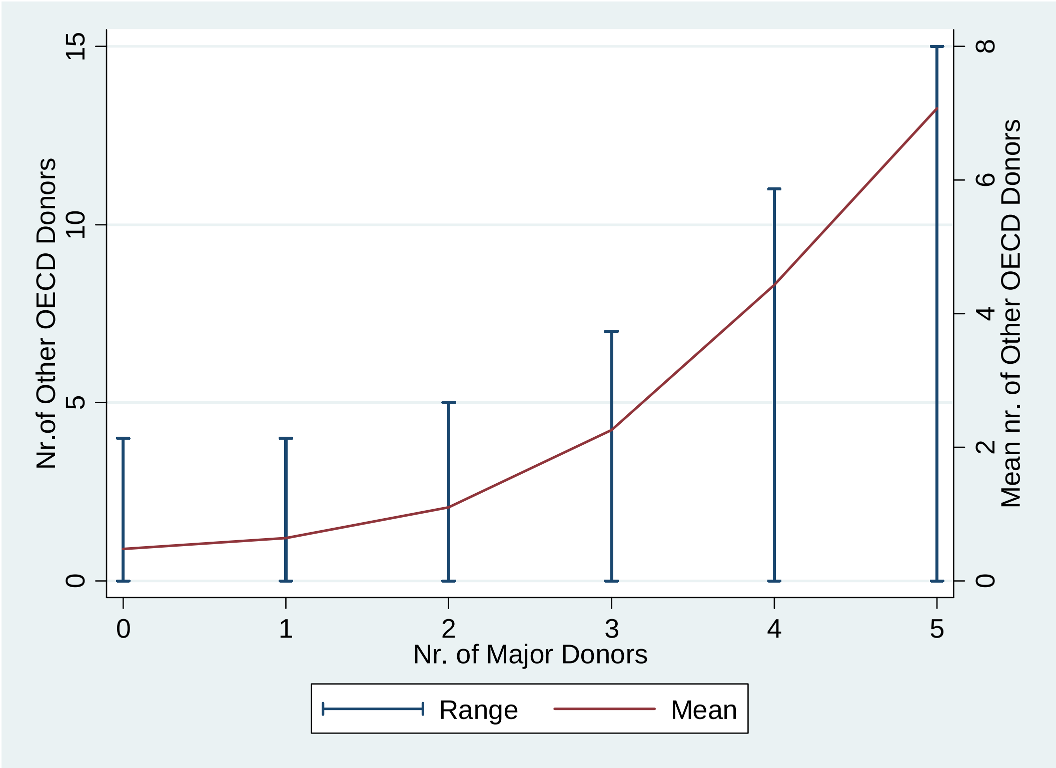

Table 1a: Correlations of Aid Interventions Among Major Donors

	US	Japan	Germany	UK	Norway
US	1.00				
Japan	0.31	1.00			
Germany	0.27	0.22	1.00		
UK	0.25	0.43	0.32	1.00	
Norway	0.26	0.27	0.23	0.29	1.00

Table 1b: Correlations of Aid Amounts Among Major Donors

	US	Japan	Germany	UK	Norway
US	1.00				
Japan	0.53	1.00			
Germany	0.52	0.59	1.00		
UK	0.29	0.36	0.29	1.00	
Norway	0.18	0.53	0.35	0.25	1.00

Table 2: Impact by Disaster Type

	Frequency	Average Number People killed	Average Number People Affected
Slow Onset Disasters			
Cold or Heat Waves	2	120	218,734
Drought	28	155	16,600,000
Epidemic	2	34	24,801
Wild Fires	9	12	34,083
Rapid Onset Disasters			
Earthquake	76	1,441	310,855
Flood	243	376	5,477,815
Slides	14	266	114,920
Volcano	14	44	38,557
Wind Storm	102	289	512,410

Table 3. Panel: Probit Analysis

Dependent variable: Pr(Donor *j* provides aid after disaster *i* =1)					
	(1) Pooled Probit	(2) Pooled Probit Donor FE	(3) RE-Probit Donor FE	(4) Conditional Logit Donor FE	(5) Conditional Logit Emergency FE
Impact measures					
Log(Nr.affected)	0.099***	0.116***	0.116***	0.215***	
	(0.014)	(0.015)	(0.013)	(0.024)	
Log(Nr. Killed)	0.199***	0.233***	0.233***	0.417***	
	(0.015)	(0.016)	(0.016)	(0.028)	
Socio economic indicators:					
Log(Population)	-0.204***	-0.221***	-0.221***	-0.423***	
	(0.026)	(0.030)	(0.029)	(0.052)	
Pop. density	0.000	-0.000	-0.000	0.000	
	(0.000)	(0.000)	(0.000)	(0.001)	
Log (GDP per capita)	-0.146***	-0.175***	-0.175***	-0.291***	
	(0.043)	(0.045)	(0.046)	(0.081)	
Oil dummy	0.324***	0.391***	0.391***	0.661***	
	(0.068)	(0.073)	(0.073)	(0.129)	
Policy performance indicators					
Trade openness	-0.001	-0.001	-0.001	-0.003	
	(0.001)	(0.002)	(0.002)	(0.003)	
ELF index	-0.044	-0.007	-0.007	0.003	
	(0.112)	(0.120)	(0.126)	(0.217)	
Gastil index	-0.000	-0.000	-0.000	-0.001	
	(0.010)	(0.011)	(0.011)	(0.019)	
Bilateral relation indicators					
Geograph. Distance	-0.018*	-0.036***	-0.036***	-0.056***	-0.086***
	(0.009)	(0.009)	(0.009)	(0.015)	(0.017)
Affinity index	-1.414***	-1.299***	-1.299***	-2.377***	-2.749***
	(0.101)	(0.259)	(0.263)	(0.469)	(0.820)
Former colony	0.519***	0.632***	0.632***	1.067***	1.354***
	(0.096)	(0.112)	(0.112)	(0.192)	(0.216)
Donor FE	NO	YES	YES	YES	YES
Disaster FE	NO	NO	NO	NO	YES
Observations	5153	5153	5153	5153	4754
Pseudo R-squared	0.16	0.28		0.18	0.31
Wald/LR Statistic	Wald chi2(38) 739.2	Wald chi2(57) 1157.2	Wald chi2(57) 1123.3	LR chi2(38) 903.8	LR chi2(22) 1117.9

Notes:
Coefficients reported; robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
All specifications include year, regional and disaster type fixed effects.
The donors included are.Austria, Australia, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Japan, Netherlands, Norway, Portugal, Spain, Sweden, Switzerland, UK and the US

Table 4: Panel: Tobit Analysis

Dependent variable: log(1+aid) donor *j* provides aid after disaster *i*

	(1) Pooled-Tobit	(2) Pooled-Tobit FE	(3) RE-Tobit DOFE	(4) Tobit EMFE
Impact measures				
Log(Nr.affected)	1.269***	1.271***	1.271***	
	(0.153)	(0.140)	(0.140)	
Log(Nr. Killed)	2.497***	2.483***	2.483***	
	(0.179)	(0.163)	(0.163)	
Socio economic indicators:				
Log(Population)	-2.488***	-2.327***	-2.327***	
	(0.316)	(0.308)	(0.308)	
Pop. density	-0.000	-0.000	-0.000	
	(0.004)	(0.004)	(0.004)	
Log (GDP per capita)	-1.982***	-1.994***	-1.994***	
	(0.532)	(0.488)	(0.488)	
Oil dummy	4.023***	4.136***	4.136***	
	(0.850)	(0.777)	(0.777)	
Policy performance indicators				
Trade openness	-0.015	-0.013	-0.013	
	(0.017)	(0.016)	(0.016)	
ELF index	-0.423	0.077	0.077	
	(1.427)	(1.323)	(1.323)	
Gastil index	0.034	0.035	0.035	
	(0.123)	(0.115)	(0.115)	
Bilateral relation indicators				
Geograph. Distance				
	-0.235**	-0.384***	-0.384***	-0.451***
Affinity index	(0.099)	(0.089)	(0.089)	(0.078)
	-16.636***	-13.586***	-13.586***	-12.952***
Former colony	(1.152)	(2.729)	(2.729)	(3.579)
	6.600***	7.077***	7.077***	7.089***
	(1.180)	(1.172)	(1.172)	(0.983)
Donor FE	NO	YES	YES	YES
Disaster FE	NO	NO	NO	NO
Observations	5153	5153	5153	5153
Pseudo R-squared	0.07	0.12	0.12	0.20

Notes:
Coefficients reported; standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
All specifications include year, regional and disaster type fixed effects.
The donors included are.Austria, Australia, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Japan, Netherlands, Norway, Portugal, Spain, Sweden, Switzerland, UK and the US

Table 5: Individual Donors: Probit Analysis

Dependent variable: Prob(donor gives=1)							
	US[(A)]	**Japan**[(A)]	**Germany**[(A)]	**UK**[(A)]	**Norway**[(A)]	**Private**[(A)]	**Test for equality of coefficients**[(B)]
Impact measures:							
Log(Nr.affected)	0.121***	0.040	0.112**	0.186**	0.181***	0.143***	6.99
	(0.049)	(0.049)	(0.053)	(0.056)	(0.054)	(0.049)	(0.221)
Log(Nr. Killed)	0.190***	0.307***	0.312***	0.307***	0.208***	0.020	18.89
	(0.063)	(0.067)	(0.069)	(0.069)	(0.067)	(0.060)	(0.002)
Socio economic indicators:							
Log(Population)	-0.115	-0.313**	-0.236**	-0.514***	-0.589***	-0.231**	13.67
	(0.107)	(0.123)	(0.120)	(0.125)	(0.134)	(0.097)	(0.018)
Pop. density	0.001	-0.003**	-0.000	-0.001	0.002*	0.000	11.02
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.051)
Log (GDP per capita)	-0.184	-0.052	-0.175	-0.340*	-0.226	-0.489***	5.51
	(0.171)	(0.188)	(0.181)	(0.193)	(0.189)	(0.174)	(0.357)
Oil dummy	0.621**	-0.398	0.367	0.896***	1.046***	0.438	25.52
	(0.278)	(0.275)	(0.290)	(0.330)	(0.324)	(0.282)	(0.000)
Policy performance indicators:							
Trade openness	0.002	0.000	-0.002	-0.012	-0.014**	-0.014***	12.34
	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)	(0.005)	(0.031)
ELF index	-0.171	-0.030	-0.552	-0.050	-1.069**	0.283	5.48
	(0.486)	(0.510)	(0.495)	(0.562)	(0.526)	(0.435)	(0.360)
Gastil index	-0.083**	0.023	-0.017	0.009	0.112**	-0.005	12.44
	(0.041)	(0.045)	(0.045)	(0.048)	(0.046)	(0.039)	(0.029)
Bilateral relation indicators:							
Geogr. Distance	0.153**	0.016	-0.125*	0.103	0.097		9.33
	(0.077)	(0.108)	(0.064)	(0.095)	(0.108)		(0.053)
Affinity index	-0.809*	-1.653	-2.059*	-1.813**	-3.866**		7.43
	(0.584)	(1.312)	(1.272)	(1.070)	(1.473)		(0.115)
Former colony				0.746**			
				(0.341)			
Nr. Obs	270	269	270	270	270	270	
Pseudo R^2	0.182	0.228	0.260	0.385	0.304	0.251	
LR chi2	67.503	84.837	97.053	142.937	110.772	92.646	
p-value	(0.002)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	

Notes:
[(A)] Additional controls in all specifications: region, disaster type and year dummies. All models include a constant term. Robust standard errors in parentheses.
[(B)] Wald test of equal coefficients across donors. *p* -values in parentheses.
*significant at 10%; **significant at 5%; *** significant at 1%.

Table 6: Individual Donors: Tobit Analysis

Dependent variable: Log(donor aid+1)	US(A)	Japan(A)	Germany(A)	UK(A)	Norway(A)	Private(A)	Test for equality of coefficients(B)
Impact measures:							
Log(Nr.affected)	0.869***	0.330***	0.457***	1.274***	1.417***	0.632***	6.44
	(0.323)	(0.349)	(0.169)	(0.364)	(0.395)	(0.180)	(0.266)
Log(Nr. Killed)	1.498***	2.302***	1.187***	2.365***	1.394***	0.432**	34.91
	(0.391)	(0.437)	(0.209)	(0.437)	(0.463)	(0.220)	(0.000)
Socio economic indicators:							
Log(Population)	-0.795	-2.324***	-0.837**	-3.563***	-4.328***	-1.021***	19.73
	(0.711)	(0.863)	(0.400)	(0.829)	(0.975)	(0.339)	(0.001)
Pop. density	0.004	-0.023	-0.002	-0.006	0.020**	0.002	14.47
	(0.008)	(0.010)	(0.004)	(0.010)	(0.010)	(0.004)	(0.013)
Log (GDP per capita)	-1.412	-0.125	-0.858	-2.533**	-2.660*	-1.775***	5.00
	(1.146)	(1.332)	(0.617)	(1.273)	(1.385)	(0.596)	(0.416)
Oil dummy	4.242*	-3.237*	1.423	5.017**	8.209***	1.808*	25.71
	(1.833)	(2.000)	(0.997)	(2.109)	(2.409)	(1.033)	(0.000)
Policy performance indicators:							
Trade openness	0.010	0.001	-0.001	-0.076*	-0.107**	-0.052***	18.06
	(0.037)	(0.042)	(0.020)	(0.044)	(0.052)	(0.016)	(0.002)
ELF index	-1.778	0.372	-0.633	0.624	-9.084**	0.927	7.45
	(3.219)	3.533)	(1.699)	(3.460)	(3.906)	(1.609)	(0.189)
Gastil index	-0.530	0.234	-0.064	0.124	0.828**	0.027	10.63
	(0.277)	(0.314)	(0.152)	(0.306)	(0.340)	(0.141)	(0.059)
Bilateral relation indicators:							
Geogr. Distance	1.109**	-0.252	-0.287	0.475	0.890		7.70
	(0.525)	(0.779)	(0.211)	(0.586)	(0.798)		(0.103)
Affinity index	-6.217	-12.427*	-7.728*	-14.987**	-29.958***		10.56
	(3.982)	(10.393)	(4.157)	(7.067)	(11.059)		(0.032)
Former colony				5.712***			
				(2.161)			
Nr. Obs	270	269	270	270	270	270	
Log likelihood	-622.666	-592.350	-813.982	-502.114	-476.812	-832.104	
LR chi2	79.534	92.179	109.050	153.032	117.691	104.074	
p-value	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	

Notes:

(A) Additional controls in all specifications: region, disaster type and year dummies. All models include a constant term. Robust standard errors in parentheses.

(B) Wald test of equal coefficients across donors. *p* -values in parentheses.

*significant at 10%; **significant at 5%; *** significant at 1%.

Table 7: Donor Interaction: IV Estimates

Dependent variable: Pr(Donor *j* provides aid after disaster *i* =1)	(1) US	(2) Japan	(3) Germany	(4) UK	(5) Norway
Number of major donors	0.192**	0.155**	0.336***	0.331***	0.254***
	(0.077)	(0.068)	(0.102)	(0.074)	(0.069)
Impact measures					
Log(Nr.affected)	0.013	-0.014	-0.015	0.001	0.016
	(0.019)	(0.016)	(0.021)	(0.016)	(0.016)
Log(Nr. Killed)	-0.006	0.052**	-0.014	-0.014	-0.023
	(0.027)	(0.026)	(0.035)	(0.026)	(0.029)
Socio economic indicators:					
Log(Population)	0.046	-0.042	0.065	-0.023	-0.076*
	(0.041)	(0.039)	(0.051)	(0.036)	(0.041)
Pop. density	0.000	-0.001***	0.000	0.000	0.001*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Log (GDP per capita)	-0.003	0.037	0.008	-0.026	0.005
	(0.057)	(0.052)	(0.062)	(0.054)	(0.058)
Oil dummy	0.118	-0.273***	-0.080	0.043	0.149*
	(0.090)	(0.086)	(0.116)	(0.091)	(0.086)
Trade openness	0.002	0.001	0.000	-0.002	-0.003*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
ELF index	0.002	0.010	0.015	0.246	-0.231
	(0.145)	(0.146)	(0.171)	(0.160)	(0.166)
Gastil index	-0.036***	0.008	-0.011	-0.004	0.034**
	(0.013)	(0.011)	(0.014)	(0.011)	(0.014)
Bilateral Variables					
Geograph. Distance	0.032	0.006	-0.122***	0.009	-0.004
	(0.024)	(0.036)	(0.039)	(0.032)	(0.033)
Affinity index	-0.027	-0.091	0.079	-0.104	-0.359
	(0.166)	(0.432)	(0.394)	(0.226)	(0.429)
Former colony				0.043	
				(0.112)	
Observations	269	269	269	269	269
Centered R-squared	0.31	0.42	0.13	0.43	0.29
Hansen OID Test	0.14	0.39	0.42	0.98	0.36
Cragg-Donald F-Stat	7.21	8.20	4.56	5.47	5.66

Notes:
2SLS estimates. Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
All specifications include year, regional and disaster type fixed effects.

Table A.1: Description of Variables and Sources

Dependent variable:	
Aid	Total aid per emergency (2000 US$-PPP) SOURCE: *FTS – own calculations*
Impact measures:	
Nr. affected	People requiring immediate assistance during an emergency situation. SOURCE: *EM-DAT*
Nr. Killed	Persons confirmed dead and persons missing and presumed dead SOURCE: *EM-DAT*
Socio-economic indicators:	
Population	Population (one year lag) SOURCE: *WDI*
Population density	Nr. of people per square km (one year lag) SOURCE: *WDI*
GDP per cap	Per capita GDP (one year lag; 2000 US$-PPP) SOURCE: *WDI – own calculations*
Oil Dummy	Dummy = 1 if oil exports exceeds 1/3 of total exports SOURCE: *WDI – own calculations*
Policy performance indicators:	
Trade openness	(Import + Exports) / GDP (one year lag; 2000 US$-PPP) SOURCE: *WDI – own calculations*
ELF index	Ethno-linguistic fractionalization index. Range from 0 (least fract.) to 1 (more fract.) SOURCE: *Fearon and Laitin, 2003*
Gastil index	Democratization index. Sum of *political rights* and *civil liberties* indexes. Both indexes range between 1 (most free) and 7 (least free) SOURCE: *Freedom House*
Bilateral relations indicators:	
Affinity index	Affinity index in UNGA recipient - donor voting. Ranges from -1 (least similar) to 1(more similar). (one year lag) SOURCE: *Gartzke, 2002*
Geogr. distance	Donor – recipient's capital cities distance SOURCE: *Gleditsch and Ward, 2001*
Former colony	Dummy = 1 if recipient was donor's colony SOURCE: *Fearon and Laitin, 2003*

Table A.2: Summary Statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Give: USA	449	0.51	0.50	0	1
Give: Japan	449	0.46	0.50	0	1
Give: Germany	449	0.43	0.50	0	1
Give: UK	449	0.43	0.50	0	1
Give: Norway	449	0.37	0.48	0	1
Give: Private	449	0.57	0.50	0	1
(Log) Aid: USA	449	6.00	6.11	0	18.27
(Log) Aid: Japan	449	5.68	6.24	0	16.85
(Log) Aid: Germany	449	5.06	5.91	0	16.60
(Log) Aid: UK	449	5.24	6.09	0	18.26
(Log) Aid: Norway	449	4.28	5.61	0	15.50
(Log) Aid: Private	449	6.95	6.23	0	18.12
(Log) Nr. killed	386	4.05	1.98	0.00	10.31
(Log) Nr. affected	447	11.16	2.60	2.30	19.22
(Log) Population	435	16.87	2.04	11.24	20.97
Pop. density	432	128.26	172.94	1.39	1049.52
(Log) GDP per capita	424	8.02	0.78	6.04	10.09
Oil dummy	381	0.24	0.43	0.00	1.00
Trade as % of GDP	408	65.93	33.87	2.58	213.33
ELF index	354	0.47	0.28	0.00	0.93
Gastil	405	8.23	3.51	2.00	14.00
Affinity index: US	438	-0.22	0.30	-0.60	1
Affinity index: Japan	436	0.57	0.15	0.23	1
Affinity index: Germany	438	0.50	0.19	0.18	1
Affinity index: UK	438	0.34	0.21	-0.13	1
Affinity index: Norway	438	0.53	0.17	0.21	1
Distance: US ('000)	441	9.87	4.03	1.62	16.34
Distance: Japan ('000)	439	8.88	4.39	1.17	18.54
Distance: Germany ('000)	441	7.57	3.27	0.52	16.36
Distance: UK ('000)	441	7.75	3.16	1.02	16.33
Distance: Norway ('000)	441	7.60	3.08	1.02	15.33

Table A.3: Panel: OLS Regressions

Dependent variable: Pr(Donor *j* provides aid after disaster *i* =1)	(1) Pooled OLS	(2) Random Effects Panel	(3) Panel Fixed Effects	(4) Panel Disaster Fixed Effects
Impact measures				
Log(Nr.affected)	0.026***	0.026***	0.026***	
	(0.003)	(0.003)	(0.003)	
Log(Nr. Killed)	0.060***	0.060***	0.060***	
	(0.004)	(0.004)	(0.004)	
Socio economic indicators:				
Log(Population)	-0.058***	-0.058***	-0.052***	
	(0.007)	(0.007)	(0.007)	
Pop. density	-0.000	-0.000	-0.000	
	(0.000)	(0.000)	(0.000)	
Log (GDP per capita)	-0.034***	-0.034***	-0.035***	
	(0.011)	(0.011)	(0.010)	
Oil dummy	0.088***	0.088***	0.088***	
	(0.018)	(0.018)	(0.017)	
Policy performance indicators				
Trade openness	-0.000	-0.000	-0.000	
	(0.000)	(0.000)	(0.000)	
ELF index	-0.019	-0.019	-0.004	
	(0.032)	(0.032)	(0.030)	
Gastil index	-0.002	-0.002	-0.001	
	(0.003)	(0.003)	(0.003)	
Bilateral relation indicators				
Geograph. Distance	-0.004	-0.004	-0.008***	-0.011***
	(0.002)	(0.002)	(0.002)	(0.002)
Affinity index	-0.434***	-0.434***	-0.316***	-0.350***
	(0.031)	(0.031)	(0.053)	(0.087)
Former colony	0.154***	0.154***	0.162***	0.164***
	(0.032)	(0.032)	(0.033)	(0.026)
Donor FE	NO	NO	YES	YES
Disaster FE	NO	NO	NO	NO
Observations	5153	5153	5153	5153
R-squared	0.18	0.18	0.16	0.20

Notes:
Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
All specifications include year, regional and disaster type fixed effects.
The donors included are.Austria, Australia, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Japan, Netherlands, Norway, Portugal, Spain, Sweden, Switzerland, UK and the US

Table A.4: IV Estimation: First Stage Results

Dependent variable: Number of Other Main Donors Providing Aid to Disaster i					
	(1) US	(2) Japan	(3) Germany	(4) UK	(5) Norway
Instruments					
Distance US		0.750***	-0.270	0.619**	-0.300
		(0.267)	(0.375)	(0.260)	(0.359)
Distance Japan	0.330**		0.574***	0.353**	0.684***
	(0.165)		(0.178)	(0.151)	(0.178)
Distance Germany					-8.222***
					(2.116)
Distance UK	-9.411*	-11.988***	-1.403		-1.826
	(5.097)	(4.575)	(5.483)		(5.059)
Distance Norway	8.869*	11.387***	9.562**	10.065**	
	(4.854)	(4.361)	(4.681)	(4.148)	
Impact measures					
Log(Nr.affected)	0.138***	0.159***	0.114**	0.127***	0.095**
	(0.047)	(0.044)	(0.045)	(0.041)	(0.042)
Log(Nr. Killed)	0.330***	0.298***	0.327***	0.311***	0.355***
	(0.049)	(0.045)	(0.047)	(0.046)	(0.043)
Socio economic indicators:					
Log(Population)	-0.409***	-0.381***	-0.362***	-0.312***	-0.262***
	(0.096)	(0.094)	(0.096)	(0.090)	(0.093)
Pop. density	0.000	0.001	0.001	-0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Log (GDP per capita)	-0.317*	-0.288*	-0.305*	-0.222	-0.355**
	(0.162)	(0.155)	(0.160)	(0.160)	(0.161)
Oil dummy	0.296	0.629***	0.387	0.386	0.215
	(0.238)	(0.233)	(0.258)	(0.245)	(0.249)
Trade openness	-0.004	-0.004	-0.002	-0.002	0.000
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
ELF index	-0.238	-0.308	-0.530	-0.477	-0.457
	(0.419)	(0.443)	(0.440)	(0.442)	(0.464)
Gastil index	0.048	0.002	-0.004	0.015	-0.044
	(0.038)	(0.038)	(0.038)	(0.036)	(0.039)
Geograph. Distance	0.644**	0.400***	-7.903***	-10.550**	10.236**
	(0.291)	(0.153)	(2.071)	(4.348)	(4.258)
Affinity index	-1.320***	-3.144***	-2.422***	-1.371**	-2.388***
	(0.431)	(1.049)	(0.794)	(0.693)	(0.886)
Former colony				0.211	
				(0.276)	
Observations	269	269	269	269	269
R-squared	0.44	0.41	0.41	0.38	0.41
Cragg-Donald F-Stat	7.21	8.20	4.56	5.47	5.66

Notes:
Robust standard errors in parentheses
*significant at 10%; ** significant at 5%; *** significant at 1%
All specifications include year, regional and disaster type fixed effects.

Chapter 2

Be as Careful of the Books You Read as of the Company You Keep: Evidence on Peer Effects in Educational Choices

Giacomo de Giorgi

Michele Pellizzari

Silvia Redaelli

1 Introduction

The importance of peers in shaping individual and social behavior has been widely recognized in both the economic and the sociological literature. Numerous studies have produced empirical evidence showing the existence of relevant peer effects in many areas, from schooling performances to criminal behavior, from productivity to financial decisions (Katz and Case, 1991; Hoxby, 2000; Sacerdote, 2001; Duflo and Saez, 2002; Ammermueller and Pischke, 2006).

However, the identification of social interactions remains very problematic because of two well-known issues: endogeneity - due either to peers self-selection or to common group (correlated) effects - and reflection - a particular case of simultaneity (Manski, 1993; Brock and Durlauf, 2001; Moffitt, 2001 and Soetevent, 2006).

The contribution of this paper is twofold. First, on the methodological side, we develop a new strategy for the identification of endogenous peer effects.[1] In a policy perspective, this is the crucial parameter for interventions that directly influence one's peers outcomes. A leading example is the immunization program implemented in Kenya analyzed in Miguel and Kremer (2004) or the PROGRESA program in Mexico (Todd and Wolpin, 2006). Moreover, in a general equilibrium framework with social interactions, endogenous effects are paramount for the ex-ante evaluation of any policy intervention.

Second, we estimate the role of peer effects on students' choices of college major. Further, we are able to assign a wage value to choosing an academic major according to one's peers, thus shedding some light on the mechanism that generates social interactions.

Our study is based on a newly constructed set of administrative data of undergraduate students from Bocconi University.

The particular structure of the degree programs offered by this institution allows to define peer groups that vary at the level of the single individual and as such are not subject to the usual simultaneity (reflection) problem. Moreover, we identify a natural set of exogenous instruments to control for correlated (within group) effects, which have been recognized as important

[1] The endongenous peer effect is usually defined as the impact of the average peers' outcomes on individual outcomes. See Section 4 for details.

determinants of group outcomes.[2]

At Bocconi University, students initially enroll in a common program and only at the end of the third semester (i.e. after 1 and 1/2 years) do they choose whether to specialize in one of two majors: business or economics. During these first three semesters all students take nine compulsory courses and attend lectures in randomly assigned classes. Since the number of available lecturers varies for each course, the assignment of students to classes is repeated for each course (see Section 2).

This setting allows us to define peer groups using information on class assignment. In other words, we assume that student i interacts with the other students attending the same lecture (in the same classroom with the same lecturer) in any of the common compulsory courses. The repeated process of random assignment to classes generates peer groups that vary at the individual level: student i's peers study with i but also with other students who are not necessarily members of i's peer group. As the peer groups of i and i''s peers do not coincide, we are able to solve the reflection problem (see Section 4).

Moreover, since the allocation into the classes is random, endogeneity in peer group formation is excluded by construction. Having peer groups that vary at the individual level also guarantees the presence of excluded classmates, i.e. students who did not attend classes with i but did attend some courses with some of i's peers. The exogenous characteristics of excluded peers represent a natural set of instruments to overcome potential endogeneity generated by common (correlated) group effects.[3]

Further, our data also contain a very rich set of observable proxies for those variables that are commonly believed to induce self-selection (i.e. ability, motivation, preferences etc.).

The combination of the particularly rich dataset, the repeated randomization and the peculiar construction of the peer groups allows us to solve the

[2] A similar approach could in principle be adopted in a number of other contexts, i.e. whenever units of analysis are linked directly to some other units (the peers) but only indirectly (through peers) they are further connected to others (the peers of peers). In the network literature (Calvó at al., 2004) this corresponds to the existence of links of degree 2. For example, groups of this type may arise when members of a football team are also members of other social groups (baseball, study group, etc...) and the two groups do not perfectly overlap.

[3] The usual suspects for group shocks in the education framework are teachers's effects or classmates' disruptive behaviors.

two key econometric problems of this literature: reflection and endogeneity, be it induced by sorting or correlated effects.

Our econometric methodology differs from the existing literature that tries to recover peer effects using either laboratory experiments (Falk and Ichino, 2006), natural experiments (Sacerdote, 2001; Zimmerman, 2003), quasi-experimental designs (Hoxby, 2000), or fixed effects (Hanushek *et al.*, 2003). The repeated randomization process exploited here distinguishes our approach from most previous studies (Sacerdote, 2001; Zimmerman, 2003) where the randomized assignment - when there is one - is typically done only once and for all.[4] Laschever (2005) is, to our knowledge, the first application of a multiple group framework.

The spirit of our identification strategy is also similar to Bayer *et al.* (2004), a study of criminal behavior that exploits the length of the individual's sentence to weight each peer's characteristic by the time spent together in the same correctional institution. Two other papers - developed independently at the same time as ours - are very close to our approach: Bramoullé *et al.,* 2006 and Calvó *et al.,* 2006. The former provides the theoretical identification conditions for endogenous peer effects in a network framework, while the latter focuses on the position of a player in a particular network in influencing performance.

With our approach we are able to identify the causal effect of peers' choice of major (economics vs. business) on one's own decision. The only paper that looks at this particular outcome is Sacerdote (2001), which does not find any significant influence of peers. In that paper, however, peer groups were defined on the basis of a single random assignment to rooms in campus dorms. Therefore, only the potential bias from endogenous sorting is excluded, while groups are fixed across peers and as such cannot (directly) account for reflection and possible common shocks. In fact, consistently with Sacerdote's results, our estimates are statistically significant only when we control for the potential bias from correlated effect.

[4]However, the first insights to our identification strategy were somewhat implicit in the work of Manski (1993) and particularly in Moffitt (2001). Manski (1993) suggests the possibility of extending the model of interactions to multiple groups in Footnote 1(i) of page 534. Moffitt (2001) suggests the use of a partial population experiment, generating an exclusion restriction, along the same line of reasoning as our approach, which is also based on exclusion restrictions and can be seen as a peculiar partial population experiment. The original approach proposed by Moffit (2001) is taken in Bobonis and Finan (2005), Lalive and Cattaneo (2005), Cipollone and Rosolia (2006), and Cooley (2006).

Our results show that, indeed, one is more likely to choose a major when many of her peers make the same choice. We, then, look at whether students who specialize in a major following the choices of their peers and against their revealed relative ability (measured as the ratio of one's average grade in economics and business courses during the first three semesters) perform better (in terms of average grades in the last three semesters, final graduation mark and time to graduation) than similar students who chose the major according to their revealed ability and against the majority of their peers.[5] Our findings indicate that, indeed, there is a negative effect of following one's peers when revealed ability would suggest a different choice. We, then, try to assign a monetary value to this effect by looking at the wage cost of such a lower academic performance. We estimate that cost to be as high as 1,117 USD a year.

We can think of at least three mechanisms that are potentially important in generating the type of social interaction we see. First, peer pressure (Mas and Moretti, 2006), being it monitoring or imitation, might be substantial in leading a student towards a particular major choice. Second, there might simply be a utility gain to studying with friends. Third, peers may facilitate the acquisition of information (or constitute a reference group in the formation of expectations) on university life and job opportunities associated with a particular major.

Although our research design is not best suited to distinguish among these alternatives, it seems plausible to rule out the information mechanism. In fact, better informed students should make "better" choices and this is at odds with our findings in terms of average grades and graduation mark. Other papers (Ichino and Maggi, 2000) have devoted more attention to the analysis of the specific mechanism that generate social interactions but typically without being able to separately identify endogenous and exogenous peer effects.

The paper is organized as follows: Section 2 describes the institutional structure of Bocconi University, the available data and the details of the allocation of students into classes; Section 3 presents our approach for the construction of the peer groups; Section 4 discusses the identification strategy and the results of the analysis of the choice of major. In Section 5 we

[5] Final graduation mark is in the particular University the sum of GPA plus additional points rewarded to a compulsory dissertation.

provide a number of robustness checks. Section 6 discusses the effects of the decision modes on average GPA, graduation mark and time to graduation. Finally, Section 7 concludes.

2 Data and institutional details

The analysis in this paper is based on administrative data from Bocconi University, an Italian private institution of higher education that specializes in business and economics. The data provide detailed information on the university curricula of all students enrolled at Bocconi since 1989.

Until the academic year 1999/2000, the most popular degree offered by Bocconi was called CLEA/CLEP. Students in this degree would first take a series of nine common exams during the first three semesters and would then choose whether to specialize in business (CLEA) or economics (CLEP) (See Figure 1). The nine common compulsory courses are listed in Table 1 and can be classified by subject areas according to the department responsible for the teaching: business, economics, quantitative subjects and law.

[Figure 1: Degree structure]
[Table 1:Common exams CLEA/CLEP]

In the academic year 1999/2000 Bocconi introduced a major reform of its structure (the so-called "Bocconi 2000" plan), abandoning this initial common track and forcing students to choose between economics and business upon entering the University. The information on the random allocation of students to classes has unfortunately been lost for the earlier cohorts of students and is reliable only starting with the academic year 1998/1999. This forces us to use only one cohort of students, i.e. students enrolled in the old CLEA/CLEP program in the academic year 1998/1999.

At that time, Bocconi offered four other degree programs: one in "Economic and Social Sciences" (DES), one in "Economics of Financial Market Institutions" (CLEFIN), one in "Management of the Public Administration and International Institutions" (CLAPI) and one in "Law and Business

Administration" (CLELI).[6] These degree programs differ both in their curricula and in the number of students admitted in each academic year.[7] In September 1998, a total of 2,580 students were admitted and 2,055 of them eventually enrolled at Bocconi.[8]

In their application package, perspective students had to rank the five programs according to their preferences. Admission was based on a standardized entry test combined with high school performance. Applicants were then ranked according to these results and, starting from the top of the ranking, students were assigned to their preferred programs depending on availability. Specifically, a student was assigned to her first choice if there were still places available in that program, otherwise, if all places in her first choice had already been taken by students higher up in the ranking, the candidate was assigned to her second choice and so on.

It is important to notice that in this mechanism the student's stated preferences across the five programs do not influence the probability of being admitted and thus excludes any strategic behavior in the reporting of preferences. This allows us to use this information to construct an indicator of ex-ante preferences. In particular, we consider students who indicated the DES degree - the more academic oriented version of CLEP - as a first or a second choice as "determined" to do economics since the beginning of their studies.[9] Similarly, students who indicated DES as a last choice are coded as "determined" to specialize in business.

Admitted candidates who decided not to register freed places for students further down in the ranking. However, only a few students (48 out of the 753 rejected candidates) who had been initially rejected took up a place freed by others, possibly because at the time of making these decisions most people had already obtained admission to another university and started to make arrangements for the registration and the accommodation.[10]

[6] Created in 1970, CLEA (Degree in Business Administration) and CLEP (Degree in Economics) are the oldest degrees offered at Bocconi University. Four years later, they were joined by DES, a more quantitative and academic version of the CLEP. All the other degrees (CLEFIN, CLAPI and CLELI) were introduced in 1990.

[7] Enrolment ceilings and admission tests were introduced in 1984.

[8] We are excluding students transferring from other universities and students from abroad who were given reserved places.

[9] These are students who either had CLEA/CLEP as a first choice and DES as a second or DES as first and CLEA/CLEP as second and did not get a place in the DES.

[10] Note also that candidates in the lower tail of the distribution of the admission test were not offered any of these residual places.

Eventually, the admission procedure in September 1998 led to 1,385 students (against a ceiling of 1,600) enrolled in the common CLEA/CLEP track, followed by CLELI (239 against a ceiling of 350), CLEFIN (208 against a ceiling of 230) CLAPI and DES (respectively with 132 and 91 against ceilings of 200 each). Once enrolled, CLEA/CLEP students were not allowed to switch to any of the other degrees, while students enrolled in the CLELI, CLEFIN, CLAPI and DES programs could move to CLEA/CLEP only after the first academic year.

In this paper we will focus exclusively on students enrolled in the CLEA/CLEP common track. Excluding a few missing values on our variables of interest and those students who did not complete the courses of the first 3 semesters, our working sample consists of the 1,141 observations described in Table 2. All of these students have complete information about their courses in the initial three semesters. A few of them (slightly less than 10%) have not graduated, either because they dropped out, changed university or are still enrolled and trying to graduate.

[**Table 2: Descriptive statistics**]

After the first 3 semesters of common courses, each student originally enrolled in CLEA/CLEP had to choose whether to specialize in business (CLEA) or in economics (CLEP). Table 3 reports some descriptive statistics on the ability and performance of these two groups of students.

[**Table 3: Characteristics of CLEA/CLEP**]

Considering all the common exams in the first three semesters, the 145 students choosing CLEP score on average almost 2 grade-points above CLEA students (exams are graded on a scale 0 to 30 with pass equal to 18). This difference is even higher when the exams are disaggregated by field. As expected, CLEP students perform relatively better in economics, while the difference is considerably smaller for the average grade in business exams, suggesting - as we will see more formally later on - that students choose their field of specialization according to their relative abilities or interest. Furthermore, the difference in the average grade of the exams of the quantitative courses is also very large, reflecting the nature of the CLEP program that was considerably more quantitative than CLEA.

2.1 Lecturing classes

The number of classes created for each of the nine common exams depends on the number of available lecturers.[11] Moreover, the capacity of the available classrooms at Bocconi varied considerably and the number of students in each class had to be determined accordingly.

Students were randomly allocated to classes for each course. The decision to adopt a random allocation algorithm was dictated by the need to avoid congestion in the classrooms resulting from students wanting to attend lectures with their friends or with the "best" teachers.

Towards the end of each term, students had to enroll in courses of the following term either at the administration desk or through some computers located in the university buildings.[12] Moreover, students who failed to pass an exam during the academic year in which they had attended the corresponding course, were required to re-register and were also assigned randomly to a new class (together with other students).[13] For these reasons, the total number of students enrolled in each course (the sum over all the classes) may vary slightly across courses.

At the time of enrollment, the algorithm would randomly assign the student to a class for each course and communicate the allocated class number.[14] The algorithm was designed to fill all classrooms at the same rate in order to obtain a final distribution with an adequate number of students in each room. By no means could the students interfere with the algorithm. For example, there was no guarantee that two students enrolling in the same course one right after the other would be placed in the same teaching class

[11] The terms *class* and *lecture* often have different meanings in different countries and sometimes also in different schools within the same country. In most British universities, for example, lecture indicates a teaching session where an instructor - typically a full faculty member - presents the main material of the course. Classes are instead practical sessions where a teacher assistant solves problem sets and applied exercises with the students. At Bocconi there was no such distinction, meaning that the same randomly allocated groups were kept for both regular lectures and applied classes. Hence, in the remaining of the paper we are going to use the two terms interchangeably.

[12] Enrolment in the courses of the first term of the first year was automatic. Students were also free to choose whether they wanted to postpone some of the courses (e.g. take a course of the second semester in the third and so on) provided they satisfied the prerequisites for each exam (e.g. statistics could only be taken after having passed math).

[13] There are normally up to 7 exam sessions per year for each of the 9 common courses during the academic year.

[14] This was just a particular number or letter by which it would be easy to look for venues and communication concerning a particular class on the University notice board system.

(and, in fact, despite the many that attempted to do so, this instance was extremely rare).

In principle, students were required to attend lectures in their assigned classes but enforcement varied substantially over time, becoming stricter in more recent years. Actually, the evolution of enforcement practices is closely related to the availability of the information on lecturing classes: as the enforcement of the allocations was made more and more stringent, lecturing classes were also recorded on various official documents and thus maintained in the administration's archives.

The mere fact that lecturing classes have been carefully recorded for the 1998/1999 cohort is an indication that the system was effectively enforced.[15] Students were forced to attend their classes by various methods. First, lecturers were supposed to circulate attendance sheets at the beginning of the class for students to sign their presence. Obviously, with a large number of students in each class (the average class size was 202 students), this method could be easily circumvented by those who wanted to attend a different class by, for example, having some friends signing for them. Mid-terms were also important in encouraging students to attend their assigned classes. In fact, while the final exams were identical for all students regardless of their classes, mid-terms were organized directly by the lecturers. Therefore, if a student wanted to take the mid-term (which were not compulsory but highly recommended and very popular among the students) she'd better attend her assigned class as the exam was prepared and marked by the same lecturer.

[**Table 4: Characteristics of courses and lecturing classes**]

Table 4 describes the average characteristics of the lecturing classes for each course. The number of classes ranges from 4 (private and public law) to 10 (mathematics, management and accounting) and the average number of enrolled students varies accordingly. The other variables in Table 4 are derived from students' questionnaires. At the end of each course, during a regular lecture time, students were distributed a standardized anonymous questionnaire to collect their opinions about numerous aspects of the teaching (quality of the lectures, logistics, etc.). A detailed description of the

[15] There are less than 2% of missing values.

data available at the level of the single class is provided in Table A.1 in the appendix.

The number of completed questionnaires is a one-off measure of attendance, as it should correspond to the number of students present in class on the day the questionnaire was distributed. Attendance is also self-reported by the students in the questionnaire, where they have to indicate the fraction of lectures they attended for that course. These figures indicate that attendance was typically very high, with students being present at over 80% of the lectures for economics, management and quantitative courses.

Only law subjects have very low attendance levels. At that time Bocconi did not have a law department and relied exclusively on external professors (from other universities). For this reason the number of law classes that could be created was relatively small (4) and their size was consequently extremely high; the administration was well aware of low attendance for these courses.

3 Peer group definition

Our definition of peer groups is based on students attending the same classes and it is meant to capture the network in which students interact academically and socially. The underlying assumption is that these interactions are fostered by class attendance so that the relevant set of peers for each student overlaps (at least partly) with classmates.[16]

While considering classes is standard in the literature on peer effects in high-school, in our case effective attendance as well as the size of the lecturing classes cast doubts on the possibility of capturing relevant peer interactions by looking at assigned classmates (see Table 4). We address this problem by excluding the two law courses from our definition of peer groups and also by weighting peers by the number of common courses attended together.[17]

Formally, individual i's peer group (G_i) includes all individuals j who were assigned to the same class as individual i for at least one of the 7

[16]If two students were to attend only one course together they would sit in the same class for six hours a week (three two hours classes) for one semester.

[17]Public and private law are the courses with the largest average class size as well as the lowest average class attendance, both self-reported and measured by the ratio of collected questionnaires over the number of officially enrolled students.

courses that we consider (all 9 common exams minus the 2 law subjects). Furthermore, each of the $j \in G_i$ is given an importance weight, $\omega_{ij}(0, 1]$, according to the number of common courses taken together with i, i.e. $\omega_{ij} = 1$ if j attends all 7 courses in the same class as i, $\omega_{ij} = 1/7$ if j attends only 1 course with i.[18]

As a further robustness check, we conduct our analysis also using groups formed on the basis of a stricter definition of peers, namely students who have attended at least 4 of the 7 common courses together.[19] This restricted definition is particularly interesting because it leads to groups sizes that are comparable to other papers in the literature, particularly those that have looked at high-school classes.

[Table 5: Size of peer groups, various definitions]

The first two columns of Table 5 report some characteristics of these groups. In column 1 the groups are constructed considering 7 courses while in column 2 we consider peers only students who have attended at least 4 courses in the same classes. The mean raw group size is approximately 674 students in the first case and goes down to 18 in the second. On average students in these groups are assigned to the same classes for 1.6 and 4.2 courses respectively, which implies that, when peers are weighted by the number of courses taken together, the size of the groups goes down to 151 with 7 courses and 10.7 with our restricted definition.

[Table 6: Peers and later accademic patterns]

Table 6 provides evidence to support our definition of peers by showing that, after the initial 3 semesters, students who have attended lectures in the same random classes also show similar academic patters. In the upper

[18]The weights adopted in the core of the paper are linear in the number of courses attended together. We have experimented with many other specifications and the results are robust to the weighting scheme, see Section 5.

[19]This is essentially a variation in the weighting scheme that assigns zero weight to peers who have attended less than 4 courses together. We choose the threshold of 4 courses because it is the highest that guarantees a non-empty peer group for all students (i.e. there are some students who have never taken more than 4 courses with others).

panel of Table 6, for example, we contrast the incidence of peers and non-peers that choose the same sub-major (i.e. field). In fact, within each of the main majors - economics and business - students can specialize in different fields, like marketing or accounting within business and finance or theory within economics. The students in our sample could choose among 8 sub-majors within the economics area and 16 sub-majors within the business area. Using our most comprehensive definition of peers, i.e. students who have attended at least one of the 7 common courses in the same random class (column 1 in Table 6), on average slightly more than 9.6% of peers choose the same major. This compares to a marginally lower incidence of students making similar choices among the non-peers (i.e. students who have never taken any class together). As we restrict our definition of peers to students who have attended more and more courses in the same classes (columns 2 and 3), the difference between peers and non-peers increases and becomes statistically significant. Only with the stricter definition (column 4) this difference becomes smaller and insignificant again.

The lower panel of Table 6 analyzes graduation sessions. In the period covered by our data students could graduate in several different sessions throughout the year (almost one session per month). During these sessions, which lasted one or two days, students present their final dissertation to a commission which decides their final mark (based on both the dissertation and their GPA). Students can freely choose when to graduate, a decision that is usually affected both by how quickly they complete their coursework and by how much time they spend on their dissertation.[20] For the average student in our sample approximately 12.5% of the non-peers graduate in the same session. This number goes up to 13.4% for peers in our widest definition (column 1) and increases steadily as the definition becomes more stringent (columns 2 to 4). The differences are always strongly significant.

The evidence in Table 6 shows that randomly assigned peers eventually follow similar academic patterns, suggesting that they actually interact with each other. Moreover, the stronger effects that emerge for peers that have attended more and more courses together supports our weighting scheme, which should indeed emphasize the most intense interactions. In section 5 we perform additional robustness checks by modifying our definitions of

[20] Late graduation has always been one of the most serious problems of the Italian university system. See Garibaldi *et al.* (2007).

peer groups.

4 Peer effects in major choices

The identification of endogenous social effects has been the topic of several papers (Manski, 1993; Brock and Durlauf, 2001 and Moffitt, 2001 to cite just a few) and it rests on two distinct dimensions: endogeneity and reflection. Endogeneity may arise for at least two reasons: first, people usually choose endogenously their peers and, second, the unobserved shocks that affect the group as a whole (teacher effects are the usual suspect in studies of education) may also generate endogeneity. As a consequence, when detecting a significant correlation between individual and group outcomes, one cannot say whether this result is due to true peer effects or simply to endogenous group formation (along some unobservable characteristics) and/or common correlated effects.

The second problem - reflection - arises because in a peer group everyone's behavior affects the others and, as in a mirror reflection, we cannot know if one's action is the cause or the effect of peers' influence. This is essentially a problem of simultaneity.

Let us start with a discussion of how we address reflection. This problem has been commonly described by using a simple linear in means model:

$$y_i = \alpha + \beta E(y|G_i) + \boldsymbol{\gamma} E(\mathbf{x}|G_i) + \boldsymbol{\delta}\mathbf{x}_i + u_i \tag{1}$$

In our framework, y_i is the chosen major (i.e. economics or business), $\mathbf{x}_i$ is a set of individual traits, $E(\mathbf{x}|G_i)$ contains the averages of the $\mathbf{x}$'s in the peer group of individual i, denoted by G_i. Following the literature, β measures the endogenous effect, $\boldsymbol{\gamma}$ the exogenous effects. For now assume $E(u_i|G_i, \mathbf{x}_i) = 0$, i.e. no correlated effects or self-selection into groups.

In the standard framework, peer-groups are fixed across individuals, i.e. if A and B are both in the peer group of C, it must also be that A and B are in the same group. Put in the wording of equation (1), if i and j are in the same peer-group, then the two groups coincide, i.e. $G_i = G_j$. In this situation, endogenous effects cannot be distinguished from exogenous effects (Manski, 1993). In fact, it is easy to show, by simply averaging equation (1) over group G_i, that $E(y|G_i)$ is a linear combination of the other regressors:

$$E(y|G_i) = \left(\frac{\alpha}{1-\beta}\right) + \left(\frac{\boldsymbol{\gamma}+\boldsymbol{\delta}}{1-\beta}\right) E(\mathbf{x}|G_i) \tag{2}$$

In our framework peer groups are instead individual specific. Consider the simple case of only three students. Students A and B study together (e.g. they attend 3 courses in the same classes), however, B also studies with C (e.g. they attend some of the remaining 4 courses in the same class, different from A's class). A's peer group, thus, includes only B while B's peer group includes both A and C. This identification can also be seen as a case of triangularization. In the standard simultaneous equation model at least one exogenous variable is excluded from each equation; here, A is excluded from the peer group of C, who is excluded from the peer group of A.

With 7 courses, each divided into 6 to 10 lecturing classes, our data exhibit enough variation to generate peer-groups that vary at the level of the single individual, e.g. every student has a different peer-group. The weighting scheme described in the previous section adds more variation to the individual peer groups.

To formally see the advantage of this framework in solving the reflection problem, rewrite equation (2) allowing peer-groups to vary at the level of the single individual:

$$E(y_i|G_i) = \alpha + \beta E[E(y|G_j)|G_i] + \boldsymbol{\gamma} E[E(\mathbf{x}|G_j)|G_i] + \boldsymbol{\delta} E(\mathbf{x}_i|G_i) \tag{3}$$

where j is a generic member of i's peer group. The key to understanding this equation is the fact that j's peer group G_j never coincides with G_i.

This result can also be clarified by the previous example with 3 students: A, B and C where A and B are in the same class for one subject and B and C sit together in another course. This structure implies that $G^A : \{B\}$, $G^B : \{A, C\}$ and $G^C : \{B\}$. Equation (1), then, translates in the following three equations:

$$\begin{aligned}
y_A &= \alpha + \beta y_B + \gamma x_B + \delta x_A + u_A^A \\
y_B &= \alpha + \beta\left(\frac{y_B + y_C}{2}\right) + \gamma\frac{(x_B + x_C)}{2} + \delta x_B + u_B^B \\
y_C &= \alpha + \beta y_B + \gamma x_B + \delta x_C + u_C^C
\end{aligned}$$

Now, consider the corresponding reduced form equations:

$$
\begin{aligned}
y_A &= \left(\alpha + \frac{\alpha\beta\,(1+\beta)}{1-\beta^2}\right) + \left(\frac{\beta(\gamma+\delta)}{1-\beta^2} + \gamma\right) x_B + \left(\frac{\beta(\gamma+\delta\beta)}{1-\beta^2}\right)\left(\frac{x_A + x_C}{2}\right) + \delta x_A + \eta_A^A \\
y_B &= \left(\frac{\alpha\,(1+\beta)}{1-\beta^2}\right) + \left(\frac{\gamma+\delta}{1-\beta^2}\right) x_B + \left(\frac{\gamma+\delta\beta}{1-\beta^2}\right)\left(\frac{x_A + x_C}{2}\right) + \eta_B^B \\
y_C &= \left(\alpha + \frac{\alpha\beta\,(1+\beta)}{1-\beta^2}\right) + \left(\frac{\beta(\gamma+\delta)}{1-\beta^2} + \gamma\right) x_B + \left(\frac{\beta(\gamma+\delta\beta)}{1-\beta^2}\right)\left(\frac{x_A + x_C}{2}\right) + \delta x_C + \eta_C^C
\end{aligned}
$$

where the new reduced form error terms- η_A^A, η_B^B and η_C^C - are linear combinations of the structural error terms - u_A^A, u_B^B and u_C^C.[21] The example above shows how we achieve identification: we are left with four reduced form parameters and four structural ones. Notice, additionally that in this particular case the last equation is redundant and, in fact, only observations with distinct groups of peers contribute to identification.[22]

Although this particular setting allows to solve reflection, one might still worry about the presence of correlated effect, i.e. common unobservable shocks at the group level which could flaw the previous identification result. Suppose, in fact, that the general error term is of the following form:

$$u_i^g = \mu_i + \theta^g + \varepsilon_i \tag{4}$$

with $g = A, B, C$ and where μ_i is an individual fixed effect, θ^g a group fixed effect (e.g. teacher quality, disruptions), and ϵ_i an i.i.d. random component.[23]

If we were to substitute 4 into 1 we would have to face two problems of endogeneity arising from the individual effect (μ_i) and the group effect (θ^g). In our particular case, the random nature of the peer groups rules out correlation between the individual effect and any endogenous or exogenous effect ($E(y|G_i)$ and $E(x|G_i)$).[24] However, unobservable group shocks could

[21] The meaning of the double indexing - subscript and superscript - will become clear in a few paragraphs.

[22] In fact A and C here have the same peer group, $\{B\}$, although they are not peers to each other.

[23] The double indexing of the previous error terms should clarify the fact that these errors include both an individual specific (μ_i) and a group shock (θ^g).

[24] Additionally, our data include several observable proxies for variables that are generally unobservable to the econometrician (i.e. standardized ability test, high-school grades, type of high-school, preferences, etc.) and we make use of all of them to purge our results from potential residual endogeneity

still be present and induce endogeneity, i.e. $Cov\left(E\left(y|G_i\right), \theta^g\right) \neq 0$.[25] Even if our strategy effectively solves reflection, the presence of correlated effects may still generate endogeneity of $E(y|G_i)$ and impede identification.

One possible solution is to use instrumental variables. Fortunately, this setting naturally offers valid instruments, namely peers of peers who are not in one's peer group. In fact, the $\mathbf{x}$'s of students who are excluded from i's peer group but included in the group of one or more of i's peers are by construction uncorrelated with the group fixed effect of i and correlated with the mean outcome of i's group through peer effects. In our previous example, x_C would be a valid instrument for y_B in group A. The logical chain is the following: x_C, which is uncorrelated with θ^A, affects y_C and, since C is a peer of B, through endogenous effects y_C also affects y_B. For the same reasoning x_A would be a valid instrument for y_B in group C.[26]

In our data, the group of peers of peers - which we label *excluded peers* for clarity - for a generic student i includes all other students who have never taken any of the 9 common courses in the same lecturing classes of i but have taken some of the 7 courses that we consider with one or more of i's peers. The average raw size of these groups is 252 students, as reported in the third column of Table 5. Notice additionally that the union of the groups of excluded and actual peers never spans the entire sample. The student with the largest groups is linked either directly or indirectly to 1085 students, thus allowing for more than 50 totally excluded peers. On average the sum of the two groups is 927 and notice that we keep the same definition of excluded peers also when using the restricted definition of peers.[27]

To better document the absence of self-selection in our setup, Table 7 reports the correlation coefficients between individual and group averages of some measures of predetermined ability and motivation for various definitions of peers. In column [1] peer groups are constructed considering all common exams excluding the two courses in law, whereas in column [2] the

[25] Note that correlated effects cannot induce endogeneity of the exogenous effect - $Cov\left(E\left(x|G_i\right), \vartheta^g\right) = 0$ - since the x's are determined prior to the allocation to the groups.

[26] Bramoullè et al. (2006), a working paper developed parallelly and independently from this work, also discuss this IV methodology but, as far as we know, our paper is the first empirical application of this strategy.

[27] This guarantees that the excluded peers of student i never attended any course in the same class of i. We could eliminate from the excluded peers those who have attended less than 4 courses together with any of i's peers but this would lead to an empty set of excluded peers for many observations.

groups are based on the restricted definition of peers, i.e. students who have attended at least 4 courses together.

[Table 7: Correlation between individual and group level predetermined variables]

The numbers in Panel A of Table 7 show that peers are not clustered by any of the attributes considered. This result is obviously not very surprising given that our peer-groups are based on random assignment to classes. The same reasoning applies to the results in Panel B which shows the correlations between individual and the average excluded peers' attributes.

To conclude, Panel C reports correlations between the average peers' characteristics and the average characteristics of the excluded peers, which are nothing but a random subsample of each peer group's complementary set. The negative and significant correlations arise mechanically from the fact that any small deviation of peers' attributes from the population averages is counterbalanced by a symmetric opposite deviation in the characteristics of the non-peers and hence also of the excluded peers. This mechanical correlation adds power to our instruments.

4.1 Results

As already mentioned, the CLEA/CLEP program offered only two majors: economics and business. Students had to make their choice after the initial three common semesters and the remaining five terms were clearly differentiated across the two majors.[28]

To estimate the effect of peers on one's decision to specialize in economics versus business, we run both a linear probability model and a probit regression similar to equation (1), where $y_i = 1$ if a student chooses economics and 0 otherwise. $E(y|G_i)$ is the share of peers choosing economics (weighted by the number of exams taken together) and $\mathbf{x}_i$ is a set of controls for individual characteristics that includes a gender dummy, household income (as recorded at the first registration), a dummy for students who reside outside the city of Milan (the site of Bocconi), a set of dummies for the region of

[28] Although some elective courses could be picked from any of the two majors, nevertheless such practice was quite uncommon and the number of such options very limited.

origin, a series of controls for academic performance and ability (high-school type and grades, results of the admission test) and an indicator of ex-ante preferences over the two majors (i.e. whether a student was determined to do economics at enrollment, as described in Section 2). Given the randomness and the relatively large size of the peer groups we have very little variation in $E(x|G_i)$ to separately identify the constant and γ in equation (1). Therefore, in the main specification we omit the average predetermined characteristics of the peer group. The results are however robust to controlling for a subset of $x's$ at the group level, the estimated γ in that specific case is never significantly different from zero.[29] Moreover, when working with the smaller groups of restricted peers, the (random) sampling variation in $E(x|G_i)$ is larger and allows to separately identify γ and the constant (see Tables A.4 and A.5).

[Table 8: Peer effects in the choice of major. Linear probability model]

Table 8 reports the results of the estimation of a linear probability model and for our two definitions of peer groups, one based on all 7 common courses - columns 1 to 3 - and one based on the restricted set of peers who have attended at least 4 courses in the same classes - columns 4 to 6. For each of these definitions we estimate the model under three different specifications: simple OLS, IV using the exogenous characteristics of the excluded peers (i.e. the peers of peers who are not in one's peer group) as instruments and IV using the same instruments weighted by the number of courses that each excluded peer has attended with any of the student's peers (see Section 4 for a detailed description of the instruments).

These estimates clearly indicate the presence of significant endogenous peer effects in the choice of major. Considering the first definition of peers in columns 1 to 3, only in the OLS specification the estimated endogenous effect is not significant while the IV results are considerably (5 to 6 times) larger.

[29] Our IV strategy uses the mechanical correlation between the $x's$ of peers and those of the excluded peers, if we were to control for $E(\mathbf{x}|G_i)$ in the main equation we would lose part of the IV strength.

For the correct interpretation of these results one should keep in mind that our measure of the endogenous effect weights peers by the number of courses attended in the same classes. Thus, for the average student the effect of one additional average peer - i.e. students with whom she has taken 1.57 courses together (see Table 5) - who chooses economics increases the probability of choosing economics by approximately 0.8-0.9 percentage points (according to our IV estimates).[30] Similarly, the effect for the average student of having one more of her strongest peers - i.e. students with whom she has taken all the 7 common courses together - choosing economics is approximately a 4 percentage-point increase in the probability of choosing economics.[31]

It is generally thought that the OLS results over-estimate the actual size of the peer-effect because they cumulate the impact of both exogenous and endogenous effects, further it is often unclear how a group shock biases those results. In our analysis, however, exogenous peer-effects are ruled out by the random nature of the groups and we consequently exclude them from the main specification presented in Table 8.[32] The remaining concern in estimating the endogenous effects in our study rests on the possible presence of correlated effects, i.e. common group shocks.

The most common interpretation of correlated effects typically assumes that the group shock affects all students in the same direction, thus leading to lower dispersion in individual outcomes within groups. In our case, the positive difference between the IV and the OLS estimates suggests, instead, that the correlated group shock leads students in the same group to make more differentiated choices that they would have otherwise made. A possible example with teacher quality would be the following: encountering the most informative of economics professors offers all students a clear picture of what

[30] The average student in our sample has approximately 151 average peers - i.e. peers with whom he/she has attended an average of 1.57 courses - and approximately 13% (i.e. approximately 20) of them choose economics as a major (see Table 5). Hence, if one additional (average) peer chooses economics, $E(y|G_i)$ for the average student increases by 1.57/(7x151)=0.0015, which according to our IV estimates leads to an increase in the probability of choosing economics of 0.0015x5.785=0.0086.

[31] Hence, if one additional strong peer chooses economics, $E(y|G_i)$ for the average student increases by 1.57/151=0.0066, which, according to our IV estimates, leads to an increase in the probability of choosing economics of 0.0066x5.785=0.0383.

[32] As mentioned earlier and documented in Tables A.4 and A.5, the inclusion of exogenous peer effects does not affect our results significantly, especially when we consider the smaller groups of restricted peers that allow for more sampling variation in $E(\mathbf{x}|G_i)$.

the subject is really about thus allowing them to make their own choice according to their actual preferences and without relying much on their peers.

To support this interpretation, we repeated the analysis focusing exclusively on the subset of students with the most homogeneous groups of peers along one specific dimension, that is the initial preference for economics. We selected only those students with either very many (top 90^{th} percentile) or very few (bottom 10^{th} percentile) peers who were "determined" to do economics since their first enrolment at Bocconi. In these homogenous groups we expect the correlated shock to affect (almost) everyone in the same direction, thus leading to similar individual behaviors. Consistently with this interpretation, in this selected sub-sample the IV estimate are smaller than the OLS (we omit the results for brevity). Thus, the difference between the OLS and the IV coefficients seem to indicate that correlated effects play an important role in this set up. In particular, the relative quality of teachers in the two areas (economics vs. business) may in fact be one of the crucial determinants of students decisions. It might also be that in some classes a particularly disruptive behavior could effectively compromise the thorough understanding of the more formal subjects, creating a similar effect to that described earlier.

Notice additionally that the limited variation in the endogenous variable - $E(y|G_i)$ - exacerbates the downward bias of the OLS estimate (which is in the order of 5 to 6 times the IV estimates).[33] This feature of the data also helps comparing our results with what is found in Sacerdote (2001), where peers are also randomly assigned but the data do not allow to solve the reflection problem nor the potential endogeneity due to common group shocks. Our OLS estimates are in line with the results in Sacerdote (2001) where no significant effect is found on major choice. However, once we account for possible endogeneity, the effect becomes sizeable and significant.

The last three columns (4 to 6) of Table 8 repeat the same exercise using our restricted definition of peers. The estimated coefficients are now much smaller. This is consistent with the fact that, given the smaller groups,

[33] To clarify this point, consider a simple linear model with just one regressor: $y = x\beta + \varepsilon$, where x is endogenous and a valid instrument z is available. In this simple case, the OLS estimator can be written as: $\widehat{\beta}_{OLS} = \beta + \frac{Cov(x,\varepsilon)}{Var(x)}$. In the particular case of the linear probability model, it is easy to show that, for given $Var(E(x|y))$, the bias is larger the smaller the variance of the endogenous variable.

$E(y|G_i)$ now varies a lot more.[34] In fact, the standard deviation of the (weighted) share of peers choosing economics is now equal to 0.09 while it was only to 0.01 if the groups include all peers encountered in the 7 common courses that we consider.

The estimated endogenous effect is still insignificant in the OLS specification and becomes statistically important in the IV estimation. The magnitude of these effects is also in line with previous findings: one additional (average) peer opting for economics raises the probability of making the same choice by approximately 0.7 percentage points.

[Table 9: Peer effects in the choice of major. Probit model]

These results are very robust to changes in the specification of the model. Table 9 reports the same estimates produced under a probit specification and shows that the endogenous effects are now slightly more significant and that the marginal effects computed at the average of the distribution of the right-hand-side variables are of about the same magnitude of the results of the linear models.

These estimates indicate the presence of strong and large endogenous peer effects and are obtained using instrumental variables that appear to be very significant in explaining the endogenous term.[35] The F-test of excluded instruments, reported at the bottom of Table 8, is always very large. Table A.2 in the appendix shows the complete first-stage regressions for all our IV specifications (note that the first-stage regressions are identical for both the linear and the probit models).

Theoretically we could have used a very large set of instruments (all the exogenous characteristics of the excluded peers), however, in order to maximize efficiency, we have selected a subset of the most powerful ones, i.e. admission test, high school final grade and preferences for economics. All these instruments are also singularly significant in the first-stage regressions at a very strong level, with t-statistics between 4 and 8.

[34] An argument similar to the one used to explain the difference between OLS and IV clarifies this point. In a simple model with just one regressor $\beta = \frac{Cov(x,y)}{Var(x)}$. However, when y is a dummy variable this becomes: $\beta = \frac{\overline{y}[E(x|y=1)-\overline{x}]}{Var(x)}$, which clearly shows that when $Var(x)$ increases only (or mostly) within groups defined by y the value of β declines.

[35] Although the analysis focus on a selective institution we have no reasons to believe that endogenous peer effects should be stronger in such setting.

As discussed in Section 4, by construction the exogenous characteristics of the excluded peers influence the outcomes of peers in the opposite direction as one own's characteristics. This explains why the first-stage coefficients of the instruments are negative, while the corresponding individual variables have a positive impact on individual outcomes. The only exception is the high school final grade which is, however, very highly correlated with the admission test result.

5 Robustness

Throughout the paper we relied on a number of more or less stringent assumptions. In this section we present a series of robustness checks to give a sense of whether a particular stand is central to the main results of the paper.

First of all, we repeat our estimates including the (weighted) average of all exogenous characteristics of peers in the set of regressors. We can do this only when the groups are based on the restricted definition of peers who sit at least 4 courses in the same classes otherwise the exogenous peer effects $E(\mathbf{x}|G_i)$ vary too little to be separately identified from the constant. As shown in row 2 of Tables A.4 (for linear models) and A.5 (for probit models), results are very similar to our baseline estimates. Moreover, none of the exogenous effects is significant in these regressions, given that their variation is generated only by random sampling differences.

The definition of peer groups that we have adopted throughout the paper is based on the assumption that students interact in the classroom and that this particular group (classmates) is relevant as far as the majoring decision is concerned. It is likely that students who meet regularly (if they share only one subject they meet at least 6 hours a week for a semester) are somehow bound to interact and influence each other.[36] However some students attend more than one subject together and, given that they spend more time in the same venue, on average they should also interact more.

We deal with this particular feature of the class assignment process by assigning a larger weight to students that meet more often. Throughout the paper we presented results based on either a very simple linear weighting or on a more extreme scheme that assigns weight zero to any peer that has

[36] Three 2-hour lectures per week.

been encountered in less than 4 courses. However, we have experimented a number of other schemes and in Tables A.4 and A.5 we present all the results on endogenous peer effects employing two alternative weighting schemes. First (in the third row of the tables), we exclude the courses in quantitative subjects (math and statistics) from our definition of peer groups, thus relying only on peers that attended courses in economics and business (5 courses in total) in the same classes. Additionally, in the fourth rows of Tables A.4 and A.5, we go back to considering all 7 courses but we adopt an exponential weighting scheme, which assigns to each peer j of student i a weight equal to the exponential of the number of courses that i and j have attended in the same classes, minus 1.

The estimated endogenous peer effects are highly comparable with our baseline specifications in Tables 8 and 9. The weighting does not seem to be central for identification. Moreover, the exponential (but also, although to a smaller extent, the restriction to only 5 courses) generates by construction a larger variation in the exogenous group effects - $E(\mathbf{x}|G_i)$ - and in fact it helps the identification of our parameter of interest, as indicated by the significance levels in Tables A.4 and A.5.

We have also performed the following thought experiment: assume that the peer groups we have defined have nothing to do with any type of social interaction and the effects that we estimate are generated by mere sample variation (or anything else). We then construct placebo peer groups by artificially and randomly assigning students to hypothetical classes. We expect to find no significant endogenous peer effect when the groups are formed using this artificial allocation. In fact, in none of the many specifications reported in the fifth rows of Tables A.4 and A.5 is there indication of significant social interactions and the magnitude of the point estimates is much closer to zero then in Tables 8 and 9 (when using the same definition of peers based on all 7 courses).

Our strategy for dealing with group specific shocks relies on the IV approach discussed in Section 4. As an alternative, we can also construct observable proxies for the plausibly most important group shock: teacher quality. From the student evaluation questionnaires we can identify for each of the 7 courses the "best" and the "worst" lecturers as those who received the highest and the lowest average mark on the item named *quality of teach-*

ing.[37] The results reported in Tables A.4 and A.5 (row 6) are obtained from models similar to those in Tables 8 and 9, where we have augmented the set of control variables with a dummy for each of the courses considered, which takes value 1 if the student attended the course in the class of the lecturer who obtained the best students' evaluation. The estimates are again in line with those of Tables 8 and 9, suggesting that either the teacher dummies are not fully capturing the effect of teaching quality or that other group shocks may also be important (e.g. disruptive behavior).

Finally, information from the students' questionnaires (see Table 4 and Table A.1) suggests that in some cases the actual allocation of students into the classes might have not been maintained. Several anecdotes tell that, especially for the most difficult courses, students tended to cluster in the class of the best teacher regardless of their officially assigned class. Our data provide some evidence in this direction. For example, from Table 4 we know that in mathematics class 12[38] the number of questionnaires collected on the day of the course evaluation (253) was almost 60% higher than the number of officially enrolled students (161).

To account for the possibility that students assigned to the same teaching class may actually attend a course in different classes, we adjust our weights by proportionally lowering the importance of peers encountered in courses where there are signals that the official allocation was not effectively maintained. We identify these particular courses by exploiting the following question from the students' questionnaire: *"For your learning, the number of students attending your class has been: insufficient (1), too low (2), ideal (3), too high (4), excessive (5)"*. Tables 4 and A.1 report the average score of this question - which we label *congestion* - across courses and for each single class, respectively.

Courses in which the random allocation is not maintained should be characterized by a large variation across classes in this measure of congestion, i.e. there should be some classes with very many students and others with very few students. We, then, construct course weights by assigning weight 1 to the course with the lowest maximum level of reported congestion across classes (i.e. 2.51 for Management II) and the weights of the other courses are scaled down accordingly (these weights are shown in the last column of

[37] Students are asked to give a synthetic evaluation on a scale 0 to 10.

[38] For anonymization purposes, this is a randomized version of the true class identifier.

Table 4). The peers of a generic student i are then assigned a weight equal to the sum of the course weights corresponding to the courses taken in the same classes as i (normalized to sum to 1 within groups).

The last rows of Tables A.4 and A.5 report the estimated endogenous peer effects under this particular weighting scheme and show that the results are very similar to our baseline, suggesting that, in fact, problems of congestion were limited to a few cases.

6 Are books better than company?

In this section we analyze the relationship between students academic performance in the second half of their degree (i.e. the non-common semesters) and how they chose their major, i.e. based more on their own revealed ability or on their peers' behavior.

To this purpose, we construct two indicators. The first one, f_i, measures the relative fraction of peers who made one's same choice of major. Suppose individual i chooses to specialize in economics, then f_i is computed as the ratio between the (weighted) fraction of i's peers who also chose economics and the fraction of all students in the sample who chose economics. If $f_i > 1$ it means that in i's peer group there is a higher than average incidence of students in economics. Similarly for students who chose business. More formally, f_i is defined as follows:

$$f_i = \begin{cases} \frac{\sum_{j \in G_i} \omega_j ECON_j}{N^{-1} \sum ECON_j} & \text{if} \quad ECON_i = 1 \\ \frac{\sum_{j \in G_i} \omega_j BUSINESS_j}{N^{-1} \sum BUSINESS_j} & \text{if} \quad BUSINESS_i = 1 \end{cases} \tag{5}$$

where $ECON_i$ is a dummy variable equal to 1 if student i chooses economics and zero otherwise (similarly for $BUSINESS_i$).

The second indicator, g_i, is a measure of relative ability. Our data include very detailed information on each exam, including the grade. We consider the nine common exams taken during the first three semesters and group them into areas - economics, business, quantitative and other - as described in Section 2. Suppose individual i chooses to specialize in economics, then g_i is computed as the ratio between i's average grade in the exams of the economics area over i's average grade in the exams of the business area. Similarly for students who chose business. We normalize also this measure by the relative performance of the full sample of students. Formally, g_i is defined as follows:

$$g_i = \begin{cases} \frac{GPA_i^{ECON}}{GPA_i^{BUSINESS}} \cdot \frac{\sum GPA_j^{BUSINESS}}{\sum GPA_j^{ECON}} \text{if} \quad ECON_i = 1 \\ \frac{GPA_i^{BUSINESS}}{GPA_i^{ECON}} \cdot \frac{\sum GPA_j^{ECON}}{\sum GPA_j^{BUSINESS}} \quad \text{if} \quad BUSINESS_i = 1 \end{cases} \tag{6}$$

where GPA_i^{ECON} is i's average grade in economics' exams and $GPA_i^{BUSINESS}$ is i's average grade in business' exams. If $g_i > 1$ it means that, during the first three semesters and compared to all other students, student i has performed better in the exams of the major she eventually chose as a specialization. Note that in constructing this indicator we only consider the common exams of the first three semesters, namely economics I and II for economics and management I, II and accounting for business. According to these indicators we define four groups of students. The first group, which we label *ability driven*, includes those students who chose the major subject in which they performed (relatively) better during the first three semesters against the (relative) majority of their peers, i.e. $f_i < 1$ and $g_i > 1$. The second group - the *peer driven* - are students who chose as the (relative) majority of their peers and against their (relative) revealed ability, i.e. $f_i > 1$ and $g_i < 1$. The third group - the *coherent* - includes those students who made a choice of major that is coherent with their performance as well as with their peers' behavior, i.e. $f_i > 1$ and $g_i > 1$. Finally, some students - the *incoherent* - chose against both their academic record and their peers, i.e. $f_i < 1$ and $g_i < 1$. Table 10 summarizes these definitions.

[Table 10: Distribution of decision modes]

As the table shows, students are rather evenly spread across the four groups. The largest (27.17%) is represented by the *ability driven*, i.e. students who choose against the relative majority of their peers and following the signal of their revealed performance. The *coherent*, i.e. students who choose both according to their ability and their peers, are only slightly less numerous (25.12%). *Peer driven* students, i.e. those who follow peers in contrast with the indication of their academic performance, represent 23.76% of the sample, leaving a sizeable 23.95% of students in the group of the *incoherent*, i.e. those who choose against both peers and revealed ability.

We use these groups to estimate the effect of these "decision modes" on three academic outcomes: average grade in the last two and a half years of the degree (i.e. after the major choice is made), graduation mark and time to graduation. A general specification of the equations that we estimate in this section is the following:

$$y_i = c + \pi_1[\text{peer driven}]_i + \pi_2[\text{coherent}]_i + \pi_3[\text{incoherent}]_i + \boldsymbol{\vartheta}\mathbf{x}_i + u_i. \quad (7)$$

where y is the outcome considered and the other variables are dummies that identify the groups (with the *ability driven* kept as a reference group). The set of controls - $\mathbf{x}_i$ - includes a gender dummy, household income (as recorded at first enrolment), a dummy for students who reside outside the city of Milan, a set of dummies for the region of origin, a series of controls for academic performance and ability (high-school grades and type, average grades in the common exams, a dummy for the specialization and the number of common exams taken on the first available session).

[Table 11: Decision modes and academic outcomes]

The results of this exercise are presented in Table 11. Columns 2 and 4 extend the specification for average grades in the non-common courses and graduation mark with time to graduation. In column 5, when we look at time to graduation, we replace the average grade in the common courses with the average grade in all courses. Notice that the maintained assumption is that, conditional on the observables, the four categories are independent from the outcome variable.[39]

Although the effect is small in magnitude, there is clear evidence that *peer driven* students on average perform worse than the *ability driven* in terms of both average and final grade, while there seems to be no detectable difference in time to graduation. We estimate a significant negative effect of -0.15 to -0.18 of a grade point on the average grade in non-common exams (exam grades are given on a scale from 0 to 30 with pass equal to 18) and of

[39] A basic version of a Conditional Independence Assumption (CIA), where selection is on observables and we can control for all those variables affecting both the decision mode and the outcomes considered.

-0.57 to -0.62 on the final grade (given on a scale 0 to 110 with pass equal to 66).

6.1 Labor market effects

In this section we try to assign a 'price' to the decision of following one's peers in contrast with one's revealed ability (i.e. being a *peer driven* student as opposed to an *ability driven*) in terms of entry wages. The ideal strategy would be one in which entry wages for the same students used for the estimation of equation (7) are regressed on the dummies for the decision modes, controlling for a set of individual characteristics.

Unfortunately, information on wages is only available in a dataset constructed by Bocconi university by interviewing almost all its graduates between one and one and a half years after graduation and these surveys currently cover only those who graduated between 2000 and 2003.[40] Only for about 1/3 of the observations used in the previous sections of this paper it is possible to recover information on labour market outcomes from these surveys and this is obviously a very selected group of early graduates.

For these reasons, we take a different approach and merge academic records with all available surveys of graduates to compute the penalty associated with a lower graduation mark for the whole sample of Bocconi students who graduated between 2000 and 2003. The data on labour market outcomes include information on monthly wages in the first job, the type of occupation and contract and a number of questions on satisfaction with the university.

[Table 12: Interval wage regressions]

In Table 12 we report the results of these estimates. In these regressions we are mostly interested in the coefficients on graduation mark but we also control for time to graduation and the entire set of ability measures and individual traits used throughout the paper. Moreover, since wages are recorded

[40] At the time of the surveys (i.e. approximately 1.5 years after graduation), several male students were on compulsory military service and others (both male and female) could not be reached.

in intervals the results in Table 12 are produced with interval regressions.[41]

The results show a sharp discontinuity at the top of the distribution of graduation marks. When this variable is introduced linearly (column 1) the estimated effect is relatively small: a one point increase in the final grade raises monthly wages by a mere 6 euros (8 USD) per month - i.e. about 78 euros (100 USD) per year. However, this effect is much bigger for students obtaining full marks (i.e. 110 with or without honors), who earn almost 67 euros (86 USD) per month (871 euros - 1,117 USD - per year) more than students who just fail to get full marks.[42]

Furthermore, if we were to consider a constant life-time loss of those amounts we would get, on average, a net present value loss of (roughly) 2,100 USD.[43] Unfortunately we cannot test whether the penalty of a *peer driven* decision is constant over time since no other information on later wages is available at this time.

7 Conclusions

In this paper we investigate whether peers' behavior has an important and significant effect on the choice of college major using a unique dataset from Bocconi University. The available data and the peculiar structure of the degree allow us to identify the endogenous effect of peers on this decision, circumventing the two crucial identification problems of studies of social interactions: endogeneity and reflection.

Our contribution to the literature is twofold. First, we solve the long-standing identification problems in the estimation of social interactions. Second, we estimate the importance of peers' actions on one's choice of major. Further, we assign a monetary value on choosing a major based more on peers' behavior than on one's revealed ability and find a small negative impact that becomes sizeable at the "full mark" threshold.

There can be many possible mechanisms generating the endogenous peer

[41] The same results have been produced with alternative econometric specifications (i.e. linear OLS on the mid-points of the intervals, quantile regression, ordered probit) and the magnitude and significance of the estimated effects are extremely robust.

[42] These results are broadly consistent with similar estimates produced on a different data source, i.e. the Bank of Italy Survey of Household Incomes and Wealth.

[43] The net present value (NPV) has been computed by assuming a constant interest rate of 5 percent and a life-time of 40 years. For those students at the margin of getting full marks the NPV loss would be a quite large (roughly) 23,500 USD.

effects. On the one hand, if students follow the choices of their peers simply because there is a utility gain in studying together, one may interpret the wage effect estimated in the last section as the monetary value of such utility advantage. Alternatively, if it is peer pressure or imitation that generates peer effects (Mas and Moretti, 2006), than this wage loss can effectively be interpreted as the cost of decisions that are not based exclusively on efficiency considerations. Finally, in the introduction we also suggested that peers may represent a source of useful information about some hard-to-see features of university life and/or major choice (where to find the right material to study, which are the best or the easiest courses, the best teachers, etc.). Our estimates of the labor market effects suggest that this is unlikely to be the mechanism that generates peer effects in our study. Better informed individuals should in principle make better choices but the wage penalty associated with the peer driven students is in contrast with this interpretation, unless peers deliver incorrect information. It should also be noted that any combination of these explanations (and possibly others) may actually be at the origin of the effects that we estimate.

Having convincingly shown the existence of pure endogenous peer effects, as in this paper, understanding the exact mechanism that underlies social interactions is perhaps the next big open question in this branch of the literature.

References

[1] Akerlof, G., (1997), “Social distance and social decisions”, Econometrica, 65 1005-1027

[2] Altonji, J.G., Elder, T.E., Taber, C.R., (2005), "Selection on Observed and Unobserved Variables: Assessing the Effectiveness of Catholic Schools", Journal of Political Economy, 113 (1): 151-183.

[3] Ammermueller, A. and Pischke J.S., (2006), "Peer Effects in European Primary Schools: Evidecne from PIRLS", IZA Discussion Paper n. 2077.

[4] Bayer, P., Pintoff, R. and Pozen, D., (2004), “Building Criminal Capital Behind Bars: Peer Effects in Juvenile Corrections”, mimeo, Duke University

[5] Bayer, P., Ross, S. and Topa, G., (2005), “Place of work and place of residence: Informal hiring networks and labor market outcomes”, NBER working paper series no.11019

[6] Bobonis, G. and Finan, F. (2005), “Endogenous Social Interaction Effects in School Participation in Rural Mexico”, mimeo, University of California Berkeley.

[7] Bramoullé, Y., Djebbari, H. and Fortin, B., (2006), “Identification of Peer Effects through Social Networks”, mimeo, University of Laval.

[8] Brock, W. and Durlauf, S., (2001), “Interaction-based Models”, Handbook of Econometrics, vol. 5, J. Heckman and Leamer E. (Eds), Amsterdam: North-Holland.

[9] Brock, W. and Durlauf, S., (2004). Identification of binary choice models with social inter- actions. Working Paper.

[10] Calvó-Armengol, A. and M. O. Jackson (2004). The effects of social networks on employment and inequality. The American Economic Review 94 (3), 426.

[11] Calvó-Armengol, A., Patacchini, E. and Zenou, Y., (2006), “Peer Effects and Social Networks in Education”, WP, Research Institute of Industrial Economics.

[12] Case, A. and Katz, L., (1991), "The Company You Keep: The Effects of Family and Neighborhood on Disadvantaged Youths", NBER Working Paper: 3705.

[13] Cipollone, P. and Rosolia, A., (2006), "Social Interactions in High School: Lessons from an Earthquake", American Economic Review, forthcoming.

[14] Cooley, J., (2006), "Desegregation and the Achievement Gap: Do Diverse Peers Help", mimeo, Univesrity of Wisconsin-Madison.

[15] Duflo, E. and Saez, E. (2002), "The Role of Information and Social Interactions in Retirement Plan Decisions: Evidence from a Randomized Experiment," unpublished working paper, MIT and University of California, Berkeley.

[16] Falk, A. and Ichino A., (2006), "Clean Evidence on Peer Effects", Journal of Labor Economics, 24(1): 39-57.

[17] Garibaldi, P., Giavazzi, F., Ichino, A. and Rettore, E., (2007), "College cost and time to complete a degree: Evidence from tuition discontinuities", mimeo.

[18] Graham, B., (2006), Identifying Social Interactions through Conditional Variance Restrictions, mimeo, UC Berkely.

[19] Graham, B. and Hahn, J., (2005), Identification and Estimation of the Linear-in-Means Model of Social Interactions, Economic Letters 88 (1): 1 - 6

[20] Hanushek, E., Kain, J., Markman, J. and Rivkin, S., (2003), "Does Peer Ability Affect Student Achievement?", Journal of Applied Econometrics 18(5): 527-544.

[21] Hoxby, C., (2000), "Peer Effects in the Classroom: Learning from Gender and Race Variation", NBER Working Paper: 7867.

[22] Ichino, A. and Maggi, G., (2000), "Work Environment and Individual Background: Explaining Regional Shirking Differentials in a Large Italian Firm", Quarterly Journal of Economics, August, 115 (3), 1057-1090.

[23] Kling, J., Liebman, J. and Katz, L., (2005), "Experimental analysis of neighborhood effects", NBER Working Paper No. 11577

[24] Lalive, R. and Cattaneo, A., (2005), "Social Interactions and Schooling Decisions", Mimeo, University of Zurich.

[25] Laschever, R., (2005), "The doughboys network: social interactions and labor market outcomes. of World War I veterans", Unpublished manuscript

[26] Manski, C., (1993), "Identification of Endogenous Social Effects: The Reflection Problem", Review of Economic Studies, 60: 531-542.

[27] Mas, A. and Moretti, E., (2006), "Peers at Work", IZA Discussion Paper n. 2292.

[28] Miguel, E. and Kremer, M. (2004),"Worms: identifying impacts on education and health in the presence of treatment externalities," Econometrica, 72(1): 159-217.

[29] Moffitt, R., (2001), "Policy Interventions, Low-Level Equilibria, and Social Interactions", in Social Dynamics, S. Durlauf and H. P. Young eds., Cambridge: MIT Press.

[30] Sacerdote, B. (2001), "Peer Effects with Random Assignment: Results for Dartmouth Roommates," Quarterly Journal of Economics, 116: 681-704.

[31] Stutzer, A. and R. Lalive (forthcoming). The role of social work in job searching and subjective well-being. Journal of the European Economic Association.

[32] Soetevent, A. (2006), "Empirics of The Identification of Social Interactions: An Evaluation of the Approaches and their Results", Journal of Economic Surveys, 20(2): 193-228.

[33] Todd, P. and Wolpin, K., (2006), "Assessing the Impact of a School Subsidy Program in Mexico: Using a Social Experiment to Validate a Dynamic Behavioral Model of Child Schooling and Fertility", American Economic Review, 96(5):

[34] Zimmerman, D., (2003), "Peer Effects in Higher Education: Evidence from a Natural Experiment, Williams Project on the Economics of Higher Education", Review of Economics and Statistics, 85(1): 9-23.

Figure 1: Degree structure

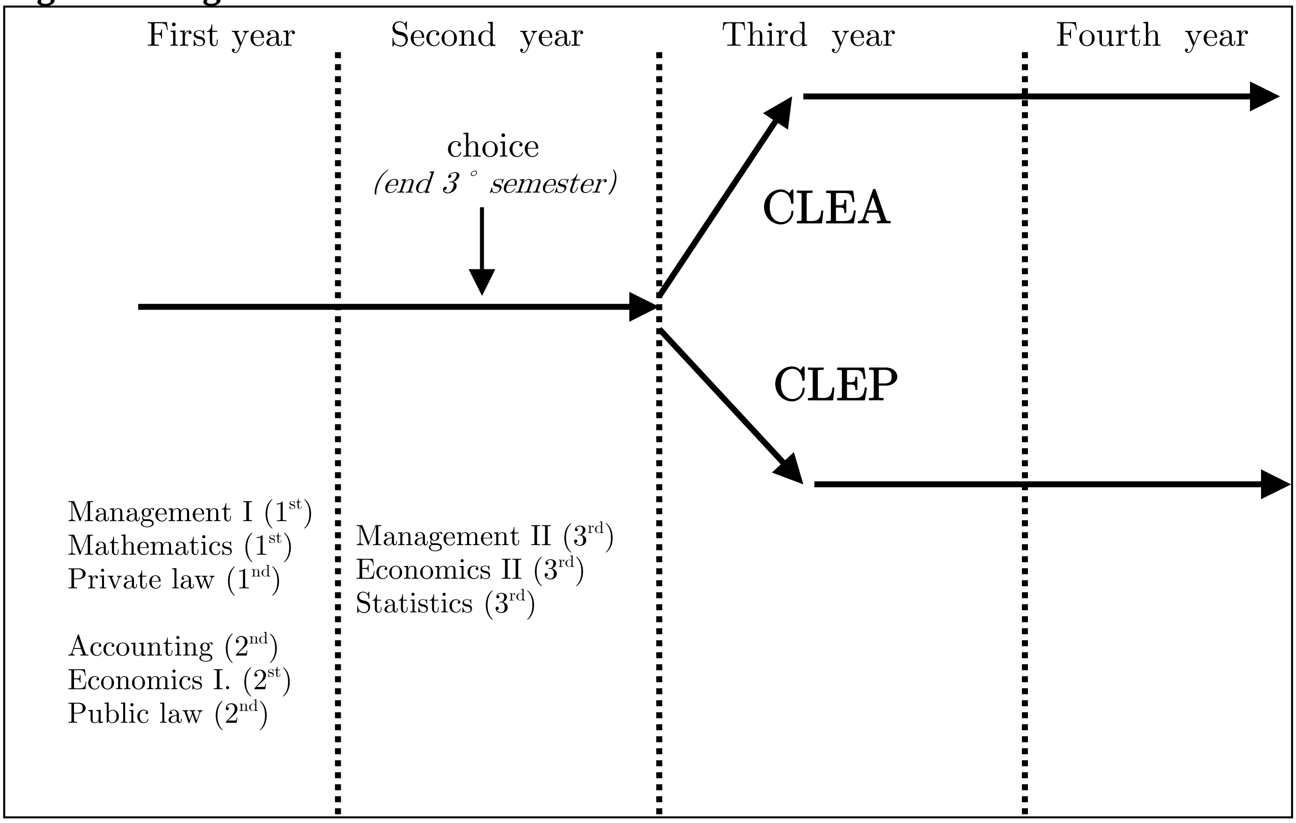

Table 1: Common exams CLEA/CLEP

	Semester	Area
Management I	1st	Business
Mathematics	1st	Quantitative
Private Law	1st	Law
Accounting	2nd	Business
Economics I	2nd	Economics
Public Law	2nd	Law
Economics II	3rd	Economics
Management II	3rd	Business
Statistics	3rd	Quantitative

Table 2: Descriptive statistics:

Variable	Mean	(s.d.)	min	max	Obs.
Individual characteristics					
1=CLEP	0.127	-	0	1	1141
1=female	0.396	-	0	1	1141
(log) household income[1]	7.91	(4.44)	0	11.7	1141
highest income braket[1]	0.227	(0.419)	0	1	1141
1=non-resident[2]	0.633	-	0	1	1141
1=determined economics[3]	0.15	-	0	1	1141
Academic measures					
Graduation mark[4]	102	(7.7)	76	111	1027
time to graduation (in years)[5]	5.34	(0.661)	4	7	1027
av. grade in all exams	26.2	(2.05)	20	30	1141
av. grade in common exams	24.8	(2.29)	19	30.3	1141
av. grade in quatitative common exams	23.7	(3.09)	18	31	1141
av. grade in economics common exams	24.7	(2.94)	18	31	1141
av. grade in business common exams	25.6	(2.49)	18	31	1141
admission test[6]	69.1	(7.42)	43	91	1141
high school final grade[7]	86.3	(11.2)	60	100	1141

Notes:
1. If a student declares that household income falls in the highest income bracket no further information is collected therefore household income is coded to 1 for households in the last bracket and an ad-hoc dummy controls for this group.
2. Resident outside the province of Milan.
3. DES as first or second preferred course in admission test courses' ranking
4. Range 0-111 (pass = 60).
5. Official duration is 4 years.
6. Normalised between 0 and 100.
7. Normalised between 0 and 100 (pass = 60).

Table 3: Characteristics of CLEA/CLEP students.

	Obs.	AVERAGE GRADE COMMON EXAMS Area Business	Area Economics	Area Quantitative	Total	High School final grade	Admission Test final score
Total	1141	25.63	24.69	23.67	24.83	86.3	69.06
CLEP	145	26.82	26.79	25.81	26.52	92.2	72.48
CLEA	996	25.48	24.39	23.35	24.59	85.4	68.57
Difference (CLEP-CLEA)		1.36***	2.40***	2.46***	1.94***	6.79***	3.91***

Table 4: Characteristics of courses and lecturing classes:

	Semester	Number of classes	Characteristics	Average	coeff. of variation	Min	Max	Weight[3]
Management I	I	10	Enrolled students	140.40	0.11	130	169	0.70
			Student questionnaires	80.70	0.17	62	109	
			Average attendance[1] (%)	85.67	0.01	84.08	87.24	
			Congestion[2] (1 to 5)	3.33	0.05	3.16	3.61	
Mathematics	I	10	Enrolled students	140.80	0.12	125	164	0.55
			Student questionnaires	102.80	0.62	28	253	
			Average attendance[1] (%)	83.89	0.02	81.39	86.51	
			Congestion[2] (1 to 5)	3.77	0.14	3.00	4.57	
Private Law	I	4	Enrolled students	351.75	0.47	189	510	0.78
			Student questionnaires	70.00	0.39	38	104	
			Average attendance[1] (%)	79.73	0.06	74.91	83.89	
			Congestion[2] (1 to 5)	3.07	0.04	2.95	3.23	
Accounting	II	10	Enrolled students	142.80	0.33	109	258	0.57
			Student questionnaires	100.30	0.61	54	215	
			Average attendance[1] (%)	84.80	0.01	82.26	86.58	
			Congestion[2] (1 to 5)	3.46	0.14	3.02	4.40	
Economics I	II	6	Enrolled students	216.50	0.43	85	316	0.52
			Student questionnaires	136.83	0.76	24	317	
			Average attendance[1] (%)	84.92	0.01	83.56	86.84	
			Congestion[2] (1 to 5)	3.63	0.20	2.83	4.82	
Public Law	II	4	Enrolled students	351.75	0.42	217	528	0.83
			Student questionnaires	41.00	0.49	15	64	
			Average attendance[1] (%)	82.72	0.03	79.45	85.62	
			Congestion[2] (1 to 5)	2.89	0.06	2.67	3.03	
Economics II	III	6	Enrolled students	222.83	0.45	156	381	0.67
			Student questionnaires	109.17	0.48	19	176	
			Average attendance[1] (%)	83.87	0.02	81.42	86.80	
			Congestion[2] (1 to 5)	2.96	0.16	2.47	3.72	
Management II	III	8	Enrolled students	184.25	0.56	123	382	1.00
			Student questionnaires	80.75	0.32	56	125	
			Average attendance[1] (%)	84.38	0.01	83.38	85.27	
			Congestion[2] (1 to 5)	2.14	0.12	1.76	2.51	
Statistics	III	8	Enrolled students	272.25	0.33	142	404	0.56
			Student questionnaires	140.75	0.42	35	203	
			Average attendance[1] (%)	85.66	0.01	83.31	86.53	
			Congestion[2] (1 to 5)	3.27	0.29	2.09	4.46	

Notes:

1. Self reported by the students.
2. Congestion is defined from students evaluations as the average answer given to the following question: *"For your learning, the number of students attending your class has been: insufficient (1), too low (2), ideal (3), too high (4), excessive (5)"*.
3. Weight A is the ratio between the lowest maximum level of congestion (i.e. 2.51 for Managemetn II) and the maximum level of congestion across the classes of each course.

Table 5: Size of peer groups, various definitions.

		All peers[1] [1]	Restricted peers[2] [2]	Excluded peers[3] [3]
Raw group size	*Mean*	674.47	18.08	252.53
	Std. dev.	(79.10)	(6.77)	(60.96)
Average number of courses taken together	*Mean*	1.57	4.16	0.00
	Std. dev.	(0.06)	(0.11)	(0.00)
Weighted group size	*Mean*	151.07	10.77	--
	Std. dev.	(19.73)	(4.08)	--

1. Students who have been assigned to the same lecturing class at least once over the 7 common courses considered.
2. Students who have been assigned to the same lecturing class in at least 4 of the 7 common courses considered.
3. Students who have never been assigned to the same lecturing class in any of the 9 common courses but who have attended some of the 7 courses considered who at least one peer student.

Table 6: Peers and later academic patterns

Definition of peers: number of courses attended in the same class	**at least 1** [1]	**at least 2** [2]	**at least 3** [3]	**at lest 4** [4]
Panel A: Percentage of students who choose the same sub-major				
Peers	9.645	9.685	9.908	9.633
Non-peers	9.603			
Diff.	*0.042*	*0.082**	*0.306****	*0.030*
Panel B: Percentage of students who graduate in the same session				
Peers	13.438	13.890	16.346	22.418
Non-peers	12.523			
Diff.	*0.915****	*1.367****	*3.823****	*9.895****

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 7: Correlation between individual and group level predetermined variables

	All peers [1]	Restricted peers [2]
Panel A: correlation between individual and peer attributes:		
Admission test score	0.0052	0.0236
High school final grade	-0.0325	-0.0701
Determined economics	0.0181	0.0169
Panel B: correlation between individual and excluded peers' attributes:		
Admission test score	-0.0474	-0.0488
High school final grade	-0.0192	-0.0169
Determined economics	-0.0050	-0.0079
Panel C: correlation between peers' and excluded peers' attributes:		
Admission test score	-0.5054***	-0.1088***
High school final grade	-0.4113***	-0.0655***
Determined economics	-0.4931***	-0.0785***

Table 8: Peer effects in the choice of major. Linear probability model

Dependent variable: probability of choosing CLEP	All peers			Restricted peers		
	OLS	2SLS[1]	2SLS[1] weighted	OLS	2SLS[1]	2SLS[1] weighted
	[1]	[2]	[3]	[4]	[5]	[6]
Fraction Peers choosing CLEP	1.000	5.785*	5.289*	0.150	1.260*	1.242*
	(0.768)	(3.171)	(2.899)	(0.105)	(0.698)	(0.643)
	[0.193]	[0.068]	[0.068]	[0.155]	[0.071]	[0.053]
Individual characteristics						
Admission test[2]	0.005**	0.005***	0.005***	0.005***	0.005***	0.005***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
High school final grade[3]	0.004***	0.004***	0.004***	0.424***	0.410***	0.416***
	(0.001)	(0.001)	(0.001)	(0.102)	(0.106)	(0.106)
1=determined economics	0.095**	0.089**	0.089**	0.098***	0.112***	0.112***
	(0.031)	(0.032)	(0.031)	(0.031)	(0.034)	(0.033)
1=female	-0.011	-0.012	-0.012	-0.012	-0.016	-0.016
	(0.020)	(0.020)	(0.020)	(0.020)	(0.021)	(0.021)
Log household income[4]	0.005	0.006	0.006	0.005	0.006	0.006
	(0.005)	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)
1=highest income bracket[4]	0.044	0.054	0.053	0.043	0.058	0.057
	(0.064)	(0.065)	(0.064)	(0.065)	(0.069)	(0.069)
1=non resident[5]	0.001	-0.003	-0.002	-0.000	-0.012	-0.012
	(0.025)	(0.026)	(0.026)	(0.025)	(0.028)	(0.027)
High school type dummies	Yes	Yes	Yes	Yes	Yes	Yes
Region of residence dummies	Yes	Yes	Yes	Yes	Yes	Yes
Nr. Obs.	1141	1141	1141	1141	1141	1141
R^2	0.12	0.09	0.09	0.12	0.16	0.16
Shea Partial R^2	--	0.0603	0.0708	--	0.0242	0.0304
1st stage F-test	--	25.46	30.30	--	8.49	12.17

Note:
1. Excluded instruments: averages of admission test, high school final grade, determined to do economics in the group of excluded peers who are not in one's peer group.
2. Normalised between 0 and 100. Average in the sample = 69.10
3. Normalised between 0 and 100 (pass = 60). Average in the sample = 86.3
4. If a student declares that household income falls in the highest income bracket no further information is collected therefore household income is coded to 1 for households in the last bracket and an ad-hoc dummy controls for this group.
5. Resident outside the province of Milan.
Robust standard errors in parentheses: p-values in square brackets.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 9: Peer effects in the choice of major. Probit model

Dependent variable: Probability of choosing CLEP	All peers			Restricted peers		
	Probit	**IVProbit**	**IVProbit weighted**	**Probit**	**IVProbit**	**IVProbit weighted**
	[1]	[2]	[3]	[4]	[5]	[6]
Fraction Peers choosing CLEP	5.30	32.747**	30.873**	0.756	6.402***	6.059***
	(4.120)	(14.134)	(13.308)	(0.548)	(2.404)	(2.257)
	[0.198]	[0.021]	[0.020]	[0.167]	[0.008]	[0.007]
marginal effects	0.905	6.154	5.723	0.129	1.351	1.248
Individual characteristics						
Admission test[2]	0.022**	0.023***	0.023***	0.022***	-0.000	-0.000
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
High school final grade[3]	0.028***	0.025***	0.026***	2.748***	0.014	0.011
	(0.006)	(0.006)	(0.006)	(0.637)	(0.697)	(0.673)
1=determined economics	0.437***	0.377**	0.384**	0.455***	-0.013*	0.473***
	(0.127)	(0.132)	(0.131)	(0.127)	(0.123)	(0.124)
1=female	-0.069	-0.068	-0.069	-0.072	0.003	0.004
	(0.110)	(0.106)	(0.106)	(0.111)	(0.102)	(0.103)
Log household income[4]	0.044	0.042	0.042	0.044	-0.001	0.038
	(0.041)	(0.038)	(0.039)	(0.042)	(0.036)	(0.037)
1=highest income bracket[4]	0.418	0.416	0.413	0.423	-0.016	-0.016
	(0.489)	(0.454)	(0.456)	(0.489)	(0.429)	(0.436)
1=non resident[5]	0.044	0.019	0.021	0.042	0.008	0.008
	(0.161)	(0.157)	(0.158)	(0.162)	(0.152)	(0.153)
High school type dummies	Yes	Yes	Yes	Yes	Yes	Yes
Region of residence dummies	Yes	Yes	Yes	Yes	Yes	Yes
Nr. Obs.	1124	1124	1124	1124	1124	1124
Pseudo R^2	0.146	--	--	0.146	--	--

Note:
1. Excluded instruments: averages of admission test, high school final grade, determined to do economics in the group of excluded peers who are not in one's peer group.
2. Normalised between 0 and 100. Average in the sample = 69.10
3. Normalised between 0 and 100 (pass = 60). Average in the sample = 86.3
4. If a student declares that household income falls in the highest income bracket no further information is collected therefore household income is coded to 1 for households in the last bracket and an ad-hoc dummy controls for this group.
5. Resident outside the province of Milan.
Robust standard errors in parentheses: p-values in square brackets.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 10: Distribution of decision modes

		ABILITY INFLUENCE	
		YES (g>1)	NO (g<1)
PEERS' INFLUENCE	YES (f>1)	Coherent 25.12%	Peer driven 23.76%
	NO (f<1)	Ability driven 27.17%	Incoherent 23.95%

Table 11: Decision modes and academic outcomes

Dependent variable:	Av. Grade in non-common exams[1]		Graduation mark[2]		Time to graduation[3] (in years)
	[1]	[2]	[3]	[4]	[5]
Decision mode					
Peer driven	-0.170*	-0.154*	-0.618**	-0.575**	0.025
	(0.056)	(0.073)	(0.036)	(0.045)	(0.623)
Coherent	-0.023	-0.060	-0.049	-0.145	-0.080
	(0.794)	(0.479)	(0.867)	(0.614)	(0.122)
Incoherent	-0.205**	-0.146*	-0.565*	-0.409	0.112**
	(0.024)	(0.097)	(0.058)	(0.161)	(0.042)
Ability measures					
Av. grade all exams	--	--	--	--	-0.167***
					(0.000)
Av. grade common exams	0.644***	0.595***	2.877***	2.751***	--
	(0.000)	(0.000)	(0.000)	(0.000)	
Time to graduation	--	-0.479***	--	-1.252***	--
		(0.000)		(0.000)	
1=CLEP	-0.262***	-0.216**	-0.201	-0.083	0.087
	(0.003)	(0.012)	(0.529)	(0.798)	(0.119)
Admission test[4]	0.006	0.005	-0.021	-0.022	0.002
	(0.228)	(0.247)	(0.200)	(0.167)	(0.583)
High school final grade[5]	0.025***	0.024***	0.075***	0.072***	0.021
	(0.000)	(0.000)	(0.000)	(0.000)	(0.242)
High school type dummies	yes	yes	yes	yes	yes
Individual characteristics					
1=female	0.341***	0.267***	1.046***	0.853***	-0.113***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.003)
Household income[6]	-0.011	-0.017	-0.052	-0.067	-0.013
	(0.730)	(0.543)	(0.634)	(0.494)	(0.385)
1=highest income bracket[6]	-0.030	-0.164	-0.414	-0.763	-0.284
	(0.935)	(0.611)	(0.746)	(0.508)	(0.120)
1=non resident[7]	0.086	0.089	0.271	0.280	0.023
	(0.194)	(0.155)	(0.219)	(0.188)	(0.691)
Region of residence dummies	yes	yes	yes	yes	yes
Nr. Observations	1027	1027	1027	1027	1027
R-squared	0.513	0.529	0.566	0.573	0.163

Note:
1. Range 0-30 (18 = pass). Average in the sample = 26.97
2. Range 0-111 (pass = 60). Average in the sample = 102.11
3. Official duration is 4 years. Average in the sample = 5.34
4. Normalised between 0 and 100. Average in the sample = 69.10
5. Normalised between 0 and 100 (pass = 60). Average in the sample = 86.3
6. If a student declares that household income falls in the highest income bracket no further information is collected therefore household income is coded to 1 for households in the last bracket and an ad-hoc dummy controls for this group.
7. Resident outside the province of Milan.
Robust standard errors in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 12: Interval wage regressions

Dependent variable: wage in the first job[1]	[1]	[2]
graduation mark[2]	6.045***	3.718**
	(1.360)	(1.612)
1=full marks[3]		66.881***
		(25.013)
time to graduation[4]	-2.450*	-2.279
	(1.443)	(1.443)
1=female	-97.039***	-94.362***
	(17.360)	(17.368)
Household income[5]	-0.000	-0.000
	(0.000)	(0.000)
1=highest income bracket[5]	-2.276	-3.612
	(26.449)	(26.428)
1=post-graduate studies	-19.498	-18.686
	(19.099)	(19.078)
High school final grade[6]	-1.093	-1.214
	(0.893)	(0.894)
High school type dummies	yes	yes
Degree programme dummies	yes	yes
Contract type dummies	yes	yes
Nr. observations	3982	3982

Note:
1. Recorded in intervals
2. Range 0-111 (pass = 66).
3. 110 with or without honours
4. Recorded in quarters. Official duration is 4 years.
5. If a student declares that household income falls in the highest income bracket no further information is collected therefore household income is coded to -1 for households in the last bracket and an ad-hoc dummy controls for this group.
6. Normalised between 0 and 1 (pass = 0.6).

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Table A.2: IV First-stage regressions

Dependent variable: fraction of peers choosing CLEP	All peers [1]	All peers weighted [2]	Restricted peers [3]	Restricted peers weighted [4]
Instruments: Excluded peers'				
Admission test[2]	-0.348***	-0.357***	0.021***	0.023***
	(0.075)	(0.074)	(0.007)	(0.006)
High school final grade[3]	0.000***	0.000***	-0.333	-0.697
	(0.000)	(0.000)	(0.567)	(0.487)
Fraction of determined economics	-0.132***	-0.140***	-0.617***	-0.463***
	(0.018)	(0.018)	(0.136)	(0.106)
Individual characteristics				
Admission test[2]	-0.000*	-0.000*	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
High school final grade[3]	0.000	0.000	0.012	0.009
	(0.000)	(0.000)	(0.028)	(0.028)
1=determined economics	0.001	0.000	-0.012*	-0.013*
	(0.001)	(0.001)	(0.007)	(0.007)
1=female	-0.000	0.000	0.003	0.003
	(0.001)	(0.000)	(0.006)	(0.005)
Log household income[4]	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.002)	(0.002)
1=highest income bracket[4]	-0.003	-0.003	-0.014	-0.015
	(0.004)	(0.004)	(0.022)	(0.022)
1=non resident[5]	0.000	0.000	0.008	0.008
	(0.001)	(0.001)	(0.008)	(0.008)
High school type dummies	Yes	Yes	Yes	Yes
Region of residence dummies	Yes	Yes	Yes	Yes
Nr. Obs.	1141	1141	1141	1141

Note:
1. Excluded instruments: averages of admission test, high school final grade, determined to do economics in the group of excluded peers who are not in one's peer group.
2. Normalised between 0 and 100. Average in the sample = 69.10
3. Normalised between 0 and 100 (pass = 60). Average in the sample = 86.3
4. If a student declares that household income falls in the highest income bracket no further information is collected therefore household income is coded to 1 for households in the last bracket and an ad-hoc dummy controls for this group.
5. Resident outside the province of Milan.
Robust standard errors in parentheses: p-values in square brackets.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table A.3: Selected descriptive statistics of individual and peers' characteristics

Variable		Individual	All peers	Restricted peers	Excluded peers (all)	Excluded peers (restricted)
1=CLEP	*Mean*	0.127	0.127	0.128	0.123	0.123
	Std. dev	(0.333)	(0.012)	(0.088)	(0.019)	(0.019)
Determined economics	*Mean*	0.147	0.146	0.146	0.144	0.144
	Std. dev	(0.354)	(0.128)	(0.094)	(0.019)	(0.019)
Admission test[1]	*Mean*	69.1	69.1	69.1	69.1	69.0
	Std. dev	(7.4)	(0.3)	(1.9)	(0.5)	(0.5)
High school final grade[2]	*Mean*	86.3	86.4	86.4	86.3	86.3
	Std. dev	(11.2)	(0.3)	(2.7)	(0.5)	(0.5)

Table A.4: Robustness checks (Linear probability models)

	All peers			Restricted peers		
	OLS	**2SLS**	**2SLS weighted**	**OLS**	**2SLS**	**2SLS weighted**
	[1]	[2]	[3]	[4]	[5]	[6]
1. Baseline	1.000	5.785*	5.289*	0.150	1.260*	1.242*
	(0.768)	(3.171)	(2.899)	(0.105)	(0.698)	(0.643)
	[0.193]	[0.068]	[0.068]	[0.155]	[0.071]	[0.053]
2. Exogenous effects	--	--	--	0.143	1.317*	1.346*
				(0.107)	(0.746)	(0.699)
				[0.184]	[0.077]	[0.054]
3. Groups based on 5 exams	1.372**	4.379*	3.930	0.215	1.648**	1.643*
	(0.661)	(2.621)	(2.398)	(0.133)	(0.828)	(0.841)
	[0.038]	[0.095]	[0.101]	[0.107]	[0.047]	[0.051]
4. Exponential weights	0.516*	3.137**	--	0.113	1.247*	--
	(0.296)	(1.588)		(0.093)	(0.710)	
	[0.081]	[0.048]		[0.223]	[0.079]	
5. Placebo peer groups	0.822	0.466	0.149	-0.081	0.440	-0.287
	(0.864)	(4.646)	(4.722)	(0.058)	(0.674)	(0.794)
	[0.342]	[0.920]	[0.975]	[0.163]	[0.513]	[0.718]
6. Teacher quality controls	0.505	5.725*	5.231*	0.099	1.207*	1.109*
	(0.807)	(3.400)	(3.134)	(0.107)	(0.727)	(0.652)
	[0.531]	[0.092]	[0.095]	[0.357]	[0.097]	[0.089]
7. Course congestion	0.505	5.725*	5.231*	0.154	1.248*	1.234*
	(0.807)	(3.400)	(3.134)	(0.105)	(0.687)	(0.640)
	[0.531]	[0.092]	[0.095]	[0.145]	[0.069]	[0.053]

Robust standard errors in parentheses: p-values in square brackets.

Table A.5: Robustness checks (Probit models)

	All peers			Restricted peers		
	Probit	IVProbit	IVProbit weighted	Probit	IVProbit	IVProbit weighted
	[1]	[2]	[3]	[4]	[5]	[6]
1. Baseline	5.30	32.747**	30.873**	0.756	6.402***	6.059***
	(4.120)	(14.134)	(13.308)	(0.548)	(2.404)	(2.257)
	[0.198]	[0.021]	[0.020]	[0.167]	[0.008]	[0.007]
marginal effect	0.905	6.154	5.723	0.129	1.351	1.248
2. Exogenous effects	--	--	--	0.823	6.707***	6.594***
				(0.569)	(2.610)	(2.380)
				[0.148]	[0.010]	[0.006]
marginal effects				0.140	1.405	1.370
3. Groups based on 5 exams	7.224**	26.110**	24.053**	1.074	8.227***	8.413***
	(3.486)	(12.587)	(11.947)	(0.698)	(2.707)	(2.686)
	[0.038]	[0.038]	[0.044]	[0.124]	[0.002]	[0.002]
marginal effects	1.226	4.732	4.297	0.183	1.762	1.823
4. Exponential weights	2.937*	16.636***	--	0.610	6.206***	--
	(1.535)	(6.202)		(0.481)	(2.197)	
	[0.056]	[0.007]		[0.205]	[0.005]	
marginal effect	0.499	3.290		0.104	1.376	
5. Placebo peer groups	5.267	4.314	2.262	-0.459	2.755	-2.015
	(4.423)	(27.703)	(28.192)	(0.334)	(4.497)	(5.127)
	[0.234]	[0.876]	[0.936]	[0.170]	[0.540]	[0.694]
marginal effect	0.898	0.735	0.386	-0.078	0.576	-0.361
6. Teacher quality controls	2.565	33.423**	31.629**	0.497	6.297**	5.691**
	(4.453)	(14.904)	(14.023)	(0.562)	(2.564)	(2.397)
	[0.565]	[0.025]	[0.024]	[0.377]	[0.014]	[0.018]
marginal effect	0.428	6.232	5.819	0.083	1.308	1.132
7. Course congestion	4.988	31.201**	28.619**	0.778	6.330***	6.035***
	(4.113)	(13.620)	(12.833)	(0.549)	(2.391)	(2.253)
	[0.225]	[0.022]	[0.026]	[0.156]	[0.008]	[0.007]
marginal effect	0.851	5.834	5.255	0.133	1.328	1.240

Robust standard errors in parentheses: p-values in square brackets.

Chapter 3

Sibling Rivalry and Early Marriage: Evidence from Rural Malawi

Silvia Redaelli

1 Introduction

The cross cutting goal promoting gender equality and empowerment of women in many developing countries is motivated by the existence of stark differences in the access to assets and opportunities between men and women and by the impact that such inequalities have on women's well-being, their families and their communities.

Gender inequalities are often sustained and promoted by cultural norms and institutions. A relatively overlooked manifestation of such a bias is the institution of early marriage. Conventionally, early marriage refers to any form of marriage that takes place before a child has reached 18 years. [1]

The incidence of early marriage is particularly severe in developing countries. Among women in the 25-49 age group, the median age at first marriage is 17.4 and 18.5 years, respectively, for Sub Saharan and for Southern-Southeastern Asian countries[2]. According to the latest World Marriage Pattern (UN, 2000), the disparity in marriage timing for males and females is broader in Africa than elsewhere. Of the 20 countries with the lowest mean age at marriage for females, 15 are in Africa while the rest are in Asia.

The practice of female early marriage can hinder several economic outcomes and prejudicate the achievement of development targets.

Early marriage significantly reduces women's educational outcomes (Field and Ambrus, 2005). Furthermore, being often associated with a higher spousal age gap, it tends to reduce women's bargaining power within the household which makes women more exposed to domestic violence (Tauchen et al., 1991) and potentially reduce future generations' outcomes through the allocation of intrahousehold resources (Thomas, 1990; Hoddinott and Haddad, 1995). Furthermore, girls who marry as adolescents tend to have higher fertility and maternal mortality rates (Jensen and Thornton, 2003; Westoff, 1992). Associated with having relatively older husbands, female early marriage also translates into a greater risk of HIV infection (Bruce and Clark, 2004; Bracker et al., 2003).

The goal of the analysis is to understand how marriage institutions affect the age at which women get married.

In particular, using data from Malawi, I investigate how dowry and bride-

[1] The African Charter on the Rights and Welfare of the Child (1990), Art. 21(2), states that "child marriage and the betrothal of girls and boys shall be prohibited" and sets the minimum age for marriage at 18.

The Committee monitoring the Convention on the Elimination of All Forms of Discrimination against Women (CEDAW Committee) has also recommended that the minimum age for marriage of both men and women should be 18, commenting that, "[when] men and women marry, they assume important responsibilities. Consequently, marriage should not be permitted before they have attained full maturity and capacity to act."

http://www1.umn.edu/humanrts/africa/afchild.htm

http://www.un.org/womenwatch/daw/cedaw/recommendations/recomm.htm#recom21

[2] DHS Stat-compiler, Macro international. Available at http://www.statcompiler.com.

price traditions interact with sibship structure in the determination of early marriage.

The choice of Malawi draws on two sets of motives. First, methodologically, the heterogeneous ethnic composition of the Malawian population, and, in particular, the coexistence of ethnic groups following matrilineal descent practices and others following patrilineal ones, creates an exogenous variation in the norms determining the direction of transfers paid at marriage. Second, Malawi's performance in terms of social indicators is particularly poor, as well as particularly relevant is the issue of early marriage.

The maternal mortality ratio is currently at 984 per 100,000 live births and it's one of the highest in the world together with its fertility rate of 6.3 children per woman. Moreover, the gender bias in adult literacy rate is relevant: 75% for males and only 54% for females. On the other hand, being inexistent any law impeding marriage below a specified age threshold, the practice of female early marriage is still widespread among the younger cohorts. Among women aged 20-24, the overall fraction of those who got married by the age of 15 is 10.2% and by the age of 18 is 46.9%, with a even higher incidence in rural areas where early marriage involves more than 50.4% of women (UNICEF, 2005).

The identification strategy relies on the coexistence of matrilineal and patrilineal kinship systems, respectively used to dowry and brideprice transfers at marriage, and exploits within groups variations of the exogenously determined gender composition of siblings and of birth order.

After controlling for the demographic composition of the natal family, for cohort and district variations, I find that, on average, women in matrilineal groups tend to marry relatively younger the higher the number of older sisters, whereas older brothers reduce average age at marriage for women in patrilineal groups, with sizeable effects of 18.5 and 26.1 percentage points, respectively.

These differences persist when looking to the probability of being married by the age of 16. Having one older sister makes matrilineal women 10 percentage points more likely to marry before the age of 16, whilst having one older brothers instead of none, increase the marriage probability by 5.4 percentage points.

The results of this analysis are related to three main strands of the literature. On the one hand there is the literature on intrahousehold resource allocation and its spin-off on sibling rivalry[3].

The literature on siblings rivalry has focused on the impact of sibship composition on education, health status and intergenerational transfers (Butcher and Case, 1994; Morduch, 2000; Garg and Morduch, 1998; Parish and Willisy, 1993; Zhang and Davies, 1995). This research has highlighted

[3] See Behrman (1997) and Bergstrom (1997) for a comprehensive review of the literature on theories of the familay and intrahousehold allocation.

the fundamental relevance of demographic factors in explaining human capital investments of parents into children, shading light on the existence of a bias against daughters in investments and assets' allocation when households are faced with credit constraints. Assuming parental altruism and that parents equally care about all their children, irrespective of their sex, these differences have been explained either calling for different returns to education for boys and girls and (or) to a different contribution of sons and daughters to the future parental household's welfare. In particular, the possibility for parents to retain part of future returns from children's education critically depends on the residential practices adopted at marriage. If marriage traditions require brides to live their natal household and to reside with their in-laws, parents will lose part of the return on their investments simply by the fact that daughters have left.

Another typical finding is that children might end up doing better if their siblings are sisters, since in many societies they have less claim over parental resources, or older sisters may contribute to school fees for younger children (Parish and Willis, 1993) or to other younger siblings caretaking (Weisner and Gallimore, 1977).

With respect to this literature, the contribution of my paper is to identify another channel through which sibling rivalry may manifest. In particular, if early marriage determines educational outcomes as found by Field and Ambrus (2005)[4], analyzing the interaction between the demographic structure of the household and educational outcomes without taking into account the implications in terms of parents' incentives to anticipate or delay their daughters' marriage could be potentially misleading.

The second relevant piece of literature is the one explicitly addressing transfers at marriage and the economics of the marriage market. Since the seminal work of Becker (1991), transfers at marriage, either in the form of dowries or brideprices, have been modeled as spot prices clearing marriage market or transfers compensating for differences in the distribution of future household's production within the couple. For example, Rao (1993) explains the positive trend in dowries payments in the Indian marriage market with a "marriage squeeze" generated by population growth and sex imbalances.

In more recent studies, transfers at marriage have been invested with other complementary roles. For example, dowries, as a form of premortem intergenerational transfers, have been interpreted either as an instrument through which parents secure future daughters' welfare[5] (Zhang and Chan, 1999), or solve free riding problems between siblings in virilocal societies

[4] Using data from rural Bangladesh, the authors isolate the causal effect on education by exploiting variation in the timing of menarche as an instrumental variable for age of first marriage. They find that each year of marriage's delay is associated with 0.30 additional years of schooling and 6.5% higher probability of literacy.

[5] Dowries both increase the wealth of the new conjugal unit and enhance the bargaining power of the bride in intrahousehold allocation.

(Botticini and Siow, 2003). Fafchamps and Quisumbing (2005), using detailed data on rural Ethiopia, provide an empirical analysis of marriage patterns and the determinants parental transfers to newly formed households, and find that intergenerational transfers take place primarily at the time of marriage.

Finally, this paper relates to the growing literature focusing on kinship systems, inheritance practices and economic outcomes. La Ferrara (2007), shows how the differences in inheritance patterns between Ghanian matrilineal and patrilineal groups affect the incentives for inter-vivos transfers. Quisumbing and Otsuka (2001) and Quisumbing et al. (2004) have studied how different inheritance system and their evolution affect parental investmets in children's and intergenerational bequests.

The paper is organized as follows. I begin in Section 2 with a brief description of the Malawian socioeconomic and institutional context, focusing in particular on kinship systems and marriage norms. Section 3 outlines the empirical strategy and describes the data used in the analysis, while Section 4 presents the main results together with robustness checks. Section 5 concludes.

2 Socioeconomic conditions and kinship systems in Malawi

Malawi is one of the poorest countries in the world, with a per capita GNI of 160 US$.

According to the 2004/2005 Integrated Household Survey, the poverty rate for the Malawian population is 52.4% with poor people being overrepresented in rural areas where the 85% of the population lives.[6]

The poor economic performance is reflected in the low ranking in terms of human development indicators which places Malawi 164th out of 177 countries.[7] Life expectancy is very low: the probability of not living to 40 years is almost 60%. The infant mortality rate is 189 per 1000, and the under-five mortality rate is 114 per 1000, confirming that one in five children will not survive to their fifth birthday. The reported maternal mortality rate is also very high: 984 per 100,000 live birth, while the adjusted maternal mortality rate is 1800 per 100,000 (WHO, 2005). Even if total fertility rates have declined over the last decade, current estimates implies that a Malawian woman would have six children in her lifetime; fertility is also reported to be higher in the younger age groups (National Statistical Office, 2005).

[6] The poverty rate is computed with respect to a national poverty line of 16,165MK (Malawi Kwacha) per year. The poverty rate in rural areas is 56%, and within this figure, about 24.3% of people face extreme poverty conditions.

[7] UNDP, Human Development Report 2007/2008.

Poor health outcomes are also associated with a wide gender gap in education. Adult literacy for males is 75 percent while that of females is only 54 percent. Moreover, around 50% of women in rural areas enter marriage by the age of 18 with 60% of them being without any formal education.

Even if the Malawi's Constitution, under section 22, provides that the minimum legal age for marriage is 18 years for all persons, no explicit ban exists on early marriage which is allowed with the consent of parents or guardians. In practice, it is quite common in the rural areas for girls of age 15 years and below to enter into marriage.

The practice of early marriage, besides hindering socioeconomic outcomes, is particularly relevant in the Malawian context as it potentially interrelates with norms governing access to the scarce land resources.

One of the key causes of poverty and vulnerability in Malawi has been identified in the limited access to land and in the increased population pressure. Malawi is in fact one of the most densely populated countries in Sub Saharan Africa.

The average population density is close to 130 persons per square km, peaking to 174 persons in the Southern region, where higher levels of poverty and lower levels of literacy are concentrated. Regional variations are further accentuated when looking at average farm sizes which are reported to be around 10-15 ha in the Northern Region, 5-10 ha in the Central, and as low as 0.1 ha in the Southern Region (MoA, 2005).

Besides the increasing demand for, and the recent attempts to promote the formation of a market for land, the customary tenure system accounts for 70 to 80 percent of the total agricultural land and it covers most of the country's smallholder farmers, as well as a disproportionate concentration of those living in poverty.

The access to land under customary tenure varies according to the norms regulating customary marriage and inheritance arrangements. These norms can be divided into matrilineal and patrilineal systems depending on the location and sociocultural history of each community. In the northern region, the main ethnic groups are Ngoni, Tonga, Ngonde and Tumbuka, which follow predominantly patrilineal kinship system; by contrast, in southern and central region, the main ethnic groups are Chewa, Yao, Nyanja, Tonga and Lomwe which are mostly matrilineal. In both systems, land is accessed at marriage[8].

In patrilineal groups the transfer and sub-division of land is from a father to his male children. When a young man gets married, with the practice of *Chitengwa*, he takes the wife and any children to his own village under the payment of a brideprice (*lobola*), and the newly formed conjugal unit lives on the resources accumulated and inherited by the husband. In the patrilineal-

[8]The customary land tenure system allows an individual only to access to, and not ownership, of land.

virilocal system, the woman obtains her rights to land through her husband. In case of death of the latter, she can exercise her rights through her male children, if she decides to remain in the village of the deceased husband. In case of divorce she will have to return to her original village and ask for land from her lineage headmen.

On the contrary, in matrilineal groups, land is transferred from a mother to her female children once the transfer is approved by the heads of the matrilineage (who, generally, are the senior maternal uncles) and sub-divided accordingly.

With the practice of *Chikamwini*, the husband goes to stay in his wife's village and his rights to land are held through his wife. In matrilineal-uxorilocal systems, marriage does not involve the payment of a brideprice, but the transfer of land at marriage constitutes a form of dowry for the newly formed conjugal unit.

Given the increased competition over scarce land resources and the very limited opportunities for non-farm employment in rural areas, the transfer of land and assets at marriage represents the main source of future livelihood for newly formed households.

3 Empirical strategy and data

3.1 Hypothesis and Methodology

In the empirical analysis I will try to understand whether the direction of transfers determined by kinship norms interacts with the demographic structure of the household and affects women's age at marriage.

In matrilineal groups, marriage of each daughter entitles her to a dowry. Typically, the dowry payment involves the transfer of a small plot, as the absence of a land market constraints the livelihood of the future household upon the transfers obtained at marriage by the bride's family. In matrilineal groups, the practice of residence after marriage is predominantly uxorirocal, that is the husband comes to live in his wife's village and works on the plot of land inherited at marriage from her family.

By contrast, in patrilineal groups which follow a virilocal residence practice, the marriage of each daughter entitles her family to receive a brideprice (*lobola*), typically involving a livestock's' transfer, whose payment empowers the husband to take the wife to his natal village to work with him on the plot of land received by his parents. Given the total amount of resources available to the bride's natal family, the opposite direction of transfers paid at marriage prevailing in matrilineal and patrilineal ethnic groups together with the demographic composition of siblings can have implications in terms of the age at which women are expected to marry.

As pointed out by anthropological studies[9], bride's lower age at marriage directly affects the amount of transfers paid or received by her natal family. Having a younger wife is generally valued more by the perspective husband as lower bride's age at marriage is typically associated with higher total fertility. If this is the case, and if the objective is to maximize the quality of the marriage match of each of their daughters for given household assets, parents can trade off the amount of resources to be transferred (or received) with the age at marriage of each daughter.

This possibility creates an additional motive for siblings rivalry over household's scarce resources. In particular, each daughter in a matrilineal household will be competing with older sisters for whom the parents have paid a dowry, whereas in patrilineal households each daughter is competing with older brothers who pay a brideprice and hence extract resources from the natal family.

Given this simple theoretical framework, I expect age at marriage to depend negatively on the number of older sisters in matrilineal groups, and on the number of older brother in patrilineal ones. In the empirical specifications, I will test this hypothesis first by estimating a standard OLS model on the age at marriage for each woman in the sample, and second by using a probit model to estimate the probability of being married by the age of 16. The main variables of interest will be those identifying the kinship system and those related to the demographic structure of each woman's sibship. In particular, I will use the matrilineal dummy identifying the ethnic groups for which marriage entails the dowry payment, and the interaction of this dummy with the number of older brothers and the number of older sisters.

Specifically the two models estimated will be the following:

$$\begin{aligned} AM_i &= \alpha M_i + \beta NrOS_i + \rho M_i * NrOS_i + \theta NrOB_i + \gamma M_i * NrOB_i \\ &\quad + \delta NrYS_i + \zeta NrYB_i + \theta X_i + \varepsilon_i \end{aligned}$$

$$\Pr(AM_i < 16) = \Phi(\alpha M_i + \beta NrOS_i + \rho M_i * NrOS_i + \theta NrOB_i + \gamma M_i * NrOB_i + \delta NrYS_i + \zeta NrYB_i + \theta X_i)$$

In the first model the dependent variable AM_i is the age at marriage for woman i. Among the regressors, the dummy variable M_i identifies a woman belonging to a matrilineal ethnic group; $NrOS_i$ and $NrOB_i$ represent, respectively, the number of $i's$ older sisters and the number of older brothers alive when she got married. These variables enter the model both alone, and interacted with the matrilineal dummy M_i. According to my hypothesis, I expect the ρ coefficient to be negative, as older sisters decrease resources

[9] See Papps (1983).

available in matrilineal households, which is expected to be compensated by a lower age at marriage. By contrast, I expect women in patrilineal groups to marry earlier the higher the number of older brothers, so that θ should be negative. Ex ante, I also expect the sign of β, which represent the impact on the age at marriage of the number of older sisters in patrilineal groups, to be positive[10] whilst I don't have any prior on the sign of γ, which is the effect of older brothers in matrilineal groups.

As further regressors, I control separately for the number of younger sisters and younger brothers ($NrYS$, $NrYB$), to take care of the total sibship size. To conclude, the X vector includes time, geographical and religion fixed effects. In particular, I control for woman's age and its square, 27 district dummies and 8 religion dummies.

The second model estimated, a Probit, the dependent variable is a dummy taking value of one if the woman got married by the age of 16, and zero otherwise; Φ is the Normal cdf. All the regressors match those in the OLS specification.

3.2 Data

The data used in this paper come from the Malawi Demographic Health Survey (MDHS) of 2004. These data, collected by Macro International, cover detailed information about women in the age period between 15 and 49. For these women, together with standard demographic characteristics, the complete birth history and the health conditions, I have detailed information on their ethnic identity, their marital status as well as on the age at which the first marriage occurred.

The main advantage of using this dataset, however, comes from the existence of a maternal mortality section conveying detailed retrospective information on each sampled woman's ever born siblings, their sex, their date of birth and of death for deceased ones. From this section I am able to recover the exact sibling composition of each woman at the time of her first marriage, which will be essential for the analysis of the impact of siblings rivalry on early marriage.

The only limitation in using MDHS survey is the lack of direct information on the transfers intercurred at marriage between the groom and the bride's households or on their income and assets. As it has been discussed with more detail in the previous paragraph, the identification strategy will help alleviating these data shortcomings, and I further test the robustness of results by using data at hand on current land ownership (Section 4.2).

The sample used in the analysis has been restricted to women in rural households, which represent 86% of the total. Urban and rural samples are

[10] If daughters enter marriage according to their seniority, older sisters are expected to have contributed to household welfare by originating brideprice payments.

clearly different along all the relevant dimensions, with women in urban areas marrying on average one year later with respect to their rural counterpart[11]. Even though the distribution of matrilineal and patrilineal groups is not dramatically different between the urban and the rural samples so that the analysis could have been practically conduced on both, the restriction to the sole rural women is meant to capture clear evidence on the impact of gifting norms on marriage outcomes. As shown in several studies[12], the erosion of customary practices is particularly accentuated in urban areas, where market driven incentives are predominant and often conflicting with traditional practices.

The analysis has been further restricted to women whose first childbirth was subsequent to first marriage's date and to women having been married only once[13]. While the former condition is meant to avoid variations in marriage contract's arrangements due to a premarital sex practice[14], the latter is aimed both at reducing measurement error in the date of first marriage and at reducing selection issues in the sample.

[Table 1: Geographical breakdown of matrilineal and patrilineal ethnic groups]

The resulting working sample consists of 7,133 women aged 15-49. Table 1 shows the geographical distribution together with the matrilineal, patrilineal breakdown. Consistently with the official figures on regional population densities, women living in the Northern region represent only 14% of the sample, whilst those living in the Southern region, the most densely populated area, represent 47.7%. The matrilineal lineage system, which we identify according to the ethnic identity[15], characterize the majority of the sample, with significant variations across the different regions. In particular, women in matrilineal groups are the majority of those living in the Central and Southern regions. In all the specifications, I will control for the district of residence. Of the 27 districts in the sample, 6 of them belong to the Northern region, 9 to the Central and the remaining 12 to the Southern one. The distribution of matrilineal ethnic groups varies considerably across districts as well, with matrilineal groups representing from 2 to 87%

[11] The average age at first marriage for women in the urban sample is 18.15 against 17.29 of rural women. The difference in averages is significant at 1% confidence level.

[12] See La Ferrara (2007) for a discussion of the issue in the Ghanian context; Quisumbing and Otsuka (2001) for Sumatra; Peters (1997) and Takane (2007) for Malawi.

[13] Taken together, these restrictions eliminate 29% of the original rural sample. More precisely, women giving birth prior to marriage constitute the 8.63% of the rural sample, whereas women with more than one marriage are the 22.41%.

[14] See Bishai and Grossbard (2007).

[15] Matrilineal ethnic groups are the Chewa, Lomwe, Tonga and Yao.

of the sample in the North, 12 to 97% in the Centre and 3 to 93% in the South. In all my specifications, district fixed effects are meant to capture both variations in land scarcity, population density and any potential spatial heterogeneity in marriage markets and transfers practices.

[Figure 1: Distribution of age at marriage for matrilineal and patrilineal ethnic groups]

In Figure 1, I've plotted the kernel density estimates for the age at marriage of women belonging to matrilineal and patrilineal groups (panel a), together with the respective cumulative density functions (panel b). The distribution of the age at first marriage does not show any significative difference between the two groups, with women in matrilineal ethnic groups having a slightly higher probability of getting married before being seventeen, the overall sample's average. This finding provides additional support to my identification strategy which relies on within group heterogeneity in the age and gender composition of women's siblings, and uses the kinship system as an exogenous source of variation in marriage norms. The Spearman rank correlation coefficient for total siblings and the fraction that are female is -0.005, whereas the one for the number of older siblings and the relative fraction who are female is -0.0105. None of them is significantly different from zero, and similar results are found when looking to matrilineal and patrilineal groups separately.

[Table 2: Test of equality of means of relevant variables between matrilineal and patrilineal groups]

Table 2 reports the results for a two sample test of equality of means for all the variables used in the empirical analysis. As pointed out by the previous graphs, no significant difference in the average age at marriage exists between women in matrilineal and patrilineal groups. In terms of the demographic composition of the natal household, matrilineal women have on average more brothers than patrilineal ones, whilst there's no significant difference in the total number of sisters alive at marriage. Those patterns are confirmed when breaking down the analysis to older and younger siblings. Matrilineal women have on average 0.989 older sisters, against 1.009 of patrilineal ones, whereas, on average, older brothers are 1.022 and 1.011, respectively. None of these differences is statistically significant. As for younger siblings, women in matrilineal groups have on average more younger brothers than patrilineal ones, a difference significant at 5% level.

Matrilineal women in the sample are on average one year younger, and with respect to current land holdings, there's no significant difference between the two groups.

[**Figure 2: Distribution of agricultural land size**]

The average land quantity is about 5 acres in both groups, an amount which is somehow bigger than the typical plot size reported by the Malawian Ministry of Agriculture.[16] This figure however can be better interpreted looking at Figure 2 in which I've plot the cumulative density function of current plots' size. It can be clearly noted that, indeed, about 37% of women have access to plots smaller than one acre, and 60% of them have no more than two acres of land.

4 Results

4.1 Main results

The first model estimated is a linear multivariate regression on the age at marriage (Table 3). In all the specifications, I've used probability weights to account for the survey's sample design and computed robust standard errors.

The first column of Table 3 shows the results from my baseline specification.[17] The first regressor included is a dummy identifying women belonging to matrilineal ethnic groups. To control for the effect of household size on the age at marriage, I use the total number of sisters an brothers in the household at the time the woman got married, together with the square of each of these variables. Furthermore, in order to capture the impact of birth and gender rank, I control separately for the number of older sisters and the number of older brothers.

Additional explanatory variables are age (and its square), 27 district dummies, 8 religion dummies and a constant term. Being the dependent variable the age at marriage, the inclusion of age in my specifications is meant to capture cohort effects.

According to these first estimates, matrilineal women do not marry significantly younger than patrilineal ones. The overall number of siblings seems to increase the age at marriage, although at a decreasing rate, whereas the opposite holds for the effect of having older siblings. This seem to indicate that having younger siblings increases women's age at marriage, a

[16] See Section 2.

[17] Detailed summary statistics on the variables used in the regressions can be found in Appendix, Table A1.

result which is consistent with the hypothesis of older sisters having care-keeping responsibilities within the household. Controlling for the overall number of older brothers and older sisters, having another (younger) sister increases the age at marriage by 16.1 percentage points; when looking to younger brothers, the effect found is of similar magnitude. In terms of age, women in more recent cohorts seem to marry relatively younger, probably due to the increased pressure on land resources, which may have created incentives to anticipate marriage in order to secure the access to land assets for younger cohorts. A further inspection shows that the concave relation between age at marriage and age has its maximum around the age of 30.

In column 2, I've included two interaction terms to take into account potential heterogeneity in the effects of the demographic composition of siblings between matrilineal and patrilineal groups. Whilst having older sisters in patrilineal groups doesn't affect significatively the age at marriage, or, if any, this effect would be to posticipate marriage, a woman in matrilineal groups faces her age at marriage reduced by 18.5 percentage points for each older sister she has in her natal family. The opposite holds for older brothers, each of whom reduces by 26 percentage points the age at marriage of women in patrilineal ethnic groups, whilst increases the age at marriage of matrilineal sisters by 14.4 percentage points.

These results are consistent with my theoretical predictions[18] according to which we should expect lower age at marriage associated with having older sisters in matrilineal groups and older brothers in patrilineal ones. In the first case older sisters extract households' resources through dowry payments, whereas in the second one, older brothers are expected to extract resources for brideprice payments. Whatever the direction of the transfers, a lower age at marriage of younger daughters could compensate for fewer household's resources, both in terms of the overall welfare of the bride's family in patrilineal groups, and in terms of the quality of marriage perspectives for the wife-to-be in matrilineal groups.

In order to corroborate the results obtained, I've also tested the interaction of the matrilineal dummy with the total number of brothers and sisters[19]. If my theoretical predictions were correct, I should not find any significant effect on younger siblings and any heterogeneous response for matrilineal and patrilineal women. The results shown in column 3 and in column 4, where I have included the full set of interactions with older and total siblings, confirm my hypothesis.

[Table 3: OLS - Linear regression: age at first marriage]

[18] See Section 3.1.

[19] Given that results are conditional on the number of older brothers and older sisters, the overall number of brothers and of sisters should be interpreted as the number of younger siblings.

[Table 4: Probit model: Probability of being married by the age of 16]

Table 4 shows the results from a Probit model on the probability of being married by the age of 16. The table reports marginal effects; sampling weights have been used and robust standard errors computed. All the specifications presented here follow those analyzed for the linear regression model and the results are very close to those previously found. The direction of all the effects remains unchanged but some of the coefficients now are significantly different from zero. Depending on the specification, matrilineal women are now from 7.5 to 8.1 percentage points more likely to get married before reaching the age of 16.[20]

The main findings are reported in column 2.

Ceteris paribus, the higher number of (younger) brothers and sisters reduces the probability of early marriage by 3.1 and 3.3 percentage points, respectively. On the other hand, having one older sister, makes matrilineal women 10 percentage points more likely to marry before the age of 16, while one older brother reduces the likelihood by 3.4 percentage points.

Going from zero to eight older sisters, ceteris paribus, makes matrilineal women 40% more likely to marry early, whereas the same variation in the number of older brothers, makes them 13% less likely. In patrilineal groups instead, having one older brother instead of none, increase the marriage probability by 5.4 percentage points, whereas one sister reduces it by 2 percentage points, although this last coefficient is not fully significant.[21]

4.2 Robustness checks

The main limitations of the DHS data are the lack of information about women's natal household resources at the moment of marriage and the absence of direct information on intergenerational transfers occurred at marriage. The availability of such information would have been useful to analyze the extent to which resources constraints foster sibling rivalry and to directly test the hypothesis that age at marriage is inversely related to the amount of transfers at marriage.

Nevertheless, a possible robustness check of the results obtained insofar is to use current land holdings as a proxy for land transfers occurred at marriage.

As described in Section 2, the land market in rural Malawi is basically inexistent and the access to land under customary tenure system occurs almost exclusively through marriage contracts. If this is the case, current

[20] This result confirm the evidence suggested by the Figure 1.

[21] The marginal effect, computed at mean of the variables are 0.056 and 0.019, respectively.

land holdings should be a good proxy, or, at least, very highly correlated with land transfers at marriage. To further refine the analysis I have restricted the sample to the sole women who are currently either household heads or spouses of the household head.

Table 5 reports the results for the main variables on interest of the second specification for both the OLS and the probit models previously estimated. All the previous results appear confirmed and slightly magnified when controlling for current land holdings.

5 Conclusions

This paper focuses on the impact of marriage norms and sibling rivalry on early marriage in rural Malawi.

Malawi is particularly well suited to this kind of analysis because of the variation in marriage norms between different ethnic groups and in particular between different kinship systems. Furthermore, Malawi has a very low performance in terms of human development indicators and in terms of gender bias so that my analysis can have relevant policy implications.

Marriage traditions of matrilineal and patrilineal groups differ in the direction of the transfers intercurred at marriage. In patrilineal groups the groom pays a brideprice, whereas in matrilineal groups, the bride gets a dowry from her parents. The direction of these transfers can have important implication on how the demographic structure of the woman's natal household affects the age at which she gets married. The estimation results show that, on average, women in matrilineal groups tend to marry relatively younger the higher the number of older sisters, whereas older brothers reduce average age at marriage for women in patrilineal ones. These differences persist when looking to the probability of being married by the age of 16.

My findings contribute to the literature of sibling rivalry by showing a potential channel through which the demographic composition of households could affect economic outcomes. Moreover, the differences outlined between matrilineal and patrilineal marriage norms could help interpreting mixed results often found when comparing the impact of siblings' gender composition across different countries (Morduch, 2000).

In terms of policy implications, my results suggest the potential heterogeneity of the effects of policy interventions aimed at hindering early marriage. Variations in marriage tradition are important when identifying policy target groups. Besides calling for legal restriction on the age at marriage, enforcement of marriage laws, and promoting behaviour-change campaigns toward the prevention of early marriage, policies could be targeted to groups particularly "at risk". For example, if keeping girls at school longer could reduce early marriage, conditional cash transfers programs tailored to be relatively more generous for women in patrilineal ethnic groups having older brothers, and for matrilineal women having older sisters can be expected to be more successful.

Moreover, to address the problem of early marriage, it could be important to have comprehensive policy interventions for example easing the access of poor people to credit and developing a land market through land reforms in order to limit the dependency of newly weds on the resources obtained at marriage.

References

[1] Becker, G.S. (1991). "A Treatise on the Family". 2nd edition. Harvard University Press, Cambridge, MA.

[2] Behrman, J. R. (1997). "Intrahousehold Distribution and the Family", Handbook of Population and Family Economics, Mark R. Rosenzweig and Oded Stark (eds.), North-Holland, Amsterdam, pp. 125–187.

[3] Bergstrom, T. C. (1997): "A Survey of Theories of the Family", Handbook of Population and FamilyEconomics, Mark R. Rosenzweig and Oded Stark (eds.), North-Holland, Amsterdam, pp. 21–79.

[4] Bishai, D. and Grossbard, S. (2007). "Far Above Rubies: The Association Between Bride Price and Extramarital Sexual Relations in Uganda", IZA Discussion Paper Nr.2982.

[5] Black, S.E., Devereux, P.J., Salvanes, K.G. (2005). "The More the Merrier? The Effect of Family Size and Birth Order on Children's Education", Quarterly Journal of Economics, Vol.120(2): 669-700.

[6] Botticini, M. and Siow, A. (2003). "Why Dowries?", The American Economic Review, Vol. 93(4): 1385-1398.

[7] Bracher, M., Santow, G. amd Cotts Watkins, S. (2003). ""Moving" and marrying: modelling HIV infection among newly-weds in Malawi", Demographic Research, Special Collection 1, 7.

[8] Bruce, J. and Clark, S. (2004). "The implications of early marriage for HIV/AIDS policy," brief based on background paper prepared for the WHO/UNFPA/Population Council Technical Consultation on Married Adolescents. New York: Population Council.

[9] Bruce, J. (2002). "Married adolescents girls; human rights, health and development needs of a neglected majority", paper presented by the Population Council at the Supporting Event: Early Marriage in a Human Rights Context, United Nations Special Session on Children, May 2002.

[10] Butcher, K. and Case, A. (1994). "The effects of siblings sex composition on women's education and earnings", Quarterly Journal of Economics, Vol.109(3): 531-563.

[11] Fafchamps, M. and Quisumbing, A. (2005). "Marriage, Bequests, and Assortative Matching in Ethiopia", Economic Development and Cultural Change, 53(2): 347-80.

[12] Field, E. and Ambrus, A. (2006). "Early marriage and female schooling in Bangladesh", Working Paper, Harvard University.

[13] Garg, A. and Morduch, J. (1998a). "Sibling rivalry and the gender gap: evidence from child health outcomes in Ghana", Journal of Population Economics, Vol.11(4): 471-493.

[14] Garg, A. and Morduch, J. (1998b). "Sibling rivalry", Working Paper, Woodrow Wilson School, Princeton University.

[15] Hoddinott, J., and Haddad, L. (1995). "Does Female Income Share Influence Household Expenditures? Evidence from Côte d'Ivoire", Oxford Bulletin of Economics and Statistics, 57(1): 77-95.

[16] Jensen, R. and Thornton, R. (2003). "Early female marriage in the developing world", Gender and Development, Vol. 11(2): 9–19.

[17] La Ferrara, E. (2007). "Descent Rules and Strategic Transfers. Evidence from Matrilineal Groups in Ghana", Journal of Development Economics, Vol.83(2): 280-301.

[18] MoA, (2005). "Road map on agriculture development in Malawi", Ministry of Agriculture, Lilongwe, Malawi.

[19] Morduch, J. (2000). "Sibling rivalry in Africa", The American Economic Review, Vol.90(2): 405-409.

[20] National Statistical Office (2005). "Preliminary results of the 2004 Demographic and Health Survey", Zomba, Malawi.

[21] Papps, I. (1983). "The Role and Determinants of Bride-Price: The Case of a Palestinian Village", Current Anthropology, Vol. 24(2): 203-215.

[22] Parish, W. and Willis, R. (1993). "Daughters, education, and family budgets: Taiwan experiences", Journal of Human Resources, Vol.28(4): 962-998.

[23] Patrinos, H.A. and Psacharopoulos, G. (1992). "Socioeconomic and ethnic determinants of grade repetition in Bolivia and in Guatemala", Policy Research Working Paper Series 1028, World Bank, Washington, DC.

[24] Peters, P. E. (1997). "Against the Odds: Matriliny, land and gender in the Shire Highlands of Malawi", Critique of Anthropology, Vol.17(2): 189-210.

[25] Quisumbing, A.R. and Otsuka, K. (2001). "Land inheritance and schooling in matrilineal societies: evidence from Sumatra", World Development, Vol.29(12): 2093-2110.

[26] Quisumbing, A. R., Estudillo, J.P. and Otsuka, K. (2004). "Land and schooling: transferring wealth across generations", Johns Hopkins University Press for the International Food Policy Research Institute, Baltimore, MD.

[27] Rao, V. (1993). "The rising price of husbands: A hedonic analysis of dowry increases in rural India", Journal of Political Economy, Vol.101(4): 666-677.

[28] Takane, T. (2007). "Customary Land Tenure, Inheritance Rules, and Smallholder Farmers in Malawi", Institute of Developing Economies Discussion Paper, Nr.104.

[29] Tauchen, H.V., Dryden Witte, A. and Long, S.K. (1991), "Violence in the family: a non-random affair", International Economic Review 32: 491-511.

[30] Thomas, D. (1990). "Intra-Household Resource Allocation: An Inferential Approach", The Journal of Human Resources, 25: 635-664.

[31] UN Population Division (2000). "World Marriage Patterns", Department of Economic and

Social Affairs, Washington, DC.

[32] UNICEF (2005). "Early marriage: A harmful traditional practice. A statistical exploration", United Nations Children's Fund, New York, April 2005.

[33] Weisner, T.S. and Gallimore, R. (1977). "My brother's keeper: Child and siblings caretaking", Current Anthropology, Vol.18(2): 169-190.

[34] Westoff, C.F. (1992). "Age at Marriage, Age at first Birth, and Fertility in Africa," World Bank Technical Papers Nr.169, The World Bank, Washington, DC.

[35] Zhang, J. and Chan, W. (1999). "Dowry and wife's welfare: a theoretical and empirical analysis", Journal of Political Economy, Vol.107(4): 786-808.

[36] Zhang, J. and Davies, J. (1995). "Gender bias, investments in children and bequests", International Economic Review, Vol.36(3): 795-818.

Table 1: Geographical breakdown of matrilineal and patrilineal ethnic groups

Region	Total	Matrilineal	Patrilineal
Northern	0.140	0.171	0.829
Central	0.393	0.785	0.215
Southern	0.467	0.733	0.267
Total	1	0.675	0.325

Table 2: Test of equality of means of relevant variables between matrilineal and patrilineal groups

Variable	MATRILINEAL			PATRILINEAL			Mean(Patril)-Mean(Matril)
	Nr. Obs	Mean	Std. Dev.	Nr. Obs	Mean	Std. Dev.	
Age at 1st marriage	3904	17.076	2.875	1880	17.040	2.606	-0.036 (-0.465)
Nr. sisters alive at marriage	3779	2.236	1.500	1831	2.265	1.494	0.029 (0.683)
Nr. brothers alive at marriage	3779	2.270	1.533	1831	2.210	1.473	-0.061* (-1.409)
Nr. older sisters alive at marriage	3779	0.989	1.171	1831	1.009	1.162	0.020 (0.597)
Nr. older brothers alive at marriage	3779	1.022	1.224	1831	1.011	1.165	-0.011 (-0.314)
Nr. younger sisters alive at marriage	3779	1.247	1.260	1831	1.257	1.267	-0.009 (0.258)
Nr. younger brothers alive at marriage	3779	1.249	1.278	1831	1.199	1.220	-0.050* (-1.393)
Age	4813	25.994	8.721	2320	26.623	9.267	0.628*** (2.793)
Land quantity (acres)	3402	5.696	64.296	1612	5.053	31.519	-0.643 (-0.381)

Figure 1: Distribution of the age at first marriage for matrilineal and patrilineal ethnic groups

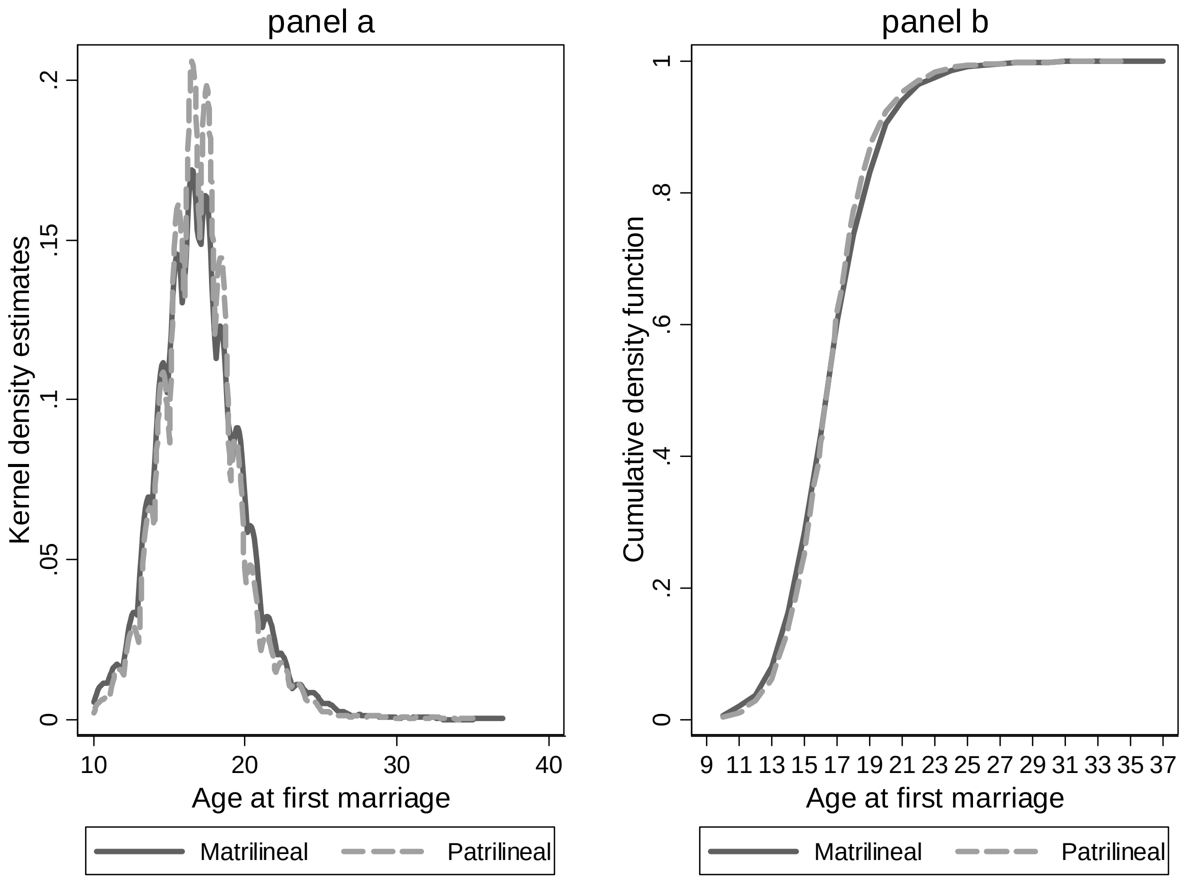

Figure 2: Distribution of agricultural land size

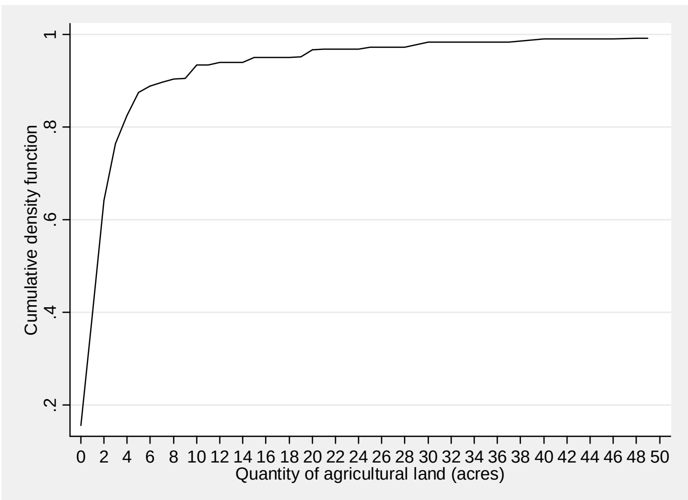

Table 3: OLS – linear regression: age at first marriage

	(1)	(2)	(3)	(4)
Matrilineal Dummy	-0.182	-0.144	-0.200	-0.192
	(1.32)	(0.82)	(0.77)	(0.74)
Nr. older sisters	-0.038	0.096	-0.040	0.119
	(0.78)	(1.32)	(0.83)	(1.42)
Nr. older brothers	-0.155***	-0.261***	-0.157***	-0.273***
	(3.47)	(3.55)	(3.54)	(3.29)
Nr. sisters alive	0.161*	0.154*	0.102*	0.034
	(1.75)	(1.69)	(1.84)	(0.54)
Nr sisters (squared)	-0.019	-0.017		
	(1.17)	(1.10)		
Nr. brothers	0.154*	0.165*	0.085	0.139**
	(1.72)	(1.84)	(1.40)	(2.05)
Nr. brothers (squared)	-0.004	-0.006		
	(0.30)	(0.43)		
Age	0.208***	0.206***	0.204***	0.204***
	(4.81)	(4.78)	(4.72)	(4.72)
Age (squared)	-0.003***	-0.003***	-0.003***	-0.003***
	(3.94)	(3.91)	(3.86)	(3.86)
Constant	13.019***	12.842***	13.135***	12.989***
	(14.32)	(13.88)	(13.80)	(13.66)
*Nr. older sisters*Matrilineal*		*-0.185***		*-0.222***
		(2.30)		*(2.17)*
*Nr. older brothers*Matrilineal*		*0.144**		*0.157*
		(1.80)		*(1.60)*
*Nr. sisters*Matrilineal*			*-0.056*	*0.040*
			(0.89)	*(0.51)*
*Nr. brothers*Matrilineal*			*0.065*	*-0.008*
			(0.97)	*(0.10)*
District Dummies	Yes	Yes	Yes	Yes
Religion Dummies	Yes	Yes	Yes	Yes
Observations	5169	5169	5169	5169
R-squared	0.050	0.052	0.050	0.051

Robust t statistics in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

<u>Table 4</u>: Probit model: Probability of being married by the age of 16. Marginal effects.

	(1)	(2)	(3)	(4)
Matrilineal Dummy	0.076***	0.075**	0.081**	0.080**
	(3.24)	(2.50)	(2.07)	(2.03)
Nr. older sisters	0.009	-0.019	0.009	-0.016
	(1.11)	(1.48)	(1.17)	(1.11)
Nr. older brothers	0.031***	0.056***	0.031***	0.052***
	(4.02)	(4.54)	(4.00)	(3.50)
Nr. sisters	-0.035***	-0.033**	-0.027***	-0.016
	(2.59)	(2.43)	(2.73)	(1.46)
Nr sisters (squared)	0.004*	0.004		
	(1.76)	(1.57)		
Nr. Brothers	-0.028**	-0.031**	-0.016	-0.026**
	(2.01)	(2.18)	(1.63)	(2.24)
Nr. Brothers (squared)	-0.000	-0.000		
	(0.20)	(0.01)		
Age	-0.012*	-0.012*	-0.011*	-0.011*
	(1.92)	(1.87)	(1.81)	(1.81)
Age (squared)	0.000**	0.000**	0.000**	0.000**
	(2.23)	(2.18)	(2.13)	(2.13)
*Nr. older sisters*Matrilineal*		*0.037****		*0.034***
		(2.67)		*(1.98)*
*Nr. older brothers*Matrilineal*		*-0.034***		*-0.028*
		(2.52)		*(1.59)*
*Nr. sisters*Matrilineal*			*0.018**	*0.003*
			(1.70)	*(0.25)*
*Nr. brothers*Matrilineal*			*-0.021***	*-0.007*
			(1.98)	*(0.53)*
District Dummies	Yes	Yes	Yes	Yes
Religion Dummies	Yes	Yes	Yes	Yes
Observations	5169	5169	5169	5169
Adj. R-squared	0.040	0.043	0.041	0.042

Robust z statistics in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 5: Robustness checks.

	OLS	PROBIT
	(1)	(2)
Matrilineal Dummy	-0.168	0.069**
	(0.89)	(2.10)
Nr. older sisters	0.141*	-0.027**
	(1.78)	(2.05)
Nr. older brothers	-0.289***	0.054***
	(3.61)	(4.01)
Nr. sisters	0.201**	-0.034**
	(2.07)	(2.33)
Nr sisters (squared)	-0.024	0.004*
	(1.43)	(1.73)
Nr. Brothers	0.215**	-0.036**
	(2.31)	(2.35)
Nr. Brothers (squared)	-0.014	0.001
	(0.91)	(0.39)
Age	0.165***	-0.010
	(3.41)	(1.45)
Age (squared)	-0.002***	0.000*
	(2.68)	(1.71)
Land quantity (acres)	-0.001*	0.000
	(1.66)	(1.42)
*Nr. older sisters*Matrilineal*	*-0.213***	*0.041****
	(2.44)	*(2.82)*
*Nr. older brothers*Matrilineal*	*0.181***	*-0.035***
	(2.10)	*(2.44)*
Constant	13.136***	
	(11.89)	
District Dummies	Yes	Yes
Religion Dummies	Yes	Yes
Observations	4457	4457
R-squared	0.052	0.048

Robust t statistics in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Table A1: Summary statistics.

Variable	Obs	Mean	Std. Dev.	Min	Max
Age at marriage	5169	17.09789	2.792088	10	37
1=Married by the age of 16	5169	.2696847	.4438389	0	1
1=Matrilineal	5169	.7308957	.4435372	0	1
Nr. of older sisters	5169	.987425	1.16384	0	8
Nr. of older brothers	5169	1.015477	1.20361	0	8
Nr. of sisters	5169	2.239698	1.493278	0	9
Nr. of sisters (squared)	5169	7.245695	8.652564	0	81
Nr. of brothers	5169	2.246856	1.505602	0	9
Nr. of brothers (squared)	5169	7.314761	8.688259	0	81
Age	5169	28.28748	8.494703	15	49
Age (squared)	5169	872.3277	536.6997	225	2401
Nr. older sisters*Matril.	5169	.7229638	1.092941	0	8
Nr. of older brothers*Matril	5169	.746953	1.140567	0	8