ABSTRACT

SANTERAMO, FABIO GAETANO. Three Essays on Applied Aspects of Transaction Costs. (Under the direction of Barry Goodwin.)

Transaction costs are important in economic transactions. Ronald Coase's article is probably the most famous instance in which transaction costs are recognized for their relevance. Yet, transaction costs are relevant in many more frameworks than we are usually inclined to think of. Empirical economists usually struggle with the challenge of disentangling the effects of transaction costs from the main explanatory variables of economic models, but less effort is put into understanding how transaction costs alter empirical results, exactly because they are confounding terms, and not the object of main interest. This dissertation explores three cases in which transaction costs matter, and adopts three novel econometric approaches capable of giving more credit to the role of transaction costs.

Three Essays on Applied Aspects of Transaction Costs

by
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DEDICATION

To Davide and Mariangela. Thanks.

BIOGRAPHY

Fabio G. Santeramo was born in Italy. He received his Bachelor of Arts and Master of Arts degrees in Agricultural Sciences at the University of Bari (Italy). In 2010 he completed a Master of Science in Economics at Iowa State University, and his doctorate in Agricultural Economics at the University of Naples "Federico II" (Italy). After working at the University of Naples "Federico II" for two years, in 2012 he started his Ph.D. program in Economics at North Carolina State University. Fabio is now completing a Ph.D. in Economics with emphasis on Applied Econometrics and Agricultural Economics, and has been appointed as tenure-track Assistant Professor at the University of Foggia (Italy).

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Chapter 1

Introduction

Transaction costs are important in economic transactions. Ronald Coase's article is probably the most famous instance in which transaction costs are recognized for their relevance. Yet, transaction costs are relevant in many more frameworks than we are usually inclined to think of, and are not at all restricted to the public goods word. Let me present a few examples.

Transaction costs are important in markets of private goods, when contracts are stipulated among agents with private information. If transaction costs were zero (i.e. the cost of stipulating or rescending a contract were negligible), stipulating a contract, switching among different contracts, or receding a contract would be frictionless and the private information of the two parties would be immediately revealed leading to a first best solution. Indeed, this is not usually the case: both the principal and the agent have private information, and, as a result, contract decisions are inertial, leading to an interesting path in the decision making process.

Transaction costs are also important in arbitrage activities. If transaction costs were zero arbitrageurs could simply try and guess alternative arbitrage opportunities and exploit the unknown profit opportunities left over in the market. Indeed, price arbitrage (i.e. spatial or temporal) is always costly, and this imposes frictions that alter both price dynamics and the transmission of price signals.

Transaction costs are also important determinants of trade activities. Transfer of goods and services occurs easily among markets that are separated by small (or zero) trade barriers, that is when transaction costs are relatively low (or close to zero). In reality, trade frictions are hardly absent, and their magnitude is a key element to understanding trade flows, and design trade policies.

While theory points to the relevance of transaction costs in all these (and other) frameworks, empirical economists usually struggle with the challenge of disentangling the effects of transaction costs from the main explanatory variables (usually the "core" of the analysis) of economic models. In other terms, less effort is put into understanding how transaction costs alter empirical results, exactly because they are confounding terms, and not the object of main interest.

This dissertation explores three cases in which transaction costs matter, and adopts three novel econometric approaches capable of giving more credit to the role of transaction costs. In the first essay I examine the role of experience, as a way of lowering transaction costs, on participation in crop insurance programs. While the role of adverse selection and moral hazard has been explored extensively, the relevance of experience with insurance contracts is underinvestigated. From farm-level data I estimate a dynamic discrete choice model of participation to model the role of experience. The methodology, coupled with exploratory analysis of the data, allows one to compare the relevance of different sources of information in the crop insurance decision making process. Direct experience is a catalyst for insurance participation of medium and large farms. The experience indirectly acquired is also relevant, especially for small farms. Policy implications are discussed.

In the second essay I revise the implications of the Law of One Price by taking into account spatial and temporal arbitrage opportunities. The existence of transaction costs creates non-linearities in price dynamics that are taken into account. I review the implications of the Law of One Price and test for them with a rich dataset of weekly prices of storable commodities, and information on transaction costs, trade and storage. I conclude that most of the statements implied by the Law of One Price are indeed empirically validated.

In the third essay I focus on the inference that can be made on transaction costs in gravity models. Despite their wide use, a major concern of gravity models relates to the heterogeneous evidence on the impact of geographic distance on trade flows. A plethora of resistance terms and estimators have been adopted to shed light on this puzzle, but the simplistic solution to feed the model with as many variables as we can is not optimal in small datasets, or in the presence of numerous candidates as resistance terms. I evaluate the performance of the Poisson Pseudo Maximum Likelihood estimator when the number of regressors is large, and possibly larger than the number of observations. I propose to use shrinkage estimators to reduce the number of resistance terms, and provide evidence that suggests in which cases shrinkage estimators should be preferred.

Chapter 2

I Learn, You Learn, We Gain Experience in Crop Insurance Markets*

Abstract

Participation in crop insurance programs is lowered by imperfect knowledge resulting in adverse selection and moral hazard problems. While the role of adverse selection and moral hazard has been explored extensively, the relevance of experience with insurance contracts is still underinvestigated. From farm-level data we estimate a dynamic discrete choice model of participation to investigate the role of experience. The methodology, coupled with exploratory analysis of the data, allows one to compare the relevance of different sources of experience in the crop insurance decision making process. We found that direct experience is a catalyst for insurance participation of medium and large farms. The experience indirectly acquired is also relevant, especially for small farms. Policy implications are discussed: in particular we discuss on the importance of bolstering uptake to exploit the advantages of the inertia and spillover effects that emerge from experience.

Keywords: Imperfect knowledge, Direct Experience, Indirect Experience, State dependence.

JEL: D23, G22, Q12, Q18

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I Learn, You Learn, We Gain
Experience in Crop Insurance Markets

L'expérience de chacun est le trésor de tous

(Gerard de Nerval)

During recent decades crop insurance programs have increased in prominence in developed and developing countries, and participation has been enhanced through subsidies (Mahul and Stutley, 2010). The world's largest risk management program - the United States of America's one - supports farmers through hedge funds, revenue insurance programs, mutual funds, and weather indexes. In the European Union, Member States have adopted autonomous national policies for assisting farmers through subsidized crop insurance or agricultural solidarity funds. Italy has a long tradition of farm subsidies, but has had difficulty in achieving crop insurance participation. Public intervention started in 1970, with the so called Fondo di Solidarieta Nazionale (FSN), intended to compensate farmers who had been affected by natural disasters. Starting in 2004 (through the Legislative Decree No. 102/2004) the intervention changed from *ex post* compensations to *ex ante* subsidies. Since its beginning, the FSN and the subsequent crop insurance program have been burdened by delays in payments, heavy bureaucracy and complex procedures that have discouraged participation in those programs. The burden of bureaucracy has increased during recent years, and it is currently a major issue for the Ministry of

Agriculture: "the *monster* of the bureaucracy over the past decade has *devoured* 100 thousand farms", headlined the main Italian business newspaper (*Il Sole 24 Ore*) on 27 February 2014. Such shocking news, attested by the main Italian small farm union (*Coldiretti*) motivated the government to allocate new budget on simplification interventions. Needless to say, bureaucracy in agriculture is a huge problem in Italy.

Participation in Italian crop insurance programs is generally low: around 10% of farmers participate in crop insurance programs. Insufficient subsidies, high costs of bureaucracy, ineffectiveness of Defense Consortia, and lack of experience with crop insurance contracts are some of the factors that have contributed to keep uptake low and farmers reluctant to participate in crop insurance programs. Adverse selection and moral hazard are also likely to characterize the Italian program. Previous studies have found that riskier farmers are keener to adopt insurance, but also that hazardous behavior (such as reduced input use) is the rule, rather than the exception (Di Falco et al., 2014; Santeramo et al., 2016).

Both adverse selection and moral hazard are substantial factors to be taken into account in insurance markets (Rothschild and Stiglitz, 1976), but they may not be exhaustive explanations for low participation rates. Chassagnon and Chiappori (1997) argue that imperfect knowledge and asymmetric information are likely to play a substantial role in the insurance decision-making process. Analyzing the automobile insurance market, Chassagnon and Chiappori (1997, p.75) found no evidence of moral hazard and adverse selection, while concluded on the role of imperfect knowledge and experience: "learning can be expected to modify this situation and, [...] driver's experience allows her to learn about her true ability faster than the insurance company."

Learning and knowledge diffusion are common channels for technology adoption in agriculture:

learning-by doing, learning from others (Foster and Rosenzweig, 1995), and social learning (Conley and Udry, 2010) have been shown to be catalysts of technology adoption in agriculture (Conley and Udry, 2001). In other words, the literature emphasizes the role of experience on technology adoption.

A vast number of studies have analyzed the determinants of participation in crop insurance programs (Skees and Reed, 1986; Goodwin, 1993; Smith and Goodwin, 1996; Sherrick et al. 2004; Tolhurst and Ker, 2015; Menapace et al., 2015). However, the existing research, with few exceptions (Cole et al., 2014) has stopped short of providing a deeper investigation into the role of experience on participation in crop insurance programs.

Because a deeper comprehension of the frictions to participation is a pressing issue for European Union (EU) policymakers, exploring the role of experience (and information) is a promising area of research. Several questions deserve investigation: Does imperfect information discourage participation? What is the role of experience on participation? Are there knowledge spillover effects among farmers?

We directly assess the role of experience through a dynamic model of participation. We investigate the relevance of direct and indirect experience with a detailed and long firm-level panel of Italian farms. We found that experience in crop insurance increases participation: the experience acquired in past harvest seasons is likely to reduce the imperfect knowledge on both sides, and thus enhance uptake. We conclude with a discussion directed to a better implementation of policy interventions in the reformed EU Common Agricultural Policy.

The Italian crop insurance system

Italian crop insurance policies are subsidized through EU funds. The market structure consists of one public-private coinsurance pool, twenty-five private insurance companies, and several mutual/cooperative entities participating in the agricultural insurance system. The main insurance company, specialized in agricultural insurance is FATA Assicurazioni, with 13% of market share in 2005 (in terms of insured values). Toro and ARA Assicurazioni follow by keeping around 7% of market share (Mahul and Stutley, 2010). While insurance companies may set their own premium rates, the companies coordinate pricing policies, and the maximum levels of insurance premiums eligible for a subsidy, with the Ministry of Agriculture, "*competition is predominantly based on quality of insurance services*" (Mahul and Stutley, 2010, Annex E p.118).

In order to be eligible for subsidies, the policies need to be stipulated (i.e. signed) in advance with respect to harvest time and before the designated deadlines: 31 March for crops with autumn-spring cycle; 31 March for permanent crops; 30 May for crop with Spring cycle; 15 July for summer crops with second harvest; 31 October for winter crops[1].

The individual demand for crop insurance contracts is aggregated through Defense Consortia, a local institution aimed at enhancing insurance uptake by matching insurers' supply and farmers' demand. Farmers are offered contracts from different insurance companies, and select the contract with the highest perceived quality. The existence of Defense Consortia is symptomatic of a clear asymmetric information that separate insurers and farmers.

Farmers may participate in crop insurance programs by stipulating one of the three types of insurance contracts, known as "monorisk", "multirisks" and "pluririsks". The monorisk policies

[1] The main crops with autumn-spring cycle in Italy are, among others, wheat, oat, barley; permanent crops are olive trees, grapevines, fruit trees; summer crops with second harvest are corn (second harvest), rice, soybean (second harvest); winter crops are broccoli, artichokes, cauliflowers.

cover one specific adversity and are no longer eligible for subsidies. Starting from 2014 the insurance policy must cover at least three climatic adversities eligible for pluririsks policies. The pluririsks policies ensure farmers against losses due to three or more climatic adversities, which need not be mutually exclusive. The multi-risks policies ensure against losses due to any type of climatic adversity included in the Annual Insurance Plan (Piano Assicurativo Annuale)[2]. The indemnities paid for mono- and pluri-risks policies are computed through qualitative and quantitative assessments of the percentage of losses due to the insured adversities; the multi-risk policy, also known as yield insurance, compensates farmers for losses when the realized yield is below the average historical yield.

During the decade 2000-2010 the total coverage (crops, livestock and farm's capital) has increased slowly but steadily from 1 million to 1.1 million hectares, the insured value grew by more than 40 % from 2004 to 2010, but the number of insured farmers has remained low.

Contracts that cover losses from multiple risks have increased in prominence. Between 2003 and 2009, the share of single-peril insurance contracts against hail has halved while the share of multiple risk contracts has increased. Following the European Union legislation, the current Insurance Program establishes a subsidy of up to 80% to insure the entire production or yield against losses larger than 30% of the historical average production (based on a 3-years average and computed by regional specialists). In all others situation the subsidies can be as high as 50% of the premium (Mahul and Stutley, 2010).

See table 1 on page 32

[2]In 2013 the Piano Assicurativo Annuale included losses due to drought, flood, freezing, hail, strong wind, frost, extreme weather fluctuations, and the most common phytopathologies. Source MIPAAF, available at https://www.politicheagricole.it/

Currently, the vast majority of contracts are purchased by farms located in Northern Italy: in 2010 the North accounted for 78% of the insured value, the Centre for 8% and the South for the remaining 14%. The number of insured crops has increased from 58 (in 2002) to 164 (in 2010), although four crops account for half of the total insured value: wine grapes, apple, rice, and corn (INEA, 2010).

Participation level and imperfect knowledge

Participation in crop insurance programs in Italy has remained low throughout recent decades. In order to increase participation policymakers tend to increase premium subsidies and expand the range of subsidized policies. Leveraging crop insurance demand with subsidies comes with large marginal costs. Glauber (2013) showed that in US the increase in subsidies boosted marginal costs from 3.31 dollars per acre with a subsidy of 2.73 dollar per acre in 1981-1994, to 25.99 with a subsidy of 7.76 in 1999-2003. Subsidizing farmers to buy insurance is costly and, in the long-run, it is a not sustainable policy . If high participation is the target, policymakers need to encourage the uninsured farmers to buy insurance, and the insured farmers to renew their contracts. It is therefore compelling to understand what factors contribute in bringing new clients in the market (e.g. spillover effects due to insurance popularity) and keeping old clients in the business (e.g. inertia in entrepreneurial strategies).

Imperfect and asymmetric information, through adverse selection and moral hazard, are the main factors that help explain low participation in insurance markets (Chiappori and Salanie, 2013). On one side riskier insurees have private knowledge on the risks they face: they will find it profitable to insure at the rate that insurers set for all customers. Such adverse selection mechanism pushes insurers to compensate their financial exposure by setting higher rates. Crop insurance literature has provided evidence on this type of adverse selection (Goodwin and Smith, 2013; Glauber, 2013)[3]. On the other side, insurers have private knowledge on the type of contract they offer[4] and therefore contracts may be perceived as not fully transparent to farmers. Theoretical explanation and evidence on such a reversed asymmetric information is provided by Chiappori and Salanie (2000, 2013)[5]. A third channel through which imperfect knowledge disfavors participation consists of high transaction costs in terms of high cost of bureaucracy - the cumbersome process to obtain subsidies for the premium, to claim reimbursements for yield losses, and the delays in payments for subsidies and claimed losses. This channel disfavors participation of farmers who are vulnerable to liquidity constraints (EC, 2001).

In all these cases the imperfect knowledge is likely to be resolved (at least partially), under the insurance contract, at the end of the harvest season. In other terms, a farmer who has stipulated an insurance contract will reveal (at the end of the season, through the realization of their business) some of their private knowledge in terms of riskiness. On the other end, the insurer will also reveal to the insured farmer (at the end of the season, by honoring the contract) some of their own private knowledge on the goodness of the contract. Finally, both the insurer and the farmer will gain experience on the bureaucracy of insurance at the end of each

[3] Following Chiappori and Salanie (2000), we test for asymmetric information for Italian farmers: data favor the presence of asymmetric information. In particular, we test whether $E[y_i|Insurance, X_i = x] > E[y_i|NotInsurance, X_i = x]$ with y_i being an outcome variable such as the number of claimed indemnities or the variability of the production. While the test does not disentangle adverse selection from moral hazard, it allows to test for asymmetric information (Einav, Finkelstein and Levin, 2010). Data support the asymmetric information hypothesis: insured farmers have larger variability in production.

[4] An alternative way to look at this problem is that insurance contract tend to be overcomplicated by commas, clauses and footnotes that are not transparent when the contract is accepted, or are not fully taken into consideration by the insuree.

[5] The quality-based competition of Italian crop insurance companies is likely to produce this type of asymmetric information. Crop insurers put large effort on advertising their insurance policies and provide complex contracts. Proof of the difficulty that farmers encounter when stipulating insurance contracts is the importance of defense consortia, both catalyst of the demand for insurance and impartial guarantors of the goodness of the contract.

season. All in all, it is likely that the more contracts are stipulated, the higher the experience on both sides, the lower the frictions due to imperfect knowledge. A similar mechanism has been hypothesized and tested in automobile (Cohen, 2005) and health care markets (Finkelstein and McGarry, 2006), which share with the crop insurance markets the characteristics of dealing with slowly depletable goods.

The model

Assume the profit of a farmer to be additively separable over two variables: the insurance decision ($I = \{0, 1\}$) and a set of variables (Z) describing the economic environment of the farm (such as land size, irrigation, number of crops, etc.). Farmers maximize the expected profit choosing input, farming strategies, and whether or not to purchase crop insurance. If insurance is actuarially fair, farmers' expected profit ($E[\pi(Z, I)]$) under insurance ($I = 1$) and under not insurance ($I = 0$) (conditional on other factors, Z) should be equal:

$$(1)\ E[\pi(Z, 1)] = E[\pi(Z, 0)]$$

However, insurance decision is a personal one, and it is influenced by several factors that are private for the farmer. The farmers' problem is to insure or not in order to maximize (subjective) expected profits. Assuming risk aversion, they will choose insurance if the expected utility from profit with insurance is greater than the expected utility from profit without insurance[6]:

$$(2)\ E[U(\pi(Z, 1))] > E[U(\pi(Z, 0))]$$

[6] Our data support the hypothesis that expected profit under insurance is greater than expected profit under no insurance.

What is left out so far is the role of private information[7]. Information, learning-by doing and learning from others (Foster and Rosenzweig, 1995), social learning (Conley and Udry, 2010) are likely to influence technology adoption in agriculture (Conley and Udry, 2001): private information includes risk aversion (μ) and familiarity (Ω) with the insurance scheme which is gained through experience - either directly or indirectly - and the observation of past realizations. We assume the probability of participating in the insurance scheme to be a function of risk aversion (μ) and familiarity (Ω) , and therefore farmers will choose insurance if the expected utility with insurance is greater than the expected utility without insurance:

$$(3)\ E[U(\pi(Z,1),\mu,\Omega)] > E[U(\pi(Z,0),\mu,\Omega)]$$

Risk averse farmers are more likely to adopt crop insurance, while the role of familiarity with an insurance scheme is unclear. A farmer who is better informed on the functioning of insurance contracts may be more or less willing to adopt crop insurance, depending on how well the insurance program works, and on how much are the net benefit (or loss) for participating farmers. Hence, *a priori* we cannot conclude on the role of familiarity with the program. Familiarity ($\Omega_{i,t} = \{Exp_{i,t}, Exp_{-i,t}\}$) is gained through direct ($Exp_{i,t}$) and indirect ($Exp_{-i,t}$) experience: a farmer (i) gains direct experience by participating in the program; he may also gain indirect experience from others ($-i$), by interacting with farmers who have experienced the insurance scheme[8].

Foster and Rosenzweig (1995) argue that the existence of learning by doing and learning from neighbors' experience enhance technology adoption. Similarly, it is likely that private and shared

[7] Other non-private information factors such as liquidity, product quality, trust, risk aversion, wealth, or culture, are captured by the farmers fixed effects.

[8] To make it clearer, farmers are expected to know Ω_t at time t. Direct experience is gained only by stipulating an insurance contracts, while indirect experience is transferred from insured farmers (at time $t-1$) to uninsured farmers (at time $t-1$) and is exploited at time t.

information would increase the likelihood of insurance. Testing for these hypotheses is a relevant empirical question.

The reduced form expression of the probability for farmer i to stipulate an insurance contract (in period t) depends on risk aversion (μ_i), familiarity ($\Omega_{i,t}$), and other (control) factors (Z_t):

$$(4)\ Prob(I_{it} = 1|\mu_i, \Omega_{i,t}, Z_{i,t}) = f(\mu_i, Exp_{i,t}, Exp_{-i,t}, Z_t)$$

Experience

Farmers take advantage of gained experience to make their decision on insurance. Experience is gained by insuring or by collecting information from others. Direct experience is expected to convey more information than indirect experience and therefore to be a stronger catalyst for participation than indirect experience. Empirically, we expect to observe coefficients for direct experience to be positive and of larger magnitude. We assume experience can be purely transitory (if the knowledge accumulation process has very short memory), or permanent (if the knowledge accumulation process has infinite memory)[9].

If direct experience is purely transitory if farmers only benefit from the information gained in the previous year, and the variable direct transitory experience (DTE) reduces to the lagged dependent variable:

$$Exp_{i,t} = I_{i,t-1}$$

On the contrary, if direct experience is permanent, the information gained through insurance lasts forever, therefore the timing of insurance is not relevant once farmers have purchased

[9] By modeling the variable with polar cases we are able to conclude on the role of experience. We also use a continuous variable to assess the role of cumulated experience. Results are commented and reported in appendix.

insurance. The variable direct permanent experience (DPE) reduces to an indicator function equals to one if the lagged dependent variable has been one at least once in previous periods:

$$Exp_{i,t} = \begin{cases} 1 & \text{if } \sum_{l-1}^{T} I_{i,t-l} > 0 \\ 0 & \text{otherwise.} \end{cases}$$

with T standing for the total number of years. Similarly, indirect experience is assessed through two specifications: the number of farmers located in the same Region who have stipulated a crop insurance contract in the previous year (indirect transitory experience, ITE) , and the number of farmers located in the same Region who have stipulated a crop insurance contract at least once in the previous years (indirect permanent experience, IPE). In other terms, the indirect experience variable is as proxy of insurance popularity in a specific Region. The first case (ITE) proxies the amount of indirect information a farmer is exposed to if experience is purely transitory, therefore the variable indirect experience is the sum of the lagged dependent variables for all close farmers (where for close farmers it is meant that farmers are located in the same region):

$$Exp_{-i,t} = \sum_{j=1}^{n_x} I_{j,t-1},\ j \neq i\ ,\ j = 1, ..., n_x$$

where n_x represents the number of farmers in the same Region. The second case (IPE) reflects the amount of indirect information a farmer is likely to receive if experience is permanent; the variable indirect experience is the sum, over close farmers, of the indicator functions equals to one if the lagged dependent variable has been one at least once in previous periods:

$$Exp_{-i,t} = \sum_{j}^{n_x} I_j,\ j \neq i\ ,\ j = 1, ..., n_x$$

with

$$I_j = \begin{cases} 1 & \text{if } \sum_{t=l}^{T} I_{j,t-l} > 0 \\ 0 & \text{otherwise.} \end{cases}$$

We assumed that information is spread at Regional level for several motivations: first, the historical data on production to compute multi-risk policies are provided by Regions; second, the insurance contracts are offered by regional consortia; third, the requests for subsidies need to be directed to the Regional offices which are also in charge of processing reimbursements to farmers when yield losses occur. While the binary specification for experience is admittedly simple, we check for the impact of cumulated experience and found no releant differences. Detailes are provided in appendix.

Econometric framework

Our data comprise six years, up to 2010, of thousands of farms representative of the entire national population of Italian farms[10]. The (strongly balanced) panel data allows one to estimate the dynamics of the decision-making process in the insurance program. A linear approximation of equation for participation in crop insurance contracts can be easily estimated:

$$(5)\ Prob(I_{it} = 1|\mu_i, \Omega_{i,t}, Z_{i,t}) = \Phi(\gamma Exp_{i,t} + \delta Exp_{-i,t} + Z'_{i,t}\beta + \mu_i)$$

Given that $Prob(I_{it} = 1|\Psi_i, \Omega_{i,t}, Z_{i,t})$ is not observed, and experience is gained through participation (e.g. $Exp_{i,t} = I_{i,t-1}$), the model is estimated as a dynamic probit model.

[10]More specifically, the farms included in the dataset (a subsample of the FADN dataset) are selected on the basis of a survey plan, conducted by each EU Member State of the EU in order to guarantee the representativeness. In particular, the dataset is constructed after stratification of the universe of farms, according to the type of farms (e.g. type of crops, livestocks, etc.), and the geographical distribution.

It must be noted that even if the error terms of the probit model are assumed serially independent, there exists serial correlation induced by the time-invariant term (μ_i)[11]. Since the dependent variable (I_{it}) is a binary variable (the decision to insure or not), we normalize it by imposing a unitary value for the variance of the error term ($\sigma_u^2 = 1$). The (conditional) transition probability for i at time t is as shown in Heckman (1981)[12]:

$$Prob(I_{i,t} = 1|\mu_i, I_{i,t-1}, Exp_{-i,t}, Z_{i,t}) = \Phi[(\gamma I_{i,t-1} + \delta Exp_{-i,t} + Z'_{i,t}\beta + \mu_i)(2I_{i,t} - 1)]$$

where Φ is the cumulative distribution function of the standard normal distribution. The model requires an assumption on the relationships between the initial observation $(I_{i,1})$ and the unobserved heterogeneity (μ_i). The simplest solution is to assume the initial observation exogenous, but this is a strong assumption for the vast majority of datasets, whose start period does not coincide with the start of the process[13]. If the initial observation is correlated with the unobserved heterogeneity, the standard Random Effects (RE) probit estimator is inconsistent and overestimates γ (i.e. the state dependence is overestimated). Following Heckman (1981), we use a reduced form equation for the initial observation $(I_{i,1})$ with instruments $(X_{i,1})$ which includes the set of explanatory variables $(Exp_{-i,1}, Z_{i,1})$ for the main model and exogenous instruments $(x_{i,1})$. The instruments are assumed to be correlated with the fixed-effects and uncorrelated with the error term.

The joint probability for the sequence of $I_{i,t}$ is as follows (Heckman, 1981):

[11] This specification implies equi-correlation between $v_{i,t} = \mu_i + u_{i,t}$ in any two different periods: $\lambda = Corr(v_{i,t}, v_{i,s}) = \frac{\sigma_\mu^2}{\sigma_\mu^2 + \sigma_u^2}$. We may also estimate a more general model by relaxing the assumption of no autocorrelation of the error term. The model requires T-dimensional integrals of normal densities but, although feasible, it requires a great computation effort (Stewart, 2005). The estimates of the state dependence coefficient are generally slightly lower, therefore the model we estimate represents an upper bound, a conservative measure to not overestimate the effects of experience.

[12] The result follows from the the normality assumption of the probit model error term $(u_{i,t})$

[13] If the assumption is correct, the model can be estimated as a standard Random Effects (RE) Probit Model.

$$\Phi[(X'_{i,1}\zeta + \theta\mu_i)(2I_{i,t}-1)]\Pi_{t=2}^{T}\Phi[(\gamma I_{i,t-1} + \delta Exp_{-i,t} + Z'_{i,t}\beta + \mu_i)(2I_{i,t}-1)]$$

and the model is estimated by maximizing, over $\mu^* = \frac{\mu}{\sigma_\mu}$ and $\sigma_\mu = \sqrt{\frac{\lambda}{1-\lambda}}$, the likelihood overall all farmers:

$$\Pi_i \int_{\mu^*} \Bigg\{ \Phi[(X'_{i,1}\zeta + \theta\mu^*\sigma_\mu)(2I_{i,t}-1)]$$

$$\Pi_{t=2}^{T}\Phi[(\gamma I_{i,t-1} + \delta Exp_{-i,t} + Z'_{i,t}\beta + \mu^*\sigma_\mu)(2I_{i,t}-1)] \Bigg\} dF(\mu^*)$$

The integral has been evaluated using the Gaussian-Hermite quadrature (Butler and Moffitt, 1982 ; Stewart, 2005). Following Wooldridge (2005), we evaluate the estimates by computing the average partial effects (APE) of state dependence ($I_{i,t-1}$). We multiply the coefficient $\hat{\gamma_\mu}$ by a weighted sample average of the distribution:

$$N^{-1}\sum_{i=1}^{N}\Phi(\hat{\gamma_\mu} I_{i,t-1} + \hat{\delta_\mu} Exp_{-i,t} + Z'_{i,t}\hat{\beta_\mu})\hat{\gamma_\mu}$$

where the subscript μ indicates that the parameter need to multiplied by $(1+\sigma_\mu^2)^{-1/2}$, and in particular $\hat{\gamma_\mu} = \gamma(1+\sigma_\mu^2)^{-1/2}$, $\hat{\delta_\mu} = \delta(1+\sigma_\mu^2)^{-1/2}$, $\hat{\beta_\mu} = \beta(1+\sigma_\mu^2)^{-1/2}$, and the coefficients γ, δ and β are the MLE estimates.

Empirical results

Data

Our analysis is based on a dataset drawn from the Farm Accountancy Data Network, a databank representative of European commercial agriculture. The dataset employed in the present analysis consists of a strongly balanced panel data of 18,382 observations: 2,626 Italian firms, continuously observed from 2004 to 2010, whose main activity is farming, located in nineteen different Italian Regions.

Crop insurance is not much diffused and uptake is low. Our dataset shows that a vast majority of farmers do not stipulate insurance contracts, and only a small fraction of insured farmers stipulated contracts for consecutive years (table 2). Interestingly, a small fraction of farmers (around 5%) has been insured for at least 4 consecutive years (from 2004 to 2007), dropping coverage in 2008, when new insurance contracts (e.g. insurance contracts with no threshold or against plant diseases) started to be offered. In our dataset demand for insurance dropped by 5% in 2009 (with respect to the sample average), with a recovery to up 9% in 2010. A similar dynamic is observed for the entire insurance market: in 2009 the number of certificates dropped by 2.5% with respect to 2007, but the market started to recover from 2010, as attested by the positive trend of total insured value.

See table 2 on page 33

Our dataset provides yearly information on land size, altitude, farmers' age, diversification of farming activities, adoption of irrigation, farms' revenue and cost. We compute revenue variability as standard deviation of farms' revenue (over the entire period, 2004-2010), and expected premium per hectare[14]. Farm-level premium rates are not available, and not available for farms not insuring. We compute the variable "Expected Premia" by averaging, across Regions and farming systems, the crop-specific total premia. The aggregate premium is a proxy of the premium farmers are expected to pay, and a proxy for the level of riskiness for all farms of a given type and located in a specific region. The approach is similar to that adopted by Goodwin

[14]Computed by averaging within regions and farming systems the (crop-specific) total premia

(1993) and Santeramo et al. (2016). Descriptive statistics of experience variables and control factors are shown in tables 3 and 4.

See table 3 on page 34

See table 4 on page 35

The farm size is quite small: for instance the average size of farms in Emilia-Romagna, Toscana, Lombardia and Piemonte, home well known agro-food products, is only eighteen hectares, compared to an average size of US farms that is twenty five times larger (500 hectares). A vast majority of farms are not insured, and not irrigated with percentage at regional level that do not exceed, respectively, 23 and 43 percent (excluded Liguria).

Experience

The estimates using a pooled probit model, a standard random effects probit model and a random effects model a-la-Heckman confirm that the effects of experience are overestimated if simple estimators are adopted (table 5). While the signs of control factors are unaltered over the three estimators, the coefficients for the direct and indirect experience are much larger if a pooled or a standard random effects probit model is adopted. The pooled probit estimates ignore the cross-correlation between the individual composite errors in different periods. When we control for the endogeneity of initial conditions (Heckman, 1981; Stewart, 2005), the effect is largely reduced. We estimate all remaining models with the Heckman model[15].

[15] A further refinement may be to allow for autocorrelation of errors: unfortunately the estimation of the model with autocorrelated errors is computationally not feasible and does not converge after 72 hours and 40 iterations on a 3.4GHz 64-bit machine, with 16 GB of RAM. On the same machine the model with no autocorrelated errors is solved in less than 30 minutes. In order to learn on the potential endogeneity bias due to possible autocorellation of the errors, we estimated a linear probability model: the overall picture is unaltered in that coefficients on direct and indirect experience remain positive and statistically significant.

See table 5 on page 35

We compute average partial effects (APE) for direct and indirect experience. A consistent estimator for the APE is the change in the probability distribution (PDF) function evaluated at the sample mean, after normalization of the maximum likelihood (MLE) coefficients. Empirically, we multiply the MLE parameters by $(1+\hat{\sigma_\mu^2})^{-1/2}$, and evaluate the PDF under different values for "Experience" at the sample mean[16].

The measures for direct and indirect experience are positive and statistically significant. As we should expect, the relevance of direct experience is larger than the relevance of indirect experience. Farmers who have experienced crop insurance contracts are much more likely to purchase insurance with respect to farmers who have never experienced insurance contracts, even if located in Regions where crop insurance programs are popular. Farmers with direct experience in crop insurance are 10% more likely to buy insurance with respect to a previously uninsured farmer; the probability of insurance may raise by 0.09% due the indirect experience, however the coefficient is statistically not different from zero[17].

Transitory direct experience is stronger than permanent direct experience: the likelihood of purchasing insurance is as high as 10% if farmers have experienced insurance during the previous season, and only 3.5% if they had experience in earlier seasons[18]. The same conclusion cannot be drawn for indirect experience: transitory experience is not relevant, whereas permanent experience increases the likelihood of insurance by 0.003%. This result reflects the intuition that indirect experience is slowly transmitted, or put differently, that the spillover effects due

[16] Knowing that $\lambda = \frac{\sigma_\mu^2}{\sigma_\mu^2+\sigma_u^2}$ and $\sigma_u^2 = 1$, we compute $\hat{\sigma_\mu^2} = \frac{\lambda}{1-\lambda}$.

[17] The estimate refers to a change from no insured neighbor to the average regional value of insured farmers. The effect is economically and statistically not significant. However, this impact refers to the transitory indirect effect that, as will be shown, is the lowest estimate for the effect of indirect experience.

[18] The effect is jointly due to the lower estimates of γ and λ.

to the familiarity with a crop insurance program are exploited after one or more years.

See table 6 on page 36

Other factors influence participation in crop insurance markets. We found that large and irrigated farms are more likely to be insured, while farms located at high altitude are less likely to be insured. The results are consistent with the existing literature on crop insurance (Goodwin, 1993; Smith and Goodwin, 1996; Enjolras and Sentis, 2011; Foudi and Erdlenbruch, 2012; Singerman, Hart and Lence, 2012)[19]. In addition we observe a positive correlation between participation in insurance schemes with the variables "revenue variability" and "expected premium": the higher the revenue variability, the higher the likelihood of stipulating an insurance contract; the higher is the expected premium, which reflects a higher level of underlying risk, the higher is the participation in crop insurance program. This seemingly counterintuitive result is explained by the crop data scarcity which imposes higher premiums in Italy (Shen et al., 2016). In order to disentagle the effects of premiums we would need to rely on expert knowledge of the degree of riskiness: unfortunately, those data are not available.

By estimating the model for North, Centre and South we gain further insights: the role of experience is less relevant where participation is higher (e.g. North). Two (opposite) explanations help understanding this finding. On one hand, since participation is likely to be driven by diffused knowledge of insurance contracts, the higher the participation, the lower the relevance of direct experience should be[20]. On the other hand, if insured farmers are self-selected, the farmers that are insured in a low-participation area are expected to have a much stronger experience

[19] Although not statistically significant, we found a negative relationships between farmers' age and crop diversification with participation in crop insurance programs. The results are consistent with Smith and Goodwin (1996), Foudi and Erdlenbruch (2012), among others.

[20] This explanation is in line with the arguments raised by Foster and Rosenzweig (1995) or Conley and Udry (2010).

on insurance (and possibly higher benefits from insurance), thus they are more likely to insure for consecutive years[21]. The lower relevance of indirect experience in the North, where uptake is higher, also suggests that there are decreasing returns to indirect experience, and therefore the information campaigns should be replicated on a regular basis[22].

See tables 7 and 8 on page 37

Direct and indirect experience have different impacts for farms differing in (economic or land) size. The vast majority of farmers (medium farms) takes advantage of direct and indirect experience: the higher the experience the higher the likelihood to participate in the program. For small farmers direct experience is (relatively) less important, whereas the indirect experience is more relevant. In order to understand this result, you need to consider that small farms are usually owned by professionals with more remunerative off-farm opportunities: their effort (and time invested) on farming activities is relatively low, and farming strategies may be guided by neighbors' suggestions or common wisdom. As one should expect, insurance decisions for large farms are not influenced at all by indirect experience, but it is also true that direct experience is very important. Lastly, transitory experience is more important than permanent experience: a result that is consistently explained by the dynamic alternation of managing directors in large-size farms rather than in small-size farms.

Policy reflections

To the extent that indirect experience helps enhance participation, policymakers should promote policies aimed at facilitating exchange of knowledge and experience among farmers.

[21] This explanation is in line with the Roy's model of self-selection (Roy, 1951; Heckman and Sedlacek, 1985).

[22] Similarly, the direct experience shows decreasing returns. The results in appendix show that, *ceteris paribus*, the cumulated direct experience has a negative and statistically significant coefficient.

We found that direct experience is a strong catalyst for participation. Participation may be also enhanced through the indirect experience channel: the higher is the participation, and the knowledge capital accumulated in a Region, the higher the likelihood that farmers will enter in the insurance market. Policymakers should pursue strategies to bolster participation (potentially even at a significant short-run cost) to take advantage of the inertia and the spillover effects that emerge from experience. Once experience is gained, there should be less friction to participate in insurance contracts, and therefore the subsidies may be lowered for farmers who have already experienced insurance contracts.

Ad hoc measures should be implemented to enhance participation of small farms, which (traditionally) are more reluctant to adopt insurance and less likely to gain from direct experience. For instance, participation of small farms may be incentivized through one-time highly subsidized premia, through targetted informative campaigns, or by proposing discounts on the premium for new clients. Lastly, multi-year insurance contracts (e.g. at lower premia) may be appealing for uninsured farms[23].

What have we learned from farmers' experience?

Participation in crop insurance programs has been low for decades in the EU, despite large subsidies granted by the governments. Further increases in subsidies, implemented during recent years in the US, are likely to create distortive effects: subsidizing risk leads agents to assume more risks and costs to raise rapidly (Smith and Goodwin, 1996; Goodwin and Smith, 2013). A different, complementary, strategy to approach the problem is highly desirable for policymakers

[23] All in all the proposed measures are not new: proposing competitive tariffs for new clients is the normality for companies in the markets of telephony, or car insurance. We are just proposing to import such a way of making business in a market that presents several frictions to entry for small farmers.

and taxpayers.

The factors affecting the demand for agricultural insurance have been extensively studied, and the literature has reached a consensus on the role of assets (e.g. Goodwin, 1993; Smith and Goodwin, 1996; Enjolras and Sentis, 2011; Singerman, Hart and Lence, 2012;), human capital (Smith and Bacquet, 1996), and farmers' strategies (Foudi and Erdlenbruch, 2012) on insurance decisions. However, little evidence has been provided to establish the role of experience in the crop insurance decision making process. Indeed, learning by doing and learning by watching others are mechanisms that may stimulate technology adoption in agriculture (Foster and Rosenzweig, 1995; Conley and Udry, 2010), and they are likely to help reducing the imperfect and asymmetric information that characterize insurance markets (Chiappori and Salanie, 2013).

We use a detailed farm-level dataset to assess how learning-by-doing and learning-from-others mechanisms influence the crop insurance decision making process. We disentangle the role of direct and indirect experience.

We conclude that experience in crop insurance increases participation. Intuitively, the direct experience acquired during the previous harvest season is likely to reduce the imperfect knowledge and act as strong catalyst for participation; the direct experience acquired over time is also relevant. The indirect experience is also valuable: the experience that farmers gain when they stipulate insurance contracts benefits them, their neighbors and the insurers. As consequence, participation in the area tends to increase. Relevant geographic differences exist, and policymakers should take them into account to promote participation. The role of information, in terms of experience, is also differing by farm size. Policymakers should take this dimension into consideration to improve the efficacy of planned interventions.

The importance of imperfect information has been highlighted, but a few possible limitations need to be underlined. The external validity of our results is limited by the lack of information about the type of contracts stipulated. However, even if such data were available, the main results should not change: the asymmetric information between farmers and insurers is partially resolved through experience, and this in turn stimulates insurance coverage renewals. To the extent that increasing uptake in crop insurance markets is a main goal for policymakers in most developed countries, understanding how imperfect knowledge is resolved represents a promising area for future research. With data availability increasing more and more, understanding how farmers decide to switch among contracts is also an important research question to help planning policy interventions. Extension for this analysis should move in this direction.

Sensitivity Analyses

In order to check for potential biases induced by the omission of year dummies or regional dummies, we provide results from several models in which year dummies, and regional dummies have been introduced (tables 9, 10 and 11). The estimates have been conducted by using the standard RE model presented in the manuscript. The results show that the inclusion of those dummies do not alter the significance and the signs of the variables of interests: direct and indirect experience. Indeed, the last column of the table shows that by controlling for year dummies and regional dummies, the differences in the coefficients of direct and indirect experience are exacerbated, suggesting once more that direct experience is a stronger catalyst for participation in crop insurance programs.

We also check for the impact of cumulated experience and, as expected, we found that direct experience decreases over time, while cumulated indirect experience is not statistically significant. The results corroborate our previous findings (shown in table 7 and commented in the manuscript).

We runned sensitivity analyses to analyze if and how the effects of direct experience differ with farms and farmers' characteristics, farmers' strategies and market conditions, and if and how the effects of direct and indirect experience have changed over time.

Direct experience is more relevant for farms located in high altitude as they are likely to be isolated. Experience is less important for older farmers, as they are likely to have stronger inertia in their farming strategies. Direct and indirect experience are substitute. Finally, the higher the expected premium (proxy of the riskiness of the area/farming system), the higher the relevance of direct experience.

Experience is more relevant from 2004 to 2006 than from 2007 to 2010, exception made for the permanent direct experience. The introduction of new contracts (with no threshold or against plant diseases), and the drop in coverage are possible explanations for these trends: with new contracts offered the relevance of experience gained in recent years, and the relevance of indirect experience, is reduced; on the contrary it becomes more relevant the overall (permanent direct) experience on crop insurance contracts.

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Tables

Table 2.1: Characteristics of the Italian insurance market

		2004	2006	2008	2010
The size of the insurance market					
Number of policies	(.000)	212	211	265	208
Insured Land	(.000 ha)	982	1125	1450	1153
Insured Value	(mln €)	3710	3789	5436	5313
Market shares by types of contract					
Monorisk	(%)	92.0	77.4	53.7	50.2
Pluririsk	(%)	7.7	19.6	40.0	46.6
Multirisk	(%)	0.3	2.9	6.3	3.3

Source: SicurAgro - ISMEA. Available at www.ismea.it

Table 2.2: Crop insurance participation by year

	2004	2005	2006	2007	2008	2009	2010	Total
Consecutive contracts								
0	2,427	2,424	2,410	2,402	2,552	2,539	2,375	17,129
1	199	52	48	45	15	22	196	577
2		150	33	27	10	9	15	244
3			135	25	9	9	6	184
4				127	8	9	7	151
5					32	6	3	41
6						32	3	35
7							21	21

Number of farms insured for zero, one or consecutive years

Table 2.3: Descriptive statistics of experience variables (averages)

	DTE	DPE	ITE	IPE
Valle DAosta	0.00	0.00	0.00	0.00
Piemonte	0.06	0.06	9.12	12.00
Lombardia	0.10	0.18	15.00	31.41
Trentino	0.44	0.51	19.52	42.01
AltoAdige	0.29	0.38	10.06	25.42
Veneto	0.09	0.12	12.58	31.11
Friuli-Venezia-Giulia	0.14	0.22	6.57	18.53
Liguria	0.01	0.01	0.47	3.14
Emilia Romagna	0.02	0.09	7.84	2.85
Toscana	0.08	0.09	5.05	11.51
Marche	0.01	0.01	0.57	0.71
Umbria	0.16	0.25	5.05	6.71
Lazio	0.01	0.03	0.68	1.57
Abruzzo	0.05	0.09	4.48	13.01
Molise	0.02	0.05	1.92	7.42
Calabria	0.00	0.00	0.00	0.00
Puglia	0.09	0.15	9.18	21.02
Basilicata	0.02	0.01	1.61	1.14
Sicilia	0.05	0.06	6.50	14.02

The averages are for the entire sample period: 2004-2010. DTE stands for Direct Transitory Experience; DPE stands for Direct Permanent Experience; ITE stands for Indirect Transitory Experience; IPE stands for Indirect Permanent Experience.

Table 2.4: Descriptive statistics of explanatory variables

		Mean	St. Dev.	Min	Max
Land Size	(ha)	28.81	60.62	0.11	1072.62
Altitude	(Dummy)	0.50	0.49	0	1
Age	(Years)	55.45	13.63	18	89
Revenue variability	(.000)	31.73	124.64	0.21	292.46
Diversification	(Dummy)	0.78	0.41	0	1
Irrigation	(Dummy)	0.27	0.44	0	1
E[Premium/Ha]	(.000)	0.14	0.09	0.01	0.32

Table 2.5: Comparison of Estimators

	Pooled Probit	Standard RE	Heckman
Direct Experience	2.25*	2.14*	1.08*
	[0.00]	[0.00]	[0.00]
[1em] Indirect Experience	0.0057*	0.0058*	0.0031
	[0.01]	[0.02]	[0.32]
Control factors	Yes	Yes	Yes
Logit λ			0.44*
			[0.00]
Ln θ			0.73*
			[0.00]
λ			0.61*
			[0.00]
θ			2.07*
			[0.00]
Observations	13926	13926	18382
Log-Likelihood	-2176.2	-2174.2	-2594.6

p-values in brackets. $^{+}$ $p < 0.10$, * $p < 0.05$
Estimations refer to transitory direct experience and transitory indirect experience. Control factors include land size, altitude, age, revenue variability, diversification, irrigation, and expected premia. For sake of comparability, the last column reports the APE computed as described in the manuscript. The estimate of λ implies that 61% of the composite error variance is due to individual-specific effects. The parameter is originally estimated as a logit transformation ($ln(\frac{\hat{\lambda}}{1-\hat{\lambda}})$) and therefore it is computed as transformed as follows: $\lambda = \frac{e^{\hat{\lambda}}}{1+e^{\hat{\lambda}}}$. The parameter θ is statistically greater than one indicating that the composite error (v_{it}) is correlated with the individual-specific effects.

Table 2.6: Alternative Experience Measures

	1	2
Transitory Direct Experience	1.07*	
	[0.00]	
Permanent Direct Experience		0.92*
		[0.00]
Transitory Indirect Experience	0.0031	
	[0.32]	
Permanent Indirect Experience		0.0076*
		[0.00]
Land Size	0.0020*	0.0019*
	[0.00]	[0.00]
Altitude	-0.15^{+}	-0.093
	[0.10]	[0.30]
Age	-0.0027	-0.0067*
	[0.40]	[0.03]
Revenue variability	0.53^{+}	0.38
	[0.08]	[0.19]
Diversification	-0.014	-0.0026
	[0.88]	[0.98]
Irrigation	0.88*	0.91*
	[0.00]	[0.00]
E[Premium/Ha]	0.91^{+}	0.72
	[0.06]	[0.12]
Logit λ	0.44*	0.24
	[0.00]	[0.25]
Ln θ	0.73*	0.38*
	[0.00]	[0.00]
λ	0.61*	0.56
	[0.00]	[0.26]
θ	2.07*	1.46*
	[0.00]	[0.00]
Observations	18382	18382
Log-Likelihood	-2594.6	-2655.8

p-values in brackets. $^{+}$ $p < 0.10$, * $p < 0.05$
Control factors include land size, altitude, age, revenue variability, diversification, irrigation, and expected premium per hectare. Land size is expressed in hectares, age in years, sigma revenues is expressed in mln of euro, and expected premium per hectare in .000 of euro. Altitude, diversification and irrigation are dummy variables.

Table 2.7: Geographical analysis

	North		South		Centre	
Transitory Direct Experience	1.01*		1.43*		1.63*	
	[0.00]		[0.00]		[0.00]	
Permanent Direct Experience		0.94*		1.10*		1.76*
		[0.00]		[0.00]		[0.00]
Transitory Indirect Experience	0.007*		-0.017^{+}		0.0038	
	[0.04]		[0.06]		[0.85]	
Permanent Indirect Experience		0.0091*		-0.010		0.0050
		[0.00]		[0.17]		[0.73]
Control factors	Yes	Yes	Yes	Yes	Yes	Yes
Logit λ	0.46*	0.11	-0.12	-0.076	-0.77	-2.61
	[0.01]	[0.69]	[0.65]	[0.88]	[0.15]	[0.18]
Ln θ	0.60*	0.31*	3.14	0.76*	2.43*	2.48
	[0.00]	[0.02]	[0.35]	[0.01]	[0.04]	[0.70]
Observations	8785	8785	7077	7077	2520	2520
Log-Likelihood	-1731.4	-1770.6	-723.9	-753.5	-238.0	-250.2

p-values in brackets. $^{+}$ $p < 0.10$, * $p < 0.05$

Control factors include land size, revenue variability, expected premia, and indirect experience.

Table 2.8: Farm size analysis

	Small		Medium		Large	
Transitory Direct Experience	1.08*		1.16*		1.16*	
	[0.00]		[0.00]		[0.00]	
Permanent Direct Experience		0.99*		1.13*		1.03*
		[0.04]		[0.00]		[0.00]
Transitory Indirect Experience	0.019*		0.0039		0.0038	
	[0.02]		[0.28]		[0.53]	
Permanent Indirect Experience		0.020*		0.0087*		-0.0050
		[0.00]		[0.00]		[0.77]
Control factors	Yes	Yes	Yes	Yes	Yes	Yes
Logit λ	0.83^{+}	0.35	0.13	-0.42	0.028	-0.036
	[0.05]	[0.64]	[0.48]	[0.19]	[0.93]	[0.93]
Ln θ	1.21	0.68^{+}	0.98*	0.50*	0.97*	0.36
	[0.12]	[0.07]	[0.00]	[0.00]	[0.02]	[0.10]
Observations	3626	3626	11102	11102	3654	3654
Log-Likelihood	-315.2	-322.3	-1688.2	-1733.2	-693.6	-714.4

p-values in brackets. $^{+}$ $p < 0.10$, * $p < 0.05$

Control factors include expected premia. Small farms (20% of sample size) are less than 5 hectares and produce, on average, a revenue less than 20K euro per year; large farms (20% of sample size) are larger than 35 hectares or produce, on average, a revenue higher than 1000K euro per year.

Table 2.9: Year and Regional Dummies, and Cumulated Experience

	Year and Region FE			Cumulated Experience	
Direct Experience	2.39*	1.89*	2.18*	2.13*	2.13*
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
[1em] Indirect Experience	0.030*	0.030*	0.018*	0.018*	0.018*
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Cumulated Direct Experience				-0.24*	-0.24*
				[0.00]	[0.00]
Cumulated Indirect Experience					0.001
					[0.33]
Control factors	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Region FE	No	Yes	Yes	Yes	Yes
Observations	13926	13926	13926	13926	13926
Log-Likelihood	-1912.7	-1909.1	-1778.75	-1631.18	-1630.71

p-values in brackets. $^{+}$ $p < 0.10$, * $p < 0.05$

The models are estimated using a standard RE probit model (second column of table 4), Estimations refer to transitory direct experience and transitory indirect experience. The cumulated direct experience is the sum of consecutive years of uptake. The cumulated direct experience is the sum, by Region, of farmers' cumulated direct experience. Control factors include land size, altitude, age, revenue variability, diversification, irrigation, and expected premia.

Table 2.10: Direct Experience - Interaction terms

	Characteristics	Strategy	Market
Transitory Direct Experience (TDE)	1.24*	0.70*	0.95*
	[0.00]	[0.00]	[0.00]
Transitory Indirect Experience (TIE)	0.0021	0.0044	0.0046
	[0.51]	[0.15]	[0.24]
TDE X Altitudine	0.24^{+}		
	[0.06]		
TDE X Age	-0.0048		
	[0.32]		
TDE X Land Size		0.00058	
		[0.59]	
TDE X σ Revenue		-0.00000088	
		[0.10]	
TDE X Diversification		0.14	
		[0.33]	
TDE X Irrigation		0.69*	
		[0.00]	
TDE X E[Premia/ha]			0.0014*
			[0.04]
TDE X TIE			-0.0069
			[0.26]
Control factors	yes	yes	yes
Logit λ	0.43*	0.38*	0.41*
	[0.00]	[0.01]	[0.01]
Ln θ	0.73*	0.78*	0.76*
	[0.00]	[0.00]	[0.00]
Observations	18382	18382	18382
Log-Likelihood	-2592.2	-2576.2	-2592.0

p-values in brackets. $^{+}$ $p < 0.10$, * $p < 0.05$
Control factors include land size, altitude, age, revenue variability, diversification, irrigation, and expected premia..

Table 2.11: Temporal analysis

	Pre-2007		Post-2007	
Transitory Direct Experience	0.87*		0.25+	
	[0.00]		[0.09]	
Transitory Indirect Experience	0.033*		-0.024*	
	[0.00]		[0.00]	
Permanent Direct Experience		0.56		1.09 *
		[0.10]		[0.00]
Permanent Indirect Experience		0.052*		0.0086*
		[0.00]		[0.01]
Control factors	yes	yes	yes	yes
Logit λ	1.75*	2.24*	0.87*	-0.051
	[0.00]	[0.00]	[0.00]	[0.82]
Ln θ	0.024	-0.33+	0.55*	0.033
	[0.92]	[0.05]	[0.02]	[0.88]
Observations	7878	7878	10504	10504
Log-Likelihood	-1240.9	-1229.6	-1646.9	-1623.0

p-values in brackets
Control factors include land size, revenue variability, expected premia, and indirect experience.
$^{+}$ $p < 0.10$, * $p < 0.05$

Chapter 3

Price dynamics and quantile regressions*

Abstract

The Law of One Price is a still puzzling economic concept. Studies that found violations of the Law are frequent and numerous, although scholars have pointed that failures of the Law are likely to be due to lack of informative datasets. In addition, for storable commodities, the possible interactions of spatial and temporal arbitrage may hide the implications of the Law, invalidating the conclusions of the studies. Based on a simplified two-market model of spatio-temporal arbitrage, I review the implications of the Law of One Price and test for them with a rich dataset of weekly prices of storable commodities, and information on transaction costs, trade and storage. I conclude that most of the statements implied by the Law of One Price are indeed not empirically violated.

Keywords: LOP, Quantiles, Storage, Trade, Transaction Costs.

JEL: Q11, Q13, Q17, C32, P42

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Prices dynamics and quantile regressions

"Victory has a hundred fathers and defeat is an orphan"

(1961, J.F. Kennedy)

Introduction

Arbitrage has been defined as "the possibility to make a profit in a financial market without risk and without net investment of capital" (Delbaen and Schachermayer, 2006). Arbitrage implies that different prices for the same commodity will converge to a single value. It is important to distinguish spatial arbitrage from temporal arbitrage: the former is the core of the Law of One Price (Fackler and Goodwin, 2001); the latter is key in competitive storage models (Williams and Wright, 2005). The Law of One Price (LOP) is used to define markets as the spaces within which the price of a good tends toward uniformity, allowance being made for transportation costs (Stigler 1966 cited in Fackler and Goodwin 2001, p.974). The role of transaction costs has been puzzling, claimed to be responsible for the numerous violations attested for the Law (Goodwin, 1990; Fackler and Goodwin, 2001). LOP is likely to have been violated more than any other economic laws and violations continue to be detected (e.g. Lamont and Thaler, 2003; Crucini et al., 2010; Gopinath et al., 2011). Several empirical methods have been adopted to validate the LOP (e.g. Richardson, 1978; Ardeni, 1989; Goodwin and Piggot, 2001; Engel and Rogers, 2001; Taylor, 2001; Gopinath et al., 2011; Novy, 2013), but relatively little effort has been made to investigate the theoretical implications of the Law when either spatial and temporal arbitrage is possible.

A common weakness of a vast majority of empirical studies is that the datasets employed contained only one (namely the prices) of the economic variables necessary to define the equilibrium in model of spatial allocation. Excluding trade flows and transportation costs lead to weak results and make the econometric specifications become unable to tests for market integration (Goodwin et al., 1990). Barret (2001) asserted that the improved statistical methods of price analysis cannot compensate for the absence of essential information. Furthermore, even if data on prices, trade flows and transaction costs are considered, the model could make inference only on equilibrium conditions. In other terms, the exclusive observation of prices movements results in skipping the microeconomic foundations of equilibrium and do not allow understanding the cause of market disequilibria which could be derived from demand or supply shifts or both. Fackler and Goodwin (2001, p. 978) argue that, because the LOP relies on an equilibrium concept, the violation of the theory could be due to an unstable trading relationship or to a disequilibrium situation. It is therefore important to enlarge the set of information in tests for the LOP.

Another issue is quite underinvestigated. In tests of the LOP temporal arbitrage is usually ignored. However the two arbitrage forces (temporal and spatial) have different implications for price dynamics, and therefore are likely to alter the implications of the LOP.

Let's take a step back. Spatial arbitrage is due to the interaction of seller(s) and buyer(s). Trade from A to B occurs if the (expected) price in B is less than the price in A plus the transaction costs required to sell the product in market B. Temporal arbitrage occurs if the expected price at time $t+1$ is higher than the current price in t (net of interest rates and costs for storage). So, while spatial arbitrage equalizes prices in separate markets, temporal arbitrage acts on price dynamics over time. However, in both cases the arbitrage affects (either directly or indirectly) the price relationships of connected markets.

So simply, does the LOP hold across separated markets when storage and trade tend to occur? Does the LOP if trade or storage do not occur?

I review three implications of the LOP for several spatially separated markets: arbitrage pushes prices to differ by less than arbitrage costs; absence of arbitrage breaks serial correlation in price differences; the larger the profitable arbitrage opportunities are, the faster they tend to be eliminated. The dataset includes ten years of weekly data on prices and transaction costs, as well as data on trade flows and stock levels, for grain commodities. I propose relatively simple tests, which indeed require a rich and informative dataset, and conclude on the empirical validity of the LOP when trade, storage and transaction costs data are available. I also explore the local persistence of profitable arbitrage opportunities through a quantile autoregressive model. The approach adds a novel tool to the price analysis toolkit and it is shown to be a useful technique to investigate price dynamics and to conclude on the effects of arbitrage. I found that most of the implications of the LOP holds, and large arbitrage opportunites are quickly exploited.

Implications of spatial and temporal arbitrage

Testing for the validity of the LOP with price data only is not trivial. As pointed by Goodwin et al. (1990) and Barrett (2001), it would be wise to convey more information into the econometric models of price transmission. I propose to rely on information on prices, trade flows, stocks and transportation costs[1]. In order to capture the main price dynamics, the framework is admittedly simple in that I assume that goods are homogeneous, trade occurs in a single currency[2], and depreciation and perishability are negligible[3].

Let me make a step back. Let's remind ourselves what the Law of One Price (conditions (7) and (8) in appendix) implies: expected prices tend to be at the boundaries of the arbitrage costs band (with edge $T-k$) if trade and storage take place, and at the boundaries of the trade costs band (with edge T) if only trade occurs. These conditions define the "inactivity band" with and without storage[4]. We should observe prices difference within the "inactivity band" if trade is not profitable and storage is not zero, while price differences may or may not exceed the band if spatial and temporal arbitrage is not occurring. In fact, if no arbitrage is in place, prices are not linked by any relationships. Finally, spatial arbitrage should push prices to differ exactly by arbitrage costs.

Given these premises, I define different regimes based on the presence or absence of spatial and temporal arbitrage. A set of (weak) assumptions on expectations, price dynamics, markets and costs structure is required. Without loss of generalization, hereafter, I define i to be the export market, and j to be the import market.

[1] Conditions 1-8, in appendix, provide a solid theoretical ground to derive testable propositions

[2] While these assumptions may seem strong, they are very reasonable for selected products, prices and data frequency. We discuss more on this later in the paper.

[3] These assumptions are very common in studies on storage.

[4] In the literature on price transmission the "inactivity band" is defined by the maximum prices difference that trigger spatial arbitrage activity.

Assumption 1 Prices are martingale processes, therefore the unconditional expected future price is equal to the current price: $E_t[P^i_{t+1}] = P^i_t$. Rational arbitrageurs, based on their information sets (Ω), forecast prices at exporting and importing locations. The conditional expected price differs by realized price for a forecast error: $E_t[P^i_{t+1}|\Omega_t] = P^i_{t+1} + \nu_t$, with $\nu_t \sim \xi(0, \sigma^2)$ and $E[\nu_t, \nu_{t-1}] = 0$.

where ν_t represents the one-period ahead forecast error (cfr. Cumby and Obstfeld, 1981). More generally, if storage or trade contracts take place over multiple periods, say p, forecast errors serially correlated up to $p-1$ periods are coherent with the LOP (cfr. Cumby and Obstfeld, 1981), and therefore $E_t[P^i_{t+1}] = \sum_{i=1}^{l} \rho_{t-l} P_{t-l}$, with conditional expectations being modified accordingly.

Assumption 2 Trade occurs unidirectionally, from the exporting market to the importing market, with trade costs exceeding storage costs (k_t)[5], no capacity limitations in trade and storage, and constant storage costs $(T_t > k_t = \bar{k}, \forall t > 0)$.

where T_t and k_t represent, respectively, the spatial arbitrage costs, and the temporal arbitrage costs. The timing is important: I assume that trade takes less than one period to occur[6], therefore, when price differentials are larger than arbitrage costs, goods are traded and reach destination in less than one period. The above assumptions and the spatial and temporal arbitrage conditions allow us to define three different regimes: the first regime comprises both spatial

[5]If not, storage would never be profitable and the model would never have trade and storage both occurring at the same time (Coleman, 2009).

[6]I recognize that if trade takes more time, price dynamics may differ (cfr. Goodwin et al., 1990) and, in particular, we would observe lagged price responses to large price differentials. In the empirical setting, I use different specifications to allow for lagged adjustments up to four periods and found no relevant differences in results.

and temporal arbitrage; the second regime occurs if temporal arbitrage is in place, while spatial arbitrage is absent; the third regime is defined by the absence of arbitrage (either temporal and spatial).

Again, spatial and temporal arbitrage, *per se*, imply the following:

$$\textbf{Spatial arbitrage} : X_t > 0 \Rightarrow E_t[P^i_{t+1}] - E_t[P^j_{t+1}] = T_t$$

or $P^i_{t+1} + \nu^i_t - P^j_{t+1} - \nu^j_t = T_t$, that is also equivalent to $E_t[P^i_{t+1} - P^j_{t+1}] = T_t$, with T_t representing the trade cost to ship a good between i and j in period t.

$$\textbf{Temporal arbitrage} : S_t > 0 \Rightarrow E_t[P^i_{t+1}] - P^i_t = k_t$$

or $P^i_{t+1} + \nu^i_t - P^i_t = k_t$, that is also equivalent to $E_t[P^i_{t+1} - P^i_t] = k_t$. Again, k_t represents the storage cost to store the commodities for one period.

In addition, if trade and storage occur, prices are expected to differ by less than trade costs. The interaction of the two arbitrage forces may imply further results that I analyze empirically. I focus also on price dynamics and in particular on the existing serial correlation across prices that are linked by arbitrage forces.

Spatial arbitrage (i.e. trade) exists until profitable opportunities are fully exploited. Practically, trade is expected to make (realized) price differences uncorrelated over time[7]. In other words, according to the LOP and the arbitrage theory, trade should eliminate profitable arbitrage opportunities across physically separated markets and, as a consequence, when trade is occurring expected prices differ exactly by the cost of spatial arbitrage, with expected price differences uncorrelated over time.

On the other hand, temporal arbitrage is able to induce high price serial correlation (Cafiero et al., 2011). Theoretical and empirical evidence have shown that prices of storable goods are

[7]If not, there would be opportunities for arbitrage which would attract arbitrageurs up to restore the zero correlation condition.

expected to be serially correlated when storage is positive, and in particular Cafiero et al. (2011) conclude that the serial correlation induced by competitive storage is regime-dependent: serial correlation is explained by storage only for positive stock levels, and cannot be explained by the competitive storage model during stockouts. Analogously, I expect to observe regime-dependent level of serial correlation for price differences: non-zero correlation is coherent with the competitive storage model when storage is positive or when supply or demand are serially correlated (Thurman, 1988), and zero correlation is expected during stockouts[8]. Below I state the two conditions in analytical terms.

$$\textbf{Spatial arbitrage}: X_t > 0 \Rightarrow Corr(P_t^i - E_t[P_{t+1}^j], E_t[P_{t+1}^i] - E_t[P_{t+2}^j]) = 0$$

or $Corr(P_t^i - P_{t+1}^j, P_{t+1}^i - P_{t+2}^j) = 0$.

Temporal arbitrage:

$$S_t > 0 \Rightarrow Corr(P_t^i - E_t[P_{t+1}^j], E_t[P_{t+1}^i] - E_t[P_{t+2}^j]) \neq 0$$

or $Corr(P_t^i - P_{t+1}^j, P_{t+1}^i - P_{t+2}^j) \neq 0$.

Lastly, if arbitrage occurs, I expect arbitrage opportunities to be exploited. Therefore, the speed of prices difference reversion to the band has to be faster in the regime with trade than in the other regimes, and it should increase with the magnitude of arbitrage opportunities.

I define three price regimes based on observed level of trade activity from the export to the

[8] Hereafter I define the years with no storage as those in which the level of storage is lower than that observed in the previous year. The approach is necessary in that there are no years with zero stocks, which is also plausible considering that when stocks are rare convenience yield is high (Brennan, 1958). For instance, if in 2004 the storage level is 1000 tonnes, in 2005 is 1200 tonnes, and in 2006 is 1100 tonnes, 2005 is defined to have positive storage ($S_{2005} > 0$), while 2006 is defined as year with no storage ($S_{2006} = 0$). Such a specification ensures to have years with no storage,in that, for these commodities level of storage equal to zero are extremely rare. Precisely, as pointed by Thurman (1988), zero correlation is expected only if aggregate stockouts occurs, a condition that is rarely observed in global markets. In fact, only if inventories are zero marginal stock adjustments are not possible, and therefore spot prices are not linked.

import market and on storage arbitrage activities in the export market[9]. Such a definition of regimes rules out endogeneity issues that may arise when regimes are defined as function of price levels or price dynamics[10].

Regime I: Trade and storage

The presence of spatial and temporal arbitrage defines the first regime. First, spatial and temporal arbitrage conditions imply that, if trade and storage are occurring, expected price differences are equal to the expected transaction costs net of storage costs. Therefore we would have that expected price differences minus expected arbitrage costs (trade costs and storage costs) have to be (on average) zero. Put differently, in the first regime price differences that are within the "inactivity band" (defined by trade costs) are consistent with the LOP.

Second, spatial and temporal arbitrage are expected to influence price serial correlation. When spatial arbitrage takes place, price differences are expected to be uncorrelated over time. At the same time, if temporal arbitrage takes place (i.e. storage is positive), prices are likely to be correlated over time. Therefore, if price differences at time t and $t+1$ are correlated, when both trade and storage occur or not, is an empirical question which depends on the prevailing force (the high correlation induced by the temporal arbitrage or the zero correlation implied by the spatial arbitrage).

Analytically, I define regime one as follows:

$$\text{Regime I: } X_t^{ij} > 0,\ S_t^i > 0 \text{ and } X_t^{ji} = 0$$

[9]In theory, and in practice, trade reversal is either possible as well as observed in the data. However, my focus is on the normal coarse of arbitrage activities and therefore I rule out trade reversal by excluding from the analysis those periods in which trade reversals occur. Therefore, analytically, regimes one, two and three, are defined in such a way that trade reversal does not occur. Similarly, I do not consider storage activities in the import market. As argued by Coleman (2009), storage in the import market should never be profitable is trade exist. These stringent conditions allow to make the empirical analysis more tightly connected to the theoretical implications of the Law of One Price.

[10]In particular, Lence et al. (2016) show that when regimes are defined as function of price differentials the inference on arbitrage activities is poor.

I expect to find that $E_t[P^i_{t+1}] - E_t[P^j_{t+1}] = T_t - k_t$.Again, trade implies $E_t[P^i_{t+1}] - E_t[P^j_{t+1}] = T_t$, and storage in i implies $E_t[P^i_{t+1}] - P^i_t = k_t$, and substituting the second expression in the first expression the above result is obtained. Moreover, trade implies $Corr(P^i_t - E_t[P^j_{t+1}], E_t[P^i_{t+1}] - E_t[P^j_{t+2}]) \to 0$ and storage implies $Corr(P^i_t - E_t[P^j_{t+1}], E_t[P^i_{t+1}] - E_t[P^j_{t+2}]) \neq 0$, therefore if price differences are serially correlated remains an open question.

Regime II: Storage, no Trade

The presence of temporal arbitrage, and the absence of spatial arbitrage defines the second regime. Temporal arbitrage conditions state that, if storage is occurring, first order price differences are not greater than the expected storage costs. In addition, if trade is not occurring price differences are expected to differ by less than the trade arbitrage costs. Therefore, when storage is positive and trade is absent I expect to observe price differences to be less than the net arbitrage costs (spatial arbitrage costs minus temporal arbitrage costs). Put differently, in the second regime price differences that are within the "inactivity band" are consistent with the LOP.

In addition, the serial correlation induced by storage may be detected also in price differences. Put differently, in the second regime (where storage is in place) serial correlation in price differences is consistent with the LOP.

Analytically, I define regime two as follows:

$$\text{Regime II: } X^{ij}_t = 0,\ S^i_t > 0 \text{ and } X^{ji}_t = 0$$

and I expect to find that $E_t[P^i_t] - E_t[P^j_{t+1}] < T_t - k_t$. Again, no trade implies $E_t[P^i_{t+1}] - E_t[P^j_{t+1}] < T_t$, and storage in i implies $E_t[P^i_{t+1}] - E_t[P^j_t] = k_t$, and substituting the second expression in the first expression the above result is obtained. Moreover storage implies

$Corr(P_t^i - E_t[P_{t+1}^j], E_t[P_{t+1}^i] - E_t[P_{t+2}^j]) \neq 0$, therefore price differences may be serially correlated as well.

Regime III: No storage, no Trade

The absence of temporal and spatial arbitrage defines the third regime. When temporal and spatial arbitrage are not occurring, price differences may exceed or not trade costs net of arbitrage costs, due to unexpected demand rises and convenience yield (Brennan, 1958). In fact prices differences may spread less than full carrying charges due to the convenience yield (Kaldor, 1939), The convenience yield is low when stocks are abundant, but it is positive when stocks are rare. However exactly because of convenience yield, large differences in prices may be not exploited by arbitrageurs. In fact, the convenience of holding stocks is motivated by the convenience of reducing the costs and delay of furniture, or to answer to unexpected demand rises, and to insure the continuity of exploitation. Therefore the absence of arbitrage (either temporal and spatial) may be due either to the absence of profitable arbitrage opportunities (i.e. the price in the export market is not expected to rise, and the price in the import market differ from the price of the export market by less than trade costs), or to the physical absence of the product (i.e. the price in the export market is expected to rise but there is no product left for storage, or the price in the import market differ from the price of the export market by more than trade costs but no product can be traded). I distinguish the two cases. So when arbitrage is not profitable, price differences are expected to be less than trade costs minus storage costs and minus convenience yield. However, since it is likey during the years of low stocks there may be unexpected demand rises, we may observe price differences exceeding trade costs (simply because there is not enough product to be traded, or convenience yield has increased as well). Lastly, the absence of trade and storage does not allow one to conclude on serial correlation, therefore I have no expectation on the correlation of price differences.

Analytically, I define regime three as follows:

$$\text{Regime III: } X_t^{ij} = 0,\ S_t^i = 0 \text{ and } X_t^{ji} = 0$$

If product is available price differences are expected to be less than arbitrage costs and therefore we may have $E_t[P_t^i] - E_t[P_{t+1}^j] < T_t - k_t]$. If product is not available price differences may exceed or not trade costs and therefore I state that the price differences can be greater or smaller than arbitrage costs: $E_t[P_t^i] - E_t[P_{t+1}^j] \gtreqless T_t - k_t$. Note that absence of trade (when product is not available) is coherent with $E_t[P_{t+1}^i] - E_t[P_{t+1}^j] > T_t$, and absence of storage in i (when product is not available) is coherent with $E_t[P_{t+1}^i] - E_t[P_t^j] > k_t$, and substituting the second expression in the first expression the above result is derived.

Empirical strategy

In order to evaluate the implications of the arbitrage conditions, I construct the variable "Profitable Trade" which equals the logarithm of the ratio of price differences over freight rate costs:

$$(1)\ E[\pi_t^T] = ln(\frac{P_{t+1}^j - P_t^i}{T_t})$$

where P^j and P^i are, respectively, the (spot) price in the import location and in the export location. The above expression is valid when traders have perfect foresights, or (more realistically) when a large amount of trade based on prices contracted before shipping. In fact when $E[\pi_t^T]$ is greater than zero the expected profits from trade are positive in that the price differences are larger than the freight rate costs. Conversely, if the variable $E[\pi_t^T]$ is less than zero, the price differences are smaller than the freight rate costs and therefore trade is not profitable. The variable described above allows one to make a parallelism with the the literature on price transmission: $E[\pi_t^T] > 0$ indicates that price differences are in the "outer" regime (i.e. prices "deviate" from their long-run relationships), while $E[\pi_t^T] < 0$ indicates that price differences are in the "inside" regime[11]. The logarithimic transformation implies that positive and negative values are, respectively, indicators of profitable and non profitable spatial arbitrage[12]. In addition, the log-form allows to interpret regression coefficients as elasticities.

Assuming that arbitrage is instantaneous is a strong assumption. More realistically the arbitrage activity restores the equilibrium after one or more periods (Goodwin et al., 1990): the stickiness in spread of information and the time required for trade and storage operations are good examples to explain lagged prices adjustments[13]. I generalize the variable "Profitable Trade" by

[11]Analytically, $E[\pi_t^T] < 0$ if $P_{t+1}^j - P_t^i < T_t$ so that trade is not profitable; $E[\pi_t^T] > 0$ occurs when $P_{t+1}^j - P_t^i > T_t$ and therefore trade is profitable; $E[\pi_t^T] = 0$ occurs when $P_{t+1}^j - P_t^i = T_t$ and therefore it is indifferent to trade or not.

[12]Again, if $E[\pi_t^T] = 0$ we have $P_{t+1}^j - P_t^j = T_t$ and therefore arbitrageurs are indifferent between trading or not.

[13]For instance, a cargo ship sailing from a European port to a US one typically takes 10 to 12 days, depending on water currents and other factors.

allowing up to p periods for arbitrage adjustments to take place[14]. Hereafter, for instance, I will use the notation "Profitable Trade 4" to indicate that the adjustment is assumed to require four periods to occur[15]. The variable "Profitable Trade" will be used to evaluate how price differences are distributed with respect to the arbitrage costs, if and how they are serially correlated, and to conclude on if and how profitable arbitrage opportunities tend to be eliminated through arbitrage. Let me elaborate on the general strategy implemented in this paper.

Preliminarily, I evaluate, through non-parametric tests, if the prices differences have similar median values[16] and similar distributions across the three regimes. If spatial and temporal arbitrage act in a similar way, prices differences will have similar median values and similar distribution. Indeed, I expect to find that medians differ and that price differences are distributed differently under different arbitrage regimes. This preliminary step allows to establish if arbitrage alters level and distribution of price differences. So, first, I evaluate if price differences tend to be equal or smaller than arbitrage costs. I compare the median values through a semi-parametric regression: the median regression (Koenker, 2005)[17]. I expect to find, for regimes I and II, a price differences median value close to or below zero.

Second, I evaluate the serial correlation in price differences. I apply the test for autocorrelation proposed by Cumby and Huizinga (1992). I expect to find evidence favoring serial correlation in regime I and evidence favoring no serial correlation in regime III. In regime II, given that trade is absent, and storage is increasing, I cannot conclude on serial correlation.

Third, I evaluate whether arbitrage tends to eliminate profitable opportunities. The analysis is conducted through a quantile autoregressive model (Koenker and Xiao, 2006): when arbitrage is

[14] As a result, the variable will be as follows: $ln(\frac{P^j_{t+p} - P^i_t}{T_t})$.

[15] Analytically, "Profitable Trade 4" is $ln(\frac{P^j_{t+4} - P^i_t}{T_t})$.

[16] The use of median values is preferred to rule out the role of outliers due to large deviations from the equilibrium (e.g. occasional and excessive price spikes).

[17] Although rational expectations are usually linked to the mathematical expectation operator, again, by using the median value I avoid potential leverage effects of outliers, and I am able to establish if price difference tend to be less than arbitrage costs.

occurring profitable opportunities (proxied by positive values of the variable "Profitable trade") should be quickly exploited and therefore should die out; moreover, the larger the arbitrage opportunities (i.e. the larger the values of "Profitable trade"), the faster the elimination of such opportunities should be. All in all, the analysis allows me to characterize if and how price behavior is altered by temporal and spatial arbitrage, and how arbitrage eliminates (unexploited) profitable opportunities.

Formal non-parametric tests of equality of median values and equal distribution are applied. The Kruskal-Wallis test is a rank sum statistics allowing to test for equality of median values across different samples[18]. The Kolmogorov-Smirnov test is a non-parametric test of equality of probability distributions. The test quantifies the distance between the empirical cumulative distribution function (CDF) of two different samples, or it may be used to evaluate the distance with respect to a the CDF of a reference distribution, and therefore to detect a statistical significant difference.

The median regression, adopted to evaluate whether price differences tend to exceed arbitrage costs, is more robust to outliers than OLS[19]. The median regression, $Q_{E[\pi_t^T]}(0.5) = \theta_0(0.5)$, is solved through a minimization problem (Koenker, 2001):

$$(2)\ \hat{\alpha} = argmin_{\alpha \in \Re} \sum_{t=1}^{T} |E[\pi_t^T] - \alpha|$$

where $\hat{\alpha}$ is the estimated median value, and the $E[\pi_t^T]$ is the "Profitable trade" variable. Koenker (2005) argues that the median autoregression is a strongly consistent estimator for the median value. In my framework the median regression indicates if price differences tend to be larger (positive coefficient) or smaller (negative coefficient) than the arbitrage costs[20].

[18] The null hypothesis that median values are equal is tested against the alternative that median values are different.

[19] Note that in the median regression the constant is the median of the sample, while in the $.q$ quantile regression the constant is the q^{th} percentile for the sample.

[20] Technically, the median regression chooses τ, equal to 0.5 in this case, so that the mass of observations (i.e. the observation values wighted by their density) are distributed so that $\xi(x_i, \beta)$ is larger than 50 percent of the

In order to evaluate the dynamics of price differences, I adopt the test of serial correlation proposed by Cumby and Huizinga (1992). Under the null hypothesis the time series have a moving average, and the hypothesis is tested against the general alternative that autocorrelations of the time series are nonzero at lags greater than the specified one[21]. Serial correlation is likely to be detected when storage is positive and trade not absent (regime I), and should die out when temporal arbitrage is not occurring (regime III).

Finally I estimate a quantile autoregression model (Koenker and Xiao, 2006), able to highlight local dynamics (i.e. local stationary or local unit-root behavior), and therefore to underline the speed at which small or large deviations from the long-run equilibrium revert to the equilibrium. I estimate the model on the entire sample, as well as on the subsets of positive and negative values. The estimates on positive values of "Profitable trade" allows to conclude on how profitable arbitrage opportunities are exploited at different magnitudes. The estimates on negative values of "Profitable trade" allows to conclude on how price differences behave when arbitrage opportunities are non-profitable. The estimates on the entire sample allows to conclude on the global behavior of price series. The autoregressive model is as follows:

$$(3)\ Q_{E[\pi_t^T]}(\tau | Q_{E[\pi_t^T]_{t-1}}) = \theta_0(\tau) + \theta_1(\tau) E[\pi_t^T]_{t-1}$$

where τ is the quantile at which the model is evaluated, θ_0 and θ_1 are the estimated coefficients, with $\frac{1}{1-\theta_1}$ representing the "speed of reversion"[22], and $h = \frac{ln(0.5)}{ln(\hat{\theta_1})}$ representing the "half life" (the number of periods required to achieve a 50 % adjustment toward the equilibrium). I consider

observations. Intuitively, the median regression indicates the observation (named "median") at which all other observations are equally distributed below and above the median. The procedure can be applied to any other quantile.

[21] Assuming that the time series is a moving average of order q, the test is general enough to test the hypothesis that the time series has no serial correlation (i.e. q equals 0) or the null hypothesis that serial correlation in the time series exists, but dies out at a known finite lag (i.e. q is greater than zero). It is worth recalling that the autocorrelation function of an MA(q) process becomes zero at lag q+1 and greater, therefore the test is a test of no serial correlation after a specified lag.

[22] I borrowed (and adapted) the terminology from the literature on price transmission, where $\frac{1}{1-\theta_1}$ represents the "speed of trasmission"

three values of τ: 0.25, 0.5 and 0.75. Due to the limited number of observations per regime, I estimate the quantile autoregression specification using a system of three equations[23]. In addition, I compute the interquantiles coefficient ("[.25-.75]") and test its statistical significance. Intuitively, larger profitable arbitrage opportunities should be exploited faster than smaller ones, and therefore the higher the quantile, the faster the reversion of price differentials to the equality should be. The quantile autoregression model should reveal lower estimated coefficients (θ_1) at higher quantiles and therefore I expect quantile coefficients to follow a concave function. Put differently, I expect to find $\theta_1(0.75) < \theta_1(0.5) < \theta_1(0.25)$ so that the larger the profit opportunities, the faster their elimination (via arbitrage) should be (Figure 1).

The specification I adopt shares analogies with the threshold cointegration model proposed by Balke and Fomby (1997). Let me elaborate more on the intuition by presenting the analogy that my approach shares with the threshold cointegration model of price transmission. The positive values of the variable "Profitabe trade" ($E[\pi_t^T]^+$) are those allocated in the outside regime of the threshold cointegration model[24] in that the positive values imply that the differences in prices exceed the transaction costs. Conversely, the negative values of the variable "Profitabe trade" ($E[\pi_t^T]^-$) corresponds to the observations allocated in the inside regime of the threshold cointegration model (Figure 2). Given this analogy, the coefficients of the threshold quantile autoregression model (θ_1) are inversely related to the "speed of reversion": if $\theta_1 \to 0$ mean reversion is immediate, and therefore the arbitrage opportunities are exploited immediately; if $\theta_1 \to 1$, the local persistence is strong and therefore the arbitrage opportunities tend to last longer; if $\theta_1 > 1$ the time series show a locally explosive tendency which would imply that the arbitrage opportunities tend to be increased by further opportunities. Again, the coefficients can be easily interpreted as by considering that $\frac{1}{1-\theta_1}$ is the "speed of reversion", and $h = \frac{ln(0.5)}{ln(\hat{\theta_1})}$

[23] The approach allows one to compute standard errors more efficiently

[24] The equivalent threshold cointegration model would be as follow: $(P_{t+1}^j - P_t^i) = I\{\alpha + \rho(P_t^j - P_{t-1}^i) + \epsilon_t\}$ with I, the indicator function, being exogenously determined by transaction costs (T_t).

is the "half life".

Data and Results

Data

Prices have been extracted from the International Grain Council which provides export prices for several grain commodities. Export prices are free on board (fob) and quoted in US $ per tonnes [25]. The IGC has 42 commodities in total: I consider only prices of homogeneous goods collected at different locations. As a result, I have collected data on wheat, barley, rice and soybean[26]. Data span a ten years period, from 2004 to 2014, and are available at weekly frequency[27]. I collect freight rates for pairs of locations. The IGC dataset in total contains 118 routes overall (61,360 prices for the past 10 years). For the main grains and oilseeds, there are around 500 estimates of end of year stocks. I have collected weekly data for most routes in order to extract information on transportation costs. In particular, freight rates for location pairs have been collected at weekly frequency from April 2005 onwards. Rates are for heavy eight grains, priced in US $ per tonnes[28]. The dataset also includes data on annual stock levels and annual and monthly trade flows, collected, respectively, from USDA and UNCOMTRADE database.

[25] More precisely, export prices are nearest position, indicative only and do not constitute actual traded/market price (IGC, 2014)

[26] However data on soybean are not shown in that not all variables were available and therefore results were not directly comparable with those presented in this paper.

[27] For few series data are also available at daily frequency.

[28] The dataset contains no missing or few missing data for three price series (0.8% for Australian price of barley, 2.5% for German price of barley, and 5.2% for Vietnamese price of rice) which have been opportunely treated through interpolation.

Arbitrage matters

Preliminary analysis shows that the three regimes are not always occurring for the six markets pairs (table 1). In addition, regimes contain a limited number of observations in that I exclude years in which trade is observed in both directions. As a result, in four out of six cases I identify only two out of three regimes.

Some characteristics of the time series deserve further note: the share of price differences exceeding the freight costs is generally larger in regime one than in regimes two and three; the maximum number of consecutive deviations (i.e. the number of consecutive periods in which price differences have exceeded freight rate costs) is larger in regime one with respect to regimes two and three. All in all this implies that deviations are more likely to be reported when trade is occurring.

I found that the medians and the distributions of prices differences are different across regimes (tables 2 and 3). In particular the null hypotheses of equal medians (Kruskal-Wallis test) and of equal distribution (Kolmogorov-Smirnov test) are rejected more often when regimes one and two are compared to regime three, implying that prices differences in presence of arbitrage and in absence of arbitrage are distributed differently. The results are more evident when I allow for a longer adjustment period (i.e. "Profitable trade 4" is considered). As a result, I conclude that the type of arbitrage (spatial or temporal) does matter: it alters proportional differences and their distributions.

Prices differ by less than arbitrage costs, if...

The median regression analysis (table 4) shows that in most of the cases the LOP is not violated: when spatial or temporal arbitrage is occurring prices tend to differ by less than arbitrage costs, as implied by the LOP. Interestingly, for rice, price differences tend to be much larger than arbitrage costs. A deeper investigation reveals that when direct trade is conspicuous in both directions[29] price differences are less likely to differ by less than arbitrage cost. Intuitively, this suggests that, in these cases, goods movements through trade "causes" deviations from the equilibrium, rather than helping to restore the equilibrium conditions. Conversely, when direct trade is mainly unilateral (as for barley and wheat pairs), the implications of the LOP are well satisfied in that prices differences are smaller than the arbitrage costs. A warning is therefore that the first "test" for the LOP (the median regression) need to be interpreted jointly with data on direct trade.

Arbitrage influences serial correlation

The law of one price asserts that prices should not be correlated if spatial arbitrage is occurring (and if long term contracts are exceptions, and not the rule). The tests of serial correlation confirm the implications of the LOP. In particular the null hypothesis of no serial correlation at four lags of the test by Cumby and Huizinga (1992) cannot be rejected in regime I for most market pairs. The implications of the LOP are more evident when I allow for a longer adjustment period in that in regime I the presence of temporal arbitrage induces serial correlation. In Regime II, I should reject the null hypothesis in that the absence of trade, and the presence of storage suggest that prices difference may be serially correlated. Indeed, I find that price differences are serially correlated only for few periods (i.e. I reject the null of no serial correlation at one lag, but fail to reject at four lags). This is not surprising in that storage is linking prices of the same market over time, but it does not link prices of spatially separated markets. Therefore, again, price differences are serially correlated only in the very short run. Similarly, serial correlation for price differences dies out after few periods when spatial arbitrage and temporal arbitrage are absent in that prices are linked by no arbitrage forces. The results are not very dissimilar across markets, so I conclude that the differences in trade flows pointed out earlier do not play

[29] This is the case, for wheat markets, for France-USA pair, and, for rice, for the Pakistan-Vietnam and the Pakistan-Thailand pairs.

the same role for serial correlation.

In order to better understand the results it is worth pointing that, in all cases, when the analysis is not broken down by regimes of arbitrage, the null hypothesis of no serial correlation is rejected very often. Put differently, I found that arbitrage modifies the serial correlation of price differences series.

I conclude that price differences tend to not be serially correlated when spatial arbitrage is occurring, or, in other words, spatial arbitrage tends to eliminate serial correlation. The presence of storage influences the results and induces serial correlation in price differences for very few periods. Also in this case, by feeding the analysis with trade and storage data, as well as with data on transaction costs, I am able to support the statements implied by the LOP: (direct) trade tends to eliminate serial correlation in prices difference.

Arbitrage opportunities are exploited

Lastly I comment on the results of the quantile autoregression model. Again, the regression is restricted to positive values of the variable "Deviation" in order to assess if profit opportunities (i.e. excessive price differences with respect to arbitrage costs) tend to be eliminated through arbitrage. A statistically significant autoregressive coefficient (less than one in absolute value) would suggest that arbitrage opportunities are gradually eliminated: the smaller the coefficient (in absolute terms) the faster the elimination of arbitrage opportunities will be (figures 1 and 2).

First, I found that arbitrage opportunities are exploited regardless of the occurrence of arbitrage. However, the analysis shows that spatial arbitrage facilitates the elimination of profitable arbitrage opportunities while temporal arbitrage mkes it less likely to occur. In few cases the estimated coefficients exceed one, indicating that arbitrage opportunities are not exploited (and indeed favor further profitable arbitrage opportunities). However, these exceptions are related to lower quantiles (.25), that is the exceptions are only related to cases in which the profits from arbitrage are likely to be small. In addition, the local unit roots (i.e. the local explosive behavior) is more evident in the three cases in which trade is bilateral[30]. Intuitively, when trade is massive in both directions it is less likely for traders to take advantage of small arbitrage opportunities in that it is relatively more difficult to forecast incoming and outgoing trade flows, than forecasting only outgoing trade flows[31].

As far the speed at which profitable opportunities are exploited, I found that the coefficients tend to decrease monotonically from the lower (0.25) to the higher (0.75) quantile: larger deviations are eliminated faster than small ones. Intuitively a large deviation means that arbitrage opportunities are large, and this is likely to attract a large number of arbitrageurs: as a results, if profit opportunities are large, they are likely to be exploited soon. While these results are evident in table 8, there is an even stronger evidence when I compute half lives (table 9)[32]. The differences in estimates across quantiles (.25-.75) are always statistically significant (see columns 5 of table 8), that is price dynamics are different at different levels of price spreads. The negative sign for the interquantiles estimate suggests that large arbitrage opportunities tend to be exploited faster than smaller ones in that the coefficients estimated at lower quantiles (.25) is larger than the coefficients estimated at higher quantiles (.75). By disaggregating the estimates for the inside and the outside regimes, we found similar evidence: when statistically significant, the interquantiles estimate is negative, so the coefficients estimated at lower quantiles (.25) is larger than the coefficients estimated at higher quantiles (.75).

Again, by feeding the analysis with data on transaction costs, I am able to support the statements implied by the LOP: trade eliminates arbitrage opportunities.

[30] Again, this is the case, for wheat markets, for France-USA pair, and, for rice, for the Pakistan-Thailand pair.

[31] We do not comment possible implications on price dynamics due to intra-industry trade and counter-season trade. The analysis does not take these issues into acocunt: exploring them is left for future research.

[32] The resutls are comparable with those attested in the literature or price transmission: Santeramo (2015) found half-lives raging from 1 to 5 weeks in vegetable markets; Stephens et al. (2012) found half lives of on month in tomato markets.

Concluding remarks

The empirical validity of the Law of One Price has been doubted and challenged numerous times. The lack of informative datasets is one of the main issues. A second important issue is that the validity of the LOP has been usually investigated ignoring the potential implications of different arbitrage regimes induced by the presence (or absence) of direct trade (or) storage. In order to revise the validity of the statements of the LOP, I review the implications of the Law, and use a rich dataset which includes weekly data on prices and transaction costs, as well as data on trade flows and stock levels. I use non-parametric tests and quantile regressions to highlight price dynamics when arbitrage is occurring, and conclude on the validity of the Law. As pointed by Goodwin et al. (1990), the inclusion of data on transaction costs results in a lower tendency to detect violations of the LOP. I found similar evidence.

More precisely, by using set of variables an informative of trade flows, level of stocks and transaction costs I conclude that most of the statements of the Law of One Price are indeed confirmed. First, I found price differences tend to be smaller than the arbitrage costs, when arbitrage is occurring. Second, the serial correlation in price differences, observed throughout the entire sample, is less evident when spatial arbitrage is occurring and dies out in few weeks. Third, arbitrage tends to eliminate unexploited profit opportunities, and larger profit opportunities are quicker exploited than smaller opportunities, especially when spatial arbitrage is occurring.

All in all, the results stand in favor of the empirical validity of the Law. The approach I use, in particular the quantile autoregression model, is novel in the literature on the Law of One Price: it represents a promising tool to further investigate price dynamics and, in particular, to deepen on local persistency (or unit root behavior) that may arise during stockouts or excess of exports. More important, the quantile autoregression allows one to investigate price dynamics in abnormal situations (e.g. when price differences are very low, or very high). To the extent that arbitrage is triggered by the excessive price differences, proposing a tool to empirically investigate price dynamics in abnormal situations is important.

Last but not leat, to the extent that price dynamics influence the functioning of the markets, and have important repercussion on welfare, understanding price dynamics under different arbitrage regimes represent not only a promising and fruitful area of research, but also an important step to plan price stabilization policies.

Methodological Appendix

On spatio-temporal arbitrage conditions

I describe arbitrage behavior when trade and storage are feasible options. Traders and stores are assumed to be price-takers, and to hold rational expectations based on the available information. Profit-seeking agents will exploit arbitrage opportunities arising from spatial or temporal disequilibria.

I describe the behavior of forward looking agents at time $t+1$. Spatial arbitrage conditions imply that, in expectation, prices of homogeneous goods in two separated markets may differ at most by the transaction costs necessary to reallocate the goods from the relatively good-abundant market to the good-scarce market. Timing is very important. I assume that trade takes time: specifically if traders commit in period t to ship a good to the other market, the good will be marketed in the destination market at time $t+1$ (cfr. previous studies such as Goodwin et al., 1990; Coleman, 2009). The arbitrage conditions can be stated as follows:

(1) $E_t[P^i_{t+1}|\Omega_t] - E_t[P^j_{t+1}|\Omega_t] < E_t[T^{ij}_t|\Omega_t]$ for $X^{ij}_t = 0$

(2) $E_t[P^i_{t+1}|\Omega_t] - E_t[P^j_{t+1}|\Omega_t] = E_t[T^{ij}_t|\Omega_t]$ for $X^{ij}_t > 0$

where $E[\cdot]$ is the expectation operator, P^i_t and P^j_t are the prices in market i and j, $E_t[T^{ij}_t]$ are expected unit cost to ship from i to j at time t, X^{ij}_t is the quantity traded from i to j, Ω_t is the information set[33]. Transaction costs (at time t) are known by informed agents. Without loss of generality I assume that $P^i_t < P^j_t$, and that $T^{ij}_t = T^{ji}_t = T_t$, thus $E_t[T^{ij}_t|\Omega_t] = T_t$. Therefore agents will face no uncertainty on <u>expected</u> prices at the final location, although the expected prices will differ from the <u>realized</u> price for a "forecast" error term ($\epsilon^P_t \overset{iid}{\sim} (0, \sigma^2)$), assumed to be iid with zero mean[34]. Notationally this means that $E_t[P^i_{t+1}|\Omega_t] = P_{t+1} + \epsilon^P_{t+1}$, that is price

[33] The model assumes that information is available regardless the location of traders. Allow for different information sets is feasible, and left as future advance.

[34] The model can easily incorporate uncertainty in transaction costs.

expectations differ by realized price for a zero mean error term. In a more compact notation, we may rewrite the trade arbitrage conditions for the two markets as follows:

(3) $(E_t[P^i_{t+1}|\Omega_t] - E_t[P^j_{t+1}|\Omega_t] - T_t) \cdot X_t = 0$

where X_t represents the traded quantity. Since there is no reason to transfer goods among the two locations if there is not a price gap, conditions (1) and (2) suggest that trade will not occur if prices differ by less than transaction costs. This implies that prices will tend to move toward the boundaries of the (expected) "transaction costs band" if trade is occurring, while they will have no relationships if trade is not occurring.

Temporal arbitrage conditions implies that the expected (and discounted) future price will differ from current price at most for the costs of storage:

(4) $\frac{(1-\delta)}{(1+r)} E_t[P^i_{t+1}|\Omega_t] - E_t[P^i_t|\Omega_t] < E_t[k_t|\Omega_t]$ for $S_t = 0$

(5) $\frac{(1-\delta)}{(1+r)} E_t[P^i_{t+1}|\Omega_t] - E_t[P^i_t|\Omega_t] = E_t[k_t|\Omega_t]$ for $S_t > 0$

where k_t represents the cost to store goods for one period, δ is the depreciation rate, r is the interest rate, S_t is the quantity stored. The net interest rate will be $r - \delta$. I assume that k_t is the same for locations i and j and it is constant over time ($k_t = k \ \forall t > 0$). Noting that $E_t[P^i_t|\Omega_t] = P^i_t$, and $E_t[k_t|\Omega_t] = k_t = k$, the expressions 4 and 5 greatly simplify. In sum, I only assume that storage costs and expected future prices are known, while realized future prices are uncertain[35].

Since there is no reason to store goods if prices are not expected to rise faster than the net interest rate and by more than the storage costs, conditions (4) and (5) suggest that storage will not occur if prices at time $t + 1$ and time t are expected to differ by less than storage costs[36]. To have a clearer picture, I rewrite the above conditions[37] as follows:

[35] For simplicity we set r and δ equal zero. The results are not sensitive to this assumption, which is in line with literature on storage (cfr. Wright and Williams, 1984). Moreover, we assume there will not be convenience yield. Indeed, if convenience yield is taken into account, we should expect price differences to spread less than storage costs when storage is zero. Exploring this issue is left for future research.

[36] Recall that $r = \delta = 0$.

[37] For simplicity we only write them for market i.

(6) $(E_t[P^i_{t+1}|\Omega_t] - P^i_t - k) \cdot S_t = 0$

This implies that (virtually) prices at different timing (t and $t+1$) will move within the boundaries of the "storage costs band" if storage is zero, and (in expectation) the first-order difference ($E[\Delta P_{t+1}] \equiv E_t[P_{t+1} - P_t]$) will equal k if storage takes place.

Different from spatial arbitrage that allows transfer of goods in both directions (from market i to market j and vice-versa), temporal arbitrage allows to transfer goods only in one direction (from period t to period $t+1$). In both cases arbitrageurs are profit-seeking agents, so spatial and temporal arbitrage will be substitutes strategies[38].

Assuming that transaction costs are constant over time ($T_t = T \ \forall \ t > 0$), spatial arbitrage implies the following:

(7) $|E_t[P^i_{t+1}|\Omega_t] - E_t[P^j_{t+1}|\Omega_t]| \leq T$ for $X_t > 0$

However, as long as $T > k$, that is spatial arbitrage is more costly than temporal arbitrage, it will be not profitable to store the imported good. Therefore, condition (8) is also valid[39]:

(8) $|E_t[P^i_{t+1}|\Omega_t] - E_t[P^j_{t+1}|\Omega_t]| \leq T - k$ for $X_t > 0$ and $S_t > 0$

Based on these arbitrage conditions I derive propositions implied by the LOP and evaluate the validity of the LOP under different trade and storage regimes.

[38] In particular, as shown by Miranda and Glauber (1995), trade is at least partial substitute for storage, while the opposite is not true. Thus trade reduces storage, while the opposite is not necessarily true.

[39] This result is also shown by Coleman (2009)

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Table 3.1: Number of deviations per regime

Markets pair	Regime I		Regime II		Regime III	
	$X > 0, S > 0$		$X = 0, S > 0$		$X = 0, S = 0$	
Wheat: FRA - USA	52		52		na	
	[44.2%]	[22]	[46.1%]	[21]		
Wheat: GER - USA	na		138		na	
			[14.4%]	[17]		
Barley: FRA - AUS	52		152		307	
	[46.2%]	[7]	[13.3%]	[9]	[51.0%]	[15]
Barley: GER - AUS	na		203		305	
			[5.2%]	[4]	[9.6%]	[11]
Rice: PAK - VIE	52		103		46	
	[86.5%]	[29]	[49.5%]	[16]	[44.2%]	[18]
Rice: PAK - THA	104		na		12	
	[80.7%]	[76]			[19.2%]	[5]

The first line of the table reports the number of observations per each regime, while in squared parentheses are reported the percentage of price differences exceeding freight rate costs, and the maximum number of consecutive deviations (i.e. price differences exceeding freight rate costs

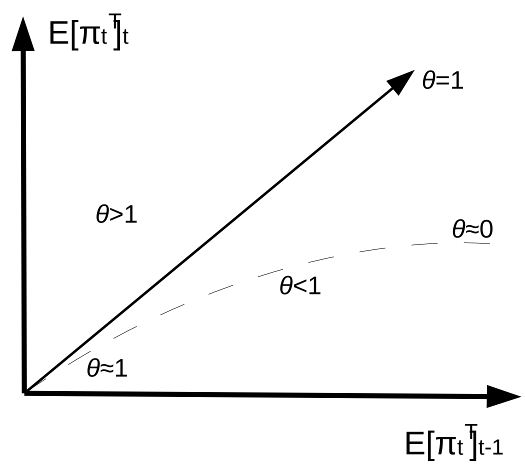

Figure 3.1: QAR on $E[\pi_t^T] = ln(\frac{P_{t+1}^j - P_t^i}{T_t})$

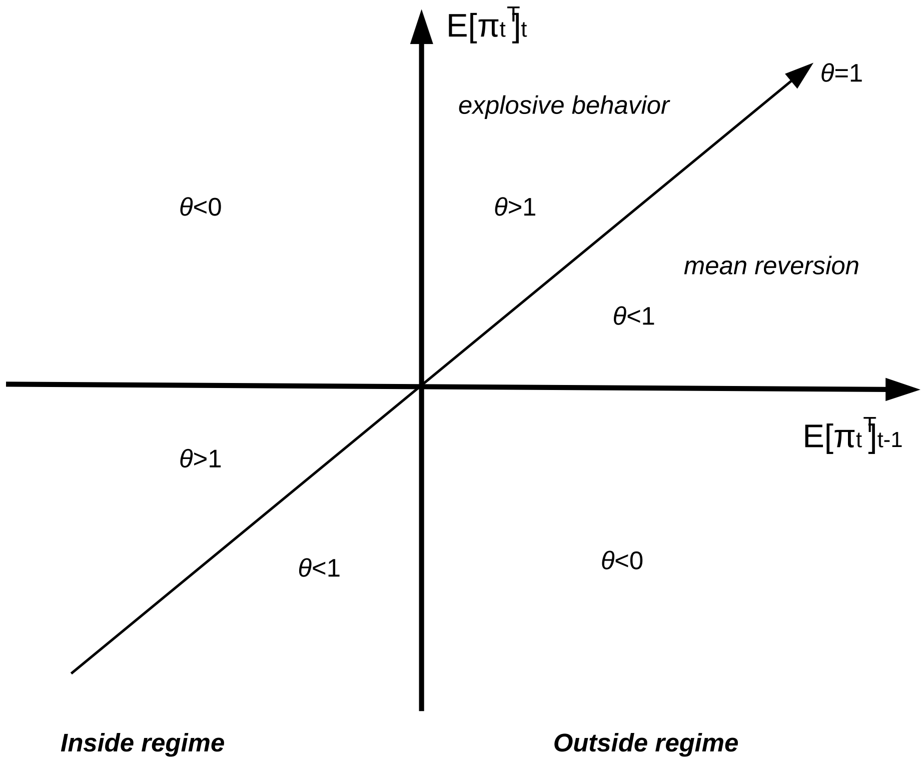

Figure 3.2: Threshold QAR on $E[\pi_t^T] = ln(\frac{P_{t+1}^j - P_t^i}{T_t})$

Table 3.2: Kruskal-Wallis equality-of-populations rank test

Markets pair	Adjustment	All Sample	I vs II	I vs III	II vs III
Wheat: FRA - USA	1 week	.468	.339		
	4 weeks	.425	.779		
Wheat: GER - USA	1 week				
	4 weeks				
Barley: FRA - AUS	1 week	.001	.159	.557	.000
	4 weeks	.001	.002	.547	.001
Barley: GER - AUS	1 week	.001			.001
	4 weeks	.001			.001
Rice: PAK - VIE	1 week	.001	.115	.011	.058
	4 weeks	.001	.007	.135	.001
Rice: PAK - THA	1 week	.000		.000	
	4 weeks	.001		.000	
N. of rejections	1 week	4 out of 5	0 out of 3	1 out of 3	2 out of 3
N. of rejections	4 weeks	4 out of 5	2 out of 3	1 out of 3	3 out of 3
Overall share of rejections	1/4 weeks	66.6%	33.3%	33.3%	62.5%

The number of rejections refers to a 10% significance level.

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Table 3.3: Two-sample Kolmogorov-Smirnov test for equality of distribution functions

Markets pair	Adjustment	I vs II	I vs III	II vs III
Wheat: FRA - USA	1 week	.417		
	4 weeks	.349		
Wheat: GER - USA	1 week			
	4 weeks			
Barley: FRA - AUS	1 week	.103	.458	.000
	4 weeks	.004	.607	.000
Barley: GER - AUS	1 week			.001
	4 weeks			.001
Rice: PAK - VIE	1 week	.011	.001	.021
	4 weeks	.000	.252	.000
Rice: PAK - THA	1 week		.000	na
	4 weeks		.000	
N. of rejections	1 week	0 out of 3	2 out of 3	2 out of 3
N. of rejections	4 weeks	2 out of 3	1 out of 3	3 out of 3
Overall share of rejections	1/4 weeks	33.3 %	50 %	62.5 %

The number of rejections refers to a 10% significance level.

Table 3.4: Median regression analysis

Markets pair	Regime I		Regime II		Regime III	
Wheat: FRA - USA	-.095	.268^{+}	.039	.041		
	[.115]	[.146]	[.136]	[.154]		
Wheat: GER - USA			-.816**	-.693**		
			[.089]	[.096]		
Barley: FRA - AUS	-.930**	-.666**	-1.022**	-.847**	-.544**	-.588**
	[.179]	[.196]	[.085]	[.098]	[.084]	[.076]
Barley: GER - AUS			-1.204**	-1.065**	-.780**	-.663**
			[.085]	[.075]	[.071]	[.069]
Rice: PAK - VIE	.405**	.163**	.120	.606**	.064	.001
	[.075]	[.177]	[.152]	[.126]	[.246]	[.250]
Rice: PAK - THA	1.813**	1.875**			.124*	.178*
	[.156]	[.125]			[.052]	[.077]

Standard errors in brackets. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$
The first and second column for each regime report, respectively, results for Deviations 1 and Deviations 4.

Table 3.5: Serial correlation tests - Profitable trade 1

Markets pair	Adjustment	All Sample	Regime I	Regime II	Regime III
Wheat: FRA - USA	1 week	.000	.014	.019	
	4 weeks	.000	.432	.727	
Wheat: GER - USA	1 week	.000		.606	
	4 weeks	.228		.737	
Barley: FRA - AUS	1 week	.001	.073	.194	.075
	4 weeks	.102	.539	.615	.217
Barley: GER - AUS	1 week	.000		.016	.134
	4 weeks	.429		.960	.324
Rice: PAK - VIE	1 week	.000	.001	.001	.002
	4 weeks	.023	.333	.626	.249
Rice: PAK - THA	1 week	.000	.000		.047
	4 weeks	.000	.005		.767
N. of rejections	1 week	6 out of 6	2 out of 4	1 out of 5	1 out of 4
N. of rejections	4 weeks	2 out of 6	1 out of 4	0 out of 5	0 out of 4
Overall share of rejections	1/4 weeks	66 %	37.5 %	10 %	10 %

The reported values are the p-values of the Cumby and Huizinga (1992) test. The number of rejections refers to a 10% significance level.

Table 3.6: Serial correlation tests - Profitable trade 4

Markets pair	Adjustment	All Sample	Regime I	Regime II	Regime III
Wheat: FRA - USA	1 week	.000	.011	.162	
	4 weeks	.002	.078	.699	
Wheat: GER - USA	1 week	.000		.558	
	4 weeks	.353		.891	
Barley: FRA - AUS	1 week	.000	.093	.387	.000
	4 weeks	.426	.573	.630	.447
Barley: GER - AUS	1 week	.000		.081	.005
	4 weeks	.481		.528	.492
Rice: PAK - VIE	1 week	.000	.001	.000	.002
	4 weeks	.000	.151	.080	.298
Rice: PAK - THA	1 week	.000	.000		.026
	4 weeks	.000	.108		.356
N. of rejections	1 week	6 out of 6	2 out of 4	1 out of 5	3 out of 4
N. of rejections	4 weeks	3 out of 6	0 out of 4	0 out of 5	0 out of 4
Overall share of rejections	1/4 weeks	75 %	25 %	10 %	30 %

The reported values are the p-values of the Cumby and Huizinga (1992) test. The number of rejections refers to a 10% significance level.

Table 3.7: Median autoregression (θ_1 estimates by regimes)

	Whole Sample	$E[\pi_t^T] < 0$	$E[\pi_t^T] > 0$
Wheat: FRA - USA			
θ_1	0.82**	0.24	0.66**
	(0.03)	(0.05)	(0.03)
$\theta_1 * R^I$	-0.19+	-0.01	-0.59**
	(0.10)	(0.13)	(0.11)
$\theta_1 * R^{II}$	0.07	-0.13	-0.04
	(0.15)	(0.23)	(0.11)
$\theta_1 * R^{III}$	na	na	na
Observations	516	277	230
Wheat: GER - USA			
θ_1	0.79**	0.55**	0.49**
	(0.03)	(0.05)	(0.03)
$\theta_1 * R^I$	na	na	na
$\theta_1 * R^{II}$	0.13	0.01	-0.31
	(0.11)	(0.13)	(0.19)
$\theta_1 * R^{III}$	0.03	0.07	na
	(0.06)	(0.07)	
Observations	508	386	117
Barley: FRA - AUS			
θ_1	0.69**	0.57**	0.16**
	(0.04)	(0.04)	(0.06)
$\theta_1 * R^I$	0.04	0.04	0.17
	(0.07)	(0.07)	(0.17)
$\theta_1 * R^{II}$	0.12*	0.09+	-0.04
	(0.05)	(0.05)	(0.27)
$\theta_1 * R^{III}$	na	na	na
Observations	489	408	69
Barley: GER - AUS			
θ_1	0.67**	0.61**	0.16
	(0.03)	(0.04)	(0.14)
$\theta_1 * R^I$	na	na	na
$\theta_1 * R^{II}$	0.13**	0.15**	-0.06
	(0.04)	(0.04)	(0.19)
$\theta_1 * R^{III}$	na	na	na
Observations	493	445	44
Rice: PAK - VIE			
θ_1	0.85**	0.55**	0.65**
	(0.03)	(0.05)	(0.06)
$\theta_1 * R^I$	-0.13	-0.51	-0.15
	(0.10)	(0.31)	(0.10)
$\theta_1 * R^{II}$	0.02	-0.19+	0.03
	(0.05)	(0.10)	(0.07)
$\theta_1 * R^{III}$	0.12	0.07	0.01
	(0.07)	(0.11)	(0.13)
Observations	444	204	233
Rice: PAK - THA			
θ_1	0.97**	0.75**	0.92**
	(0.01)	(0.07)	(0.01)
$\theta_1 * R^I$	0.01	-0.01	0.03
	(0.02)	(0.12)	(0.02)
$\theta_1 * R^{II}$	na	na	na
$\theta_1 * R^{III}$	-0.03	1.12	-0.23
	(0.46)	(4.52)	(0.33)
Observations	387	78	305

(100 bootstrapped) s.e in brackets. $^+$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$
R^I, R^{II} and R^{III} are dummies equal to one for regimes I, II and III and zero otherwise

Table 3.8: Quantile autoregression (θ_1 estimates and interquantiles estimates)

	Quantiles			IQ [.25-.75]	IQ [.25-.75]	IQ [.25-.75]
Markets pair	0.25	0.50	0.75	Whole Sample	$E[\pi_t^T] < 0$	$E[\pi_t^T] > 0$
Wheat: FRA - USA	1.05**	0.79**	0.63**	-0.43**	-0.12	-0.14**
	(0.06)	(0.04)	(0.04)	(0.05)	(0.16)	(0.07)
Wheat: GER - USA	0.95**	0.80**	0.66**	-0.29**	-0.35**	0.06
	(0.04)	(0.04)	(0.04)	(0.05)	(0.08)	(0.09)
Barley: FRA - AUS	0.90**	0.74**	0.57**	-0.33**	-0.38**	-0.05
	(0.04)	(0.05)	(0.03)	(0.04)	(0.07)	(0.07)
Barley: GER - AUS	0.89**	0.75**	0.62**	-0.26**	-0.30**	-0.12
	(0.04)	(0.04)	(0.04)	(0.04)	(0.06)	(0.02)
Rice: PAK - VIE	0.93**	0.86**	0.73**	-0.19**	-0.20**	0.07
	(0.05)	(0.04)	(0.05)	(0.05)	(0.09)	(0.01)
Rice: PAK - THA	1.02**	0.98**	0.92**	-0.11**	0.02	-0.10**
	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)

(100 bootstrapped) s.e in brackets. $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Table 3.9: Half lives by quantiles (expressed in weeks)

	0.25	0.50	0.75
Wheat: FRA - USA	∞	3.1	1.4
Wheat: GER - USA	13.5	3.1	1.6
Barley: FRA - AUS	6.6	2.3	1.2
Barley: GER - AUS	5.9	2.4	1.4
Rice: PAK - VIE	6.5	4.3	2.2
Rice: PAK - THA	∞	22.7	8.3

Chapter 4

Distance in Gravity Models: Does it Resist the Resistance Terms?*

Abstract

The gravity equation has been widely adopted to analyze international flows and to estimate trade costs. Despite its wide use, a major concern relates the heterogeneous evidence on the impact of geographical distance on trade flows. A plethora of resistance terms and estimators have been adopted to shed light on this puzzle, but the simplistic solution to feed the model with a large number of variables as we can is not optimal in small datasets, or in the presence of numerous candidates as resistance terms. I evaluate the performance of the Poisson Pseudo Maximum Likelihood estimator when the number of regressors is large, and the parameter for distance is latent. I propose to use shrinkage estimators to reduce the number of resistance terms, and conclude on the potential bias induced by failing to do so.

Keywords: Trade, PPML, Shrinkage, LASSO, RIDGE, Transaction Costs.

JEL: C23, C51, F13, F14

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Distance in Gravity Models

"Ad minora me demittere no recusabo" (Quintilian)

(I will not refuse to descend to the most minute details)

Introduction

The gravity equation has been widely adopted to analyze international flows. Despite its wide use, major concerns remain, and in particular the effect of physical distance on trade flows is debated (e.g. Chaney, 2008; Disdier and Head, 2008; Novy, 2013).

Five decades of theoretical research and empirical estimation have not clarified the role of transaction costs. Disdier and Head (2008) summarized the empirical findings and concluded that the estimates of transaction costs are quite heterogenous: the theories for transaction costs in gravity equations are numerous (Head and Ries, 2001; Anderson and Van Wincoop, 2003; Chaney, 2008), and many variables have been considered to capture the role of transaction costs (or frictions) in international trade (Yamarik and Ghosh, 2005; Guiso et al., 2009; Head and Mayer, 2014).

Recently, Head and Mayer (2013) have proposed several explanations for resistance terms in trade. Their analysis is based on the evidence provided by Disdier and Head (2008) and describes numerous issues: sensitivity of estimates to the estimation methods, the puzzling role of borders, competing theories on trade frictions (e.g. imperfect competition, social and capital network, and localized tastes). Particularly challenging is the role of distance. Head and Mayer (2013) decompose the distance effect into a trade cost effect and a geographical distance effect. The former influences directly the volume of trade, the latter has an indirect effect through costs.

However, many other variables may enter in this mechanism: it needs further investigation. In fact, decades of research have been able to tell us not much more than "distance impedes trade": a negative sign on the coefficient of distance in a gravity equation is expected. However, the magnitude of the coefficient (which the theory predicts to be one) is heterogeneous across studies and depends on the set of variables taken (and not taken) into account. This confounds empirical settings, and leads researchers to introduce several proxies on the basis of somehow "ad hoc" (or at least not well supported) theoretical grounds.

The issue is very relevant. Understanding the distance effect is a necessary step to determine the role of trade barriers (e.g. tariffs and technical barriers). Selecting "trade resistance terms," and understanding which factors we need to control for, would help either to model trade flows, or to assess the economic impacts of trade policies, global changes, etc.

Two recent papers have analyzed the distance effect: Disdier and Head (2008) and Head and Mayer (2013). Disdier and Head (2008) review the estimates on geographical distance by means of a meta analysis. This approach relies on evidence from the existing literature, and may suffer from the same biases in estimation and specification that may undermine existing studies. Therefore, the study tells us what has driven results and provide an average estimate of the effect of distance on trade. On the other hand, Head and Mayer (2013) review the possible explanations for trade resistances and provide some empirical evidence, leaving open two puzzles: understanding the exact role of distance and borders. In order to solve these puzzles further empirical investigation is needed. A naive approach that has been adopted to isolate the role of distance is to feed the model with (available) control factors: the danger of omitting variables or including (highly) correlated variables is around the corner. In fact, many (probably too many) variables are possible candidates to account for trade resistance and the set of variables that should be adopted is not clear cut.

The goal of the present analysis is to understand how well one of the most commonly adopted estimator for the gravity models (the Pseudo Poisson Maximum Likelihood, PPML), compared to a set of penalized regression estimators, is able to capture the role of geographical distance when numerous control factors are included, or relevant variables are omitted[1]. To this extent, I run a simulation study: starting from real data on GDP and distance I generate data on trade, and infer the role of distance. In particular, I compare the performance of the PPML to three penalized regression estimators (LASSO, ELASTICNET and RIDGE) and one two-step estimator (named Post-LASSO PPML) capable of optimally selecting resistance terms. The penalized regression estimators are able to shrink the set of control factors by selecting the regressors. The estimators I have adopted work well even when the number of variables (k) is very large (and eventually larger than the number of observations (n)). The natural application of these estimators is in small samples and short panels, although I prove that they may be of help even in large samples to select control factors.

My contribution to the current literature is twofold. First, I review the validity of the theoretical assumption on the effect of geographical distance in gravity models by controlling for numerous resistance terms and conclude on the economic relevance of physical distance in trade. Second, and more important, by comparing the PPML, three shrinkage estimators and a two-step

[1] The focus on PPML is motivated by the large number of studies that adopt such estimator. However we are aware that, compared to other estimators, the PPML does not performs the best. For instance, Gmez-Herrera (2013) shows that PPML exhibits a much stronger bias than a fixed efects panel model, and is also less precide than the Heckman and the truncated models. Hence, although PPML shows a smaller variance, the prediction is poor, especially for low trade values, which are overestimated. Indeed, Gmez-Herrera (2013) shows that the Heckman sample selection model performs better overall among the most commonly adopted estimators for the gravity equation.

estimator for gravity models I on conclude on the best performing estimator for the distance when a large number of control factors are included. Lastly, I show the bias that may be potentially induced by an incorrect selection of control factors.

Theoretical framework

According to the seminal paper of Tinbergen (1962), trade flows can be described as functions of economic masses and trade costs (or bilateral accessibility). Proxies for economic masses are the countries' GDPs, while trade costs are usually proxied by geographical distance (Head and Mayer, 2013):

$$(1)\ X_{ij} = k \cdot Y_i^{\beta} \cdot Y_j^{\xi} \cdot \Phi_{ij}^{\delta}$$

where X_{ij} is the trade flow from i to j, Y_i and Y_j are, respectively, the GDP of the exporter and of the importer, while Φ_{ij} is the bilateral accessibility term, collecting the frictions to trade, and k is a constant. Hereafter I will refer to this specification as the Basic Gravity Model (BGM). Starting from Harrigan (1996), the trend in the literature has been to replace GDPs with shifters for importers and exporters (Anderson and van Wincoop, 2003; Feenstra, 2004; Baier and Bergstrand, 2007; Subramanian and Wei, 2007). The shifters (i.e. country fixed effects) control for multilateral resistance terms (cfr. Baldwin and Taglioni, 2001; Head and Mayer, 2013)[2]:

$$(2)\ X_{ij} = k \cdot S_i^{\beta} \cdot M_j^{\xi} \cdot \Phi_{ij}^{\delta}$$

[2] Multilateral resistance terms are able to eliminate problems such as the incorrect deflation of trade (Baldwin and Taglioni, 2006).

where S_i (M_j), according to Head and Meyer (2014), proxies the ability of the exporter i (importer j) to supply to (import from) all destinations[3]. From an empirical point of view S_i and M_j will be, respectively, dummies for the exporters and he importers. Hereafter, following the usual nomenclature in the literature of gravity models, I will refer to this specification as the Structural Gravity Model (SGM).

While trade costs are commonly approximated solely by geographical distance, this assumption is quite strong: indeed, it is very common to augment the gravity equation with several resistance terms (R) capturing the complexity of trade costs. For instance, colonial links, or common languages tend to facilitate trade. Similarly, Regional Trade Agreements (RTAs) are expected to influence bilateral trade. Following the specification of the BGM, the bilateral accessibility is assumed to be a function of geographical distance and trade resistance terms (i.e. trade agreements and other dummies) (cfr. Head and Mayer, 2014):

$$(3)\Phi_{ij} = D_{ij}^{\delta_1} \cdot R_{ijt}^{\delta_2} \cdot RTA_{ij}^{\delta_3}$$

where, again, the bilateral accessibility (Φ_{ij}) depends on geographical distance (D_{ij}), trade resistance terms (R) and trade agreements (RTA); δ_1, δ_2, δ_3 replace the parameter δ; D_{ij} represents the pair-wise geographical distance between i and j; R_{ij} and RTA_{ij} stand, respectively, for the socio-cultural resistance dummies (e.g. common languages, colonial links, etc.) and political-economic resistance terms (e.g. Regional Trade Agreements). According to the theory, a negative and statistically significant effect is expected for the bilateral accessibility term, and, in particular, for the geographical distance (δ_1). Hereafter I will refer to the specification

[3]Put differently, each country has a peculiar capacity to trade internationally, depending on several factors such as production, domestic consumption, openess to trade, etc. The structural gravity recognizes the failure of capturing both supply and demand of trade via GDP, and thus relies on fixed effects.

with expanded terms for bilateral accessibility as the Augmented Gravity Model (AGM), and distinguish between the the Augmented Basic Gravity Model (ABGM) and the Augmented Structural Gravity Model (ASGM).

Econometric framework

After log-linearization, the augmented structural gravity model may be rewritten as follows[4]:

$$(4)\ X_{ijt} = \alpha + \sum_{k=1}^{I} \beta_k S_i + \sum_{l=1}^{J} \xi_l M_j + \delta_1 lnD_{ij} + \sum_{r}^{R} \delta_r R_{ijt} + \sum_{rta}^{RTA} \delta_{rta} RTA_{ijt} + ln\epsilon_{ij}$$

where S_i and M_j are dummy variables equal to one if the trade flow concerns the exporter i (importer j) and zero otherwise, and thus represent exporters and importers fixed effect; D_{ij} is the geographical distance; R_{ijt} and RTA_{ijt} are the resistance terms and the bilateral trade agreements; R is the total number of dummies (or variables) added to control for colonial links, common languages, etc; RTA is the total number of bilateral regional trade agreements, coded as dummy variables; the additive error, $ln\epsilon_{ij}$, is assumed to be identically and independently normally distributed[5].

From an empirical point of view, several econometric issues need to be considered for a correct estimation of the gravity equation. The presence of zero flows and the heteroskedasticity in the error term affect gravity-type estimations (Silva and Tenreyro, 2006)[6]. For instance, least squares estimates tend to be biased by the presence of zero trade flows: the larger the percentage

[4]It is common to control for time-varying effects by including year fixed effects.

[5]Santos and Silva (2006) specify that ϵ_{ij} follows a log-normal distribution, with $E[\epsilon_{ij}|Y_i, Y_j, D_{ij}] = 1$, and $\sigma_{ij} = f(Y_i, Y_j, D_{ij})$. As a result, $ln\ \epsilon_{ij}$ follows a normal distribution, with $E[\epsilon_{ij}|Y_i, Y_j, D_{ij}] = 1/2ln(1 + \sigma_{ij})$. Since $E[\epsilon_{ij}|Y_i, Y_j, D_{ij}]$ depends on regressors, the OLS estimates are not consistent. They propose to use the Pseudo-Poisson Maximum Likelihood estimator.

[6]Since Trefler (1993), it is recognized that enodegeneity of trade agreements are a further issue. The problem is ignored in the present paper and results are derived mainly from a simulation design that rule out endogeneity issues that remain an interesting topic worth further investigation.

of zeros, the larger the bias in OLS is likely to be. Two naive approaches consist of replacing zero trade flows with small numbers or dropping observations with zero flows. The former neglects the intrinsic information conveyed by zero trade flows; the latter discards an even larger proportion of information contained in the dataset. A further solution consists in estimating a Tobit or a Poisson model. The Heckman (1979) specification can also handle sample selection induced by zero flows. However, the preferred specification in the vast majority of studies, due to its capability of dealing with zero flows and heteroskedasticity, is the approach proposed by Silva and Tenreyro (2006)[7]. The Poisson Pseudo-Maximum Likelihood (PPML) estimator maximizes the log-likelihood function:

$$(5)\ \mathcal{L}(\beta|Z,X) = \sum_{i=1}^{N}(x_i\beta' z_i - e^{\beta' z_i} - ln(x_i!))$$

where x and z are, respectively, the dependent variabe and the independent variables. The last term indicates the factorial of the dependent variable. The estimates ($\hat{\beta}$) are obtained by solving a set of non linear first order conditions ($\sum[x_i - e^{z_i\hat{\beta}}]z_i = 0$) where x_i represents the trade flows, z_i stands for the explanatory variables and $e^{z_i\hat{\beta}}$ is the mean of trade flows, conditional on covariates (i.e. $E[x_i|z_i] = e^{z_i\hat{\beta}}$). Wooldridge (2010, p. 676) argues that the PPML estimates of β are consistent if the conditional mean is correctly specified, that is if $E[x_i|z_i] = e^{z_i\hat{\beta}}$ holds[8]. The point I raise is that when negligible trade resistance terms are included, the model is incorrectly specified (overfit); on the other hand, by ignoring trade resistance terms that matter the model

[7] In particular, Silva and Tenreyro (2006) argue that the log linear specification of the gravity equation induces heteroskedasticity, violates the assumption of statistically independency among regressors and error term, and thus leads to inconsistent estimates. The PPML assumes that the conditional mean of the dependent variable is proportional to its conditional variance ($E[x_i|z_i] = e^{z_i\hat{\beta}} \propto Var(x_i|z_i)$), and therefore the heteroskedasticity is taken into account.Furthermore, PPML estimates are robust to the inclusion of zero trade flows. Both results are proved in their paper.

[8] The property applies regardless of the count data adopted. In particular, Santos and Silva (2006, p 645) state that "data do not have to be Poisson at all and [...] x_i does not even have to be an integer for the estimator based on the Poisson likelihood function to be consistent. This is the well-known PML result first noted by Gourieroux, Monfort, and, Trognon (1984)".

is also incorrectly specified (underfit). A possible solution to avoid the omission of relevant variables may be the "ALL-IN" one: estimate the model by including the largest number of (available) trade resistance terms[9]. Put simply, how well does the PPML perform in this case? I analyze the properties of the PPML estimator compared to poisson shrinkage estimators, a set of estimators capable of "selecting" the regressors that matter, thus improving efficiency of estimates (Gruber, 1998). I estimate a generalized linear model via penalized maximum likelihood (Tibshirani et al., 2012):

$$(6)\ Min_{(\beta)}\ \{-\tfrac{1}{N}\mathcal{L}(\beta|Z,X) + \lambda((1-\alpha)\textstyle\sum_{p=1}^{P}\tfrac{1}{2}||\beta_p||_2^2 + \alpha\sum_{p=1}^{P}||\beta_p||_1)\}$$

$$\text{with } \mathcal{L}(\beta|Z,X) = \textstyle\sum_{i=1}^{N}(x_i\beta' z_i - e^{\beta' z_i} - ln(x_i!))$$

where $\mathcal{L}(\beta|Z,X)$ is the log-likelihood, N is the number of observations, P is the number of variables of the model that will be "selected"[10], Z represents the set of covariates, and X is the dependent variable (i.e. trade flows). The notation "$||\cdot||_l$" indicates the l-norm, with $||\cdot||_l \equiv (\sum_{i=i}^{n}|x_i|^l)^{1/l}$. In the above equation, the L1 norm is the absolute value, and the L2 norm is the squared root of the sum of squared absolute values. The parameter $\lambda \geq 0$ is a complexity parameter, responsible for global shrinkage. While the parameter may be fixed, it is usually optimally selected by the algorithm in order to maximize the penalized fit. Lastly, the parameter α allows for linear combinations of L1 and L2 norms. I specify three values for α in order to estimate a LASSO model ($\alpha = 1$), a RIDGE model ($\alpha = 0$) and the ELASTIC NET

[9]Similar approach has been adopted in several paper in agricultural economics to study the effects of non tariff barriers (Grant and Lambert, 2008; Grant and Boys, 2011; Dal Bianco et al., 2016.

[10]The set of variables that are subject to penalization is included in the set of regressors in that I will always keep the physical distance as regressor.

$(0 < \alpha < 1)$[11]. According to the literature on shrinkage estimators, the number of excluded variables is maximum when α equals one, no regressor is excluded when α equals zero, and the not-excluded variables that are highly correlated receive a low weight. Therefore, the RIDGE penalty shrinks the coefficients of correlated regressors towards each other (the higher the correlation across variables, the lower the weights they receive after the shrinkage), while the LASSO tends to pick one of them and discard the others.

In order to evaluate the empirical relevance of using a shrinkage estimator to select control factors, I suggest a POST-LASSO PPML estimator (PLPPML). The PLPPML is a simple two-step estimator: the first step consists in estimating a (Poisson) LASSO model on the entire set of variables (z)[12], and the second step in estimating the PPML on the set of variables selected in the first step. Analytically, I use a LASSO estimator in the first stage:

$$\text{(7a) } Min_{(\beta)} \; - \tfrac{1}{N}\mathcal{L}(\beta|Z, X) + \lambda(\textstyle\sum_{p=1}^{P} ||\beta_p||_1)$$

and then regress trade flows against the set of variables for which $\beta_{LASSO} \neq 0$ in the first stage, using the Poisson Pseudo-Maximum Likelihood (PPML) estimator, and maximizing the log-likelihood function:

$$\text{(7b) } \mathcal{L}(\beta|Z', X) = \textstyle\sum_{i=1}^{N}(x_i\beta' z_i' - e^{\beta' z_i'} - ln(x_i!))$$

where Z' indicates the subgroup of variables (Z) that have been selected in the first stage. The rationale for using the PLPPML is to estimate the reduce the number of independent variabes when estimating the PPML by eliminating the control factors that are highly correlated.

[11] The value adopted for the ELASTIC NET is 0.5. Furthermore, it has been checked if the results are sensitive to the adoption of $\alpha = 1 - \epsilon$ or $\alpha = \epsilon$, with ϵ relatively small. Results are not affected, so I conclude that the selection of variables is regular and those not heavily depend on the value of ϵ.

[12] For the gravity model I set distance and GDP (in the basic specification) to be always in the model (i.e. in the "active set").

Datasets, Simulation Design, and Estimation

In order to compare the performances of different estimators, I use a large dataset on bilateral trade collected from the UN ESCAP website. The dataset has been used to generate, by using information on distance and GDP, data on the bilateral accessibility term and on trade flows. The simulation approach is similar to that followed by Head and Mayer (2014). In addition, I use the original dataset of Silva and Tenreyro (2006) to compare the estimates provided by five estimators, and comment on their differences. The dataset from UN ESCAP consists of 52,521 observations, collected from 1994 to 2011: it is an unbalanced panel and includes 65 importers and 63 exporters. The dataset include 7 resistance dummies to control for contiguous borders, common languages and colonial links. From the WTO website I have extracted information on existing regional trade agreements (RTAs) for the countries of the dataset: I have coded 104 RTAs[13]. The rich dataset represent an interesting framework to test if and how the estimates of geographical distance change across different specifications of the gravity model. Descriptive statistics of distance, trade flows, and GDPs are presented in table 1.

The general approach of the present essay is divided in few steps. First, I run a simulation to evaluate how the estimators perform with different sample sizes: this is aimed at replicating the analysts' problem of dealing with few Countries, or short panels. Second, I run several simulations to understand how different assumptions on the bilateral accessibility term may chage the results. Lastly, I revise the estimates of Silva and Tenreyro (2006), comparing the estimates obtained through the PPML with those of shrinkage estimators.

Starting from the (real) data on distance, I generate (simulated) data on bilateral accessibility

[13] A number of RTAs have not been included simply because they are related to countries that are not in the dataset provided by UN ESCAP.

following the same specification adopted in Head and Mayer (2014), which represents a benchmark for my study. The data on bilateral accessibility are simulated by using the (real) data on distance. Bilateral accessibility is a function of geographical distance[14]:

$$(8)\ \widetilde{\Phi_{ij}} = e^{-lnD_{ij}} \cdot \eta_{ij}$$

where $\widetilde{\Phi_{ij}}$ stands for the generated values of bilateral accessibility, D_{ij} is the real distance between the pairs of countries, and η_{ij} is a log-normally distributed error term. I generate trade flows by feeding the basic gravity model with real data on GDP and the (simulated) data on bilateral accessibility ($\widetilde{\Phi_{ij}}$). In particular, following the basic gravity model (equation 1), trade flows (X_{ij}) are as follows:

$$(9)\ \widetilde{X_{ij}} = k \cdot Y_i \cdot Y_j \cdot \widetilde{\Phi_{ij}}$$

where $k = 1$, $\beta = \xi = 1$ to have unit income elasticity (Silva and Tenreyro, 2006), Y_i and Y_j are the real data on GDPs. As in Head and Mayer (2013), the only stochastic term is η_{ij} which follows a log-normal distribution[15]. The true parameter for geographical distance is therefore known and equal to one: the expected estimate of δ_1 is one.

In addition, I generated Φ_{ij} by assuming that other resistance terms (namely the regional trade agreements) matter[16]:

$$(10)\ \widetilde{\Phi_{ij}} = e^{-lnD_{ij}+.5*RTA_{ij}} \cdot \eta_{ij}$$

[14]I use a convenient specification that, while it is admittedly simple, it is coherent with the theoretical model.

[15]The parameter is set to have mean 0.1 and standard deviation 5. By assuming constant variability I rule out potential confounding effects due to the heteroskedasticity. In addition I rule out the problem of zero trade flows to have a clean simulation design.

[16]I use the same specifiction adopted in Head and Mayer (2014), and calibrate the model so to have the coefficient 0.5 for the other resistance terms.

where the coefficient .5 for the RTAs is in line with Head and Mayer (2014). Two specifications are adopted: first I assume that only the presence of at least one RTA (and not the total number of RTAs stipulated between two countries) is relevant for bilateral accessibility (10a); second, I assume that the total number of RTAs stipulated between two countries is relevant for bilateral accessibility (10b). The parameter "bilateral accessibility" is therefore generated as follows:

$$(10a)\widetilde{\Phi_{ij}} = e^{-lnD_{ij}+.5AnyRTA_{ij}} \cdot \eta_{ij}$$

$$(10b)\widetilde{\Phi_{ij}} = e^{-lnD_{ij}+.5\#RTA_{ij}} \cdot \eta_{ij}$$

The first specification assumes that RTA_{ij} is equal to one if there is at least one trade agreement stipulated between the two countries, and zero otherwise; in the second specification, RTA_{ij} is equal to the number of bilateral trade agreements in which the two countries are involved. These two specifications are intended to test how the estimates of δ_1 change when the trade resistance terms are under- or oversupplied. To test for the sensitivity of the estimates to irrelevant variables, I introduce variables that are correlated with distance but have zero mean. Such a noise should not change the estimates, and is intended to replicate modelers' problem of feeding the model with several "resistance terms" to control for as many frictions in trade as they can. In particular, I generate variables that are correlated with distance and have a noise term normally distributed with mean zero and standard deviation of one[17]. The approach is to understand if and how multicollinearity influences the estimates of the bilateral accessibility term.

[17]For instance a random variable (rv) with low correlation is generated as follows: $rv = 0.1 * Distance + noise$, with $noise \sim N(0, 1)$.

Results

The estimates for the economic masses are provided in table 2. Although I do not have access to the data adopted by Head and Mayer (2014), the simulation approach is the same, so comparison of results is feasible. For sake of comparison, I also consider the results provided by Silva and Tenreyro, who have proposed the PPML estimator. As expected, the coefficients for the economic masses are positive and statistically significant. The estimates are close to .8 and not affected by the inclusion of the trade resistance terms. Differently, the estimates on bilateral accessibility vary across specifications, from .49 to .87. However, the estimates are in line with literature: Disdier and Head (2008) found that the average "distance effect" is .90, but PPML estimates tend to be lower (-.35). In addition, the basic gravity model and the structural gravity models provide estimates, respectively, of -0.72 (compared to -0.75 of Silva and Tenreyro, 2006) and -0.87 (compared to -0.78 of Silva and Tenreyro, 2006). Notably, the structural gravity models tend to have higher estimates for bilateral accessibility[18], while the inclusion of resistance terms tend to lower the estimates: the more control factors (i.e. resistance terms) are included, the lower the estimates[19].

A first simulation has been employed to compare the estimates when "noise" is added (table 3). I estimate the basic gravity model with the addition of zero-mean terms, correlated to the geographical distance. The table shows the estimates for bilateral accessibility. By comparing the estimates in columns 2 through 5 it is evident that while on average the estimates are unbiased, the shrinkage estimators provide more stable estimates (i.e. the standard deviation is sistematically lower): the standard deviation of the PPML in column 5 is ten times bigger

[18] This is also in line with Disdier and Head (2008).

[19] Also this finding is in line with literature. Disdier and Head (2008) show that control factors alter estimates for distance.

than the standard deviation of RIDGE and ELASTIC NET.

The results in columns 6 through 11 allow one to conclude on how the estimators perform with small samples, and when the number of regressors is very large (columns 9 to 11). Overall, shrinkage estimators (LASSO, ELASTICNET and RIDGE) provide stable estimates. The estimates are always downward biased, with a bias not exceeding 16% (columns 10 and 11). The PPML produces downward biased estimates in all but the case in which the number of regressors (k) is larger than the observations (n). In this case (columns 10 and 11) the PPML performs very poorly: the bias is, on average, 47%. As expected, in all cases the bias and the efficiency of the estimates (i.e. the difference between the average estimates and the true value, and the standard deviation of estimates) respectively increases and decreases with the ratio k/n. Put differently, the bias and the standard deviation of estimates are largest in columns 10 and 11, when the number of regressors equates (or exceeds) the number of observations. However, it is worth stressing once more that marked differences exist among estimators: the average bias, and the standard deviation of estimates for LASSO are, respectively, three and twenty times smaller than the bias and standard deviation for PPML. The Post Lasso PPML is of limited interest in normal situations (columns 7 to 9), while it performs the best when the ratio k/n is one or above (that is when the number of regressors is equal or abve the number of observations): on average, the bias is only 5% (compared to 13%, 14% and 16% for shrinkage estimators and to 47% for PPML) and the standard deviation, compared to LASSO, RIDGE and ELASTIC NET is slightly larger.

Tables 4, 5 and 6 synthesize the performance of the estimators under different scenarios for the bilateral accessibility parameter. The baseline scenario confirms that the PPML provides the least stable estimates (the MSE is higher for PPML). The estimates are, on average, unbiased, or biased no more than 2% (*cfr.* PLPPML). So, if trade flows were only limited by geographical distance, the adoption of shrinkage estimators would result in unbiasedness, with a gain in precision of the estimates[20]. Table 5 presents an interesting comparison: the top boxes (which present results on basic and structural gravity models) represent the case in which (important) variables have been omitted; the bottom boxes present the case of inclusion of superfluous variables. As expected, the estimates are biased when variables are omitted, and, again, the PPML is the estimator that provides the lowest precision. Table 6 shows the most interesting (and realistic) case: bilateral accessibility is determined by several factors. When variables are omitted (top boxes) the bias is, on average, 40%, regardless of the estimator adopted, but the PPML produces the least precise estimates. When the model is fed with numerous variables (either the RTAs and the other resistace terms, R), the PLPPML is able to produce the least biased and most precise estimates. Last but not least, in all cases, the structural models show lower values of MSE.

Finally, I revise (table7) the estimates on bilateral accessibility provided by Silva and Tenreyro (2006). The PLPPML's estimates are -0.80 and -0.74 compared to -0.75 and -0.78 of the PPML. Based on previous simulations, we know that the structural gravity model is able to produce more precise estimates, and that the PLPPML is likely to provide the least biased and least dispersed estimates. Therefore, the real effect of geographical distance is likely to be -0.74 (rather than -0.78): the difference is not negligible. Moving from -0.78 to -0.74 implies to forecast (for my dataset) a difference in trade equals to 6000 million of US dollars (equivalent to the GDP

[20]Notably, RIDGE is able to provide unbiased and very stable estimates: however, one drawback of RIDGE is that it does not set to zero the "superfluous" regressors, while it shrinks the estimates for them to small values, close to zero. As a result RIDGE is not useful to select control variables.

of Guinea or Moldova).

Conclusion

The gravity equation is widely adopted to analyze international flows and to estimate trade costs. Despite its wide use, several puzzles are still open, and a major concern relates the impact of geographical distance on trade flows. Disdier and Head (2008) pointed that the estimates provided by the literature are heterogeneous. Part of this heterogeneity depends on the diversity of specifications that are adopted in gravity estimations, where somewhat "ad hoc" resistance terms are included to control for unobserved heterogeneity. As a result, we are far from reaching a consensus on how the model should be augmented to control for resistance terms. A simple solution would be to feed the model with as many variables as we can. In OLS overfitting is likely to affect efficiency but not unbiasedness. However, one of the most widely adopted estimator is not OLS. Indeed, the presence of zero flows and heteroskedasticity has suggested to adopt PPML in gravity estimations: a widely used estimator which probably owe its fortune to the simplicity of the algorithm of estimation provided by Silva and Tenreyro (2006). So how well can the PPML handle the inclusion of many (or too many) resistance terms to control for unobserved factors that influence trade costs? And how are the estimates of the effect of distance affected?

I estimate basic and structural gravity equations to answer the above research questions. A rich panel of fifty thousands observations, that span for more than a decade, is adopted to reach conclusions on the robustness of the estimates on geographical distance. The analysis is enriched by a Monte Carlo analysis aimed at highlighting how PPML performs in extreme cases (i.e.

when the number of control factors exceeds the number of observations) and under different specifications of the bilateral accessibility term. The PPML is compared to a set of shrinkage estimators (namely LASSO, RIDGE, ELASTICNET) and I suggest a novel approach to select the control factors: the use of PPML after having selected the control factors, via LASSO. The approach is generalizable to any estimator fo the gravity model (e.g. Heckman, Tobit, etc.).

I found that PPML works well under normal circumstances (i.e. when the number of variables is not very large compared to the number of observations), but it fails to pick up the right parameter for distance when the sample size is very small, or the number of control factors is large. The estimates provided are also more dispersed than those obtained through the shrinkage estimators. Both the omission of variables and the inclusion of irrelevant regressors tend to bias the estimates obtained through the shrinkage estimators, but the impact is relatively more severe for the PPML estimator. Lastly, I prove that by selecting control factors via LASSO and then estimating the model via PPML in the second stage, the estimates are less biased and more precise.

To the extent that understanding the effective relevance of impediments to trade is an important issue, with direct implications for policymakers, traders, analysts, and theorists, improving our toolkit to asses the impact of geographical distance on trade remains an important goal. The present paper suggests a different approach to estimate trade costs, that consists in selecting the control factors through a shrinkage estimator before estimating the coefficients of the gravity model. Hence, the paper shows how to reconcile the strategy of controlling for numerous factors with the approach of selecting the relevant variables to estimate a parsimonious model.

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Tables

Table 4.1: Descriptive statistics

	$Export_{ij}$	$\mathbf{ln}(GPD_i)$	$\mathbf{ln}(GPD_j)$	$\mathbf{ln}(Distance_{ij})$
Mean	17.51	25.92	25.14	8.55
Median	18.08	26.16	25.68	8.81
Min	0	17.86	17.84	4.08
Max	26.53	30.33	30.33	9.88
St. dev.	3.89	2.08	2.58	0.92
Skewness	-0.61	-0.77	-0.55	-1.05
Kurtosis	3.07	3.77	2.67	3.88
Observations	52521	52410	52422	52521

The distance is expressed in kilometers

Table 4.2: PPML Estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	BGM	SGM		ASGM		ABGM	
β_i	0.738***					0.719***	0.758***
	(99.11)					(250.41)	(260.81)
β_j	0.797***					0.776***	0.812***
	(76.44)					(274.17)	(283.24)
δ_1	-0.718***	-0.875***	-0.874***	-0.728***	-0.491***	-0.610***	-0.561***
	(-83.46)	(-208.38)	(-264.84)	(-133.96)	(-63.62)	(-152.89)	(-87.71)
α	-14.04***	33.879***	33.160***	32.245***	30.045***	-13.959***	-16.58***
	(-36.28)	(789.40)	(863.10)	(582.83)	(397.23)	(-119.66)	(-119.355)
FE	NO	YES	YES	YES	YES	NO	NO
YE	NO	NO	YES	NO	NO	NO	NO
Rdummies	NO	NO	NO	YES	YES	YES	YES
RTAdummies	NO	NO	NO	NO	YES	NO	YES
Sample size	52521	52521	52521	52521	52521	52521	52521

t statistics in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

BGM: Basic Gravity Model; SGM: Structural GM; ASGM: Augmented SGM; ABGM: Augmented BGM

Table 4.3: Monte Carlo Analysis - Basic specification

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Baseline	SA on correlation and n. of variables				SA on sample size					
PPML	-1.00	-1.00	-0.99	-0.99	-1.00	-0.97	-0.97	-0.95	1.00	-1.47	-1.47
	[.037]	[.037]	[.097]	[.046]	[.290]	[.139]	[.053]	[.140]	[.535]	[2.782]	[2.782]
LASSO		-1.00	-1.00	-0.99	-1.00	-0.96	-0.97	-0.93	-0.91	-0.87	-0.87
		[.037]	[.062]	[.044]	[.223]	[.132]	[.053]	[.133]	[.190]	[.134]	[.134]
ELASTIC NET		-1.00	-1.00	-1.00	-1.00	-0.96	-0.97	-0.93	-0.91	-0.87	-0.87
		[.037]	[.037]	[.037]	[.038]	[.131]	[.053]	[.133]	[.189]	[.131]	[.131]
RIDGE		-1.00	-1.00	-1.00	-1.00	-0.94	-0.98	-0.92	-0.85	-0.84	-0.84
		[.037]	[.037]	[.037]	[.037]	[.125]	[.060]	[.126]	[.147]	[.160]	[.160]
POST LASSO PPML							-0.93	-0.94	-0.99	-0.95	-0.95
							[.499]	[.139]	[.266]	[.165]	[.165]
Regressors	4	14	14	104	104	104	104	104	234	256	1004
Random variables	NO	10	10	100	100	100	100	100	230	252	1000
Correlation	NO	.1	.9	.1	.9	.9	.9	.9	.9	.9	.9
Sample size	52521	52521	52521	52521	52521	525	9471	576	256	256	256
Exporters	ALL	ALL	ALL	ALL	ALL	ALL	ALL	3	2	2	2
Years	ALL	ALL	ALL	ALL	ALL	ALL	3	3	2	2	2

I use 200 replications. For Lasso, Ridge and Elastic Net $\alpha = 0$, $\alpha = 1$, $\alpha = 0.5$. True parameter for distance is 1.

Col. 6: 1% observations randomly selected; Col. 7: 2009-2011; Col. 8: Canada, USA, China exporters.

Top values are mean estimates (rounded at the third decimal). Standard deviation of estimates in "[ ]". SA: "Sensitivity Analysis"

Table 4.4: MC Analysis for $\Phi_{ij} = e^{-lnD_{ij}} \cdot \eta_{ij}$

		Basic Gravity Model			
	PPML	LASSO	RIDGE	ELASTICNET	PLPPML
Min	-2.48	-2.33	-1.12	-2.32	-2.45
Median	-1.01	-0.99	-1.00	-0.99	-1.00
μ	-1.00	-1.00	-1.01	-1.00	-1.00
Max	-0.20	-0.35	-0.93	-0.36	-0.20
MSE (%)	8.36	5.10	0.13	5.09	8.06
		Structural Gravity Model (FE)			
	PPML	LASSO	RIDGE	ELASTICNET	PLPPML
Min	-2.25	-2.12	-1.12	-2.11	-2.24
Median	-1.00	-0.99	-1.00	-0.99	-1.00
μ	-1.00	-1.00	-1.01	-1.00	-0.99
Max	-0.25	-0.34	-0.93	-0.39	-0.26
MSE (%)	7.79	5.01	0.13	4.95	7.46
		Augmented Structural Gravity Model (FE+R+RTA)			
	PPML	LASSO	RIDGE	ELASTICNET	PLPPML
Min	-2.15	-2.03	-1.12	-2.02	-2.14
Median	-0.99	-0.99	-1.00	-0.99	-0.99
μ	-0.99	-0.98	-1.00	-0.99	-0.98
Max	-0.27	-0.34	-0.93	-0.36	-0.26
MSE (%)	7.45	4.04	0.13	3.92	5.83
		Augmented Basic Gravity Model (R+RTA)			
	PPML	LASSO	RIDGE	ELASTICNET	PLPPML
Min	-2.32	-2.15	-1.12	-2.15	-2.30
Median	-0.99	-1.00	-1.00	-1.00	-0.99
Mean	-0.99	-0.99	-1.00	-0.99	-0.98
Max	-0.244	-0.41	-0.93	-0.37	-0.31
MSE (%)	7.80	2.96	0.13	2.82	4.36

All models include 100 zero-mean noise terms, with .9 correlation with distance.
Sample size is 52521 for all specifications.
MSE is computed, in % terms, dividing $\sum(\hat{\delta_1} - \delta)^2/$ by the number of replications (200).

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Table 4.5: MC Analysis for $\Phi_{ij} = e^{-lnD_{ij}+.5AnyRTA} \cdot \eta_{ij}$

	PPML	LASSO	RIDGE	ELASTICNET	PLPPML
Basic Gravity Model					
Min	2.70	-2.48	-1.23	-2.47	-2.67
Median	-1.11	-1.09	-1.12	-1.09	-1.11
μ	-1.11	-1.11	-1.11	-1.12	-1.11
Max	-0.20	-0.33	-1.03	-0.34	-0.18
MSE (%)	11.44	7.03	1.39	7.24	10.97
Structural Gravity Model (FE)					
	PPML	LASSO	RIDGE	ELASTICNET	PLPPML
Min	-2.48	-2.32	-1.23	-2.32	-2.47
Median	-1.12	-1.10	-1.11	-1.11	-1.11
μ	-1.11	-1.11	-1.11	-1.11	-1.11
Max	-0.28	-0.35	-1.03	-0.35	-0.27
MSE (%)	10.68	7.38	1.39	7.37	10.13
Augmented Structural Gravity Model (FE+R+RTA)					
	PPML	LASSO	RIDGE	ELASTICNET	PLPPML
Min	-2.32	-2.17	-1.23	-2.18	-2.30
Median	-1.02	-1.09	-1.11	-1.09	-1.09
μ	-1.03	-1.06	-1.11	-1.06	-1.05
Max	-0.20	-0.31	-1.03	-0.32	-0.21
MSE (%)	9.31	5.47	1.39	5.50	7.67
Augmented Basic Gravity Model (R+RTA)					
	PPML	LASSO	RIDGE	ELASTICNET	PLPPML
Min	-2.51	-2.33	-1.23	-2.33	-2.48
Median	-1.03	-1.09	-1.12	-1.09	-1.09
μ	-1.03	-1.06	-1.11	-1.06	-1.05
Max	-0.18	-0.41	-1.03	-0.37	-0.28
MSE (%)	9.84	4.44	1.39	4.44	6.28

All models include 100 zero-mean noise terms, with .9 correlation with distance.

Sample size is 52521 for all specifications.

MSE is computed, in % terms, dividing $\sum(\hat{\delta_1} - \delta)^2/$ by the number of replications (200).

Table 4.6: MC Analysis for $\Phi_{ij} = e^{-lnD_{ij}+.5\#RTA} \cdot \eta_{ij}$

	PPML	LASSO	RIDGE	ELASTICNET	PLPPML
Basic Gravity Model					
	PPML	LASSO	RIDGE	ELASTICNET	PLPPML
Min	-4.44	-3.94	-1.79	-3.91	-4.37
Median	-1.37	-1.40	-1.47	-1.41	-1.39
μ	-1.40	-1.42	-1.48	-1.42	-1.40
Max	0.25	-0.04	-1.39	0.05	0.25
MSE (%)	49.26	39.12	23.59	38.74	48.18
Structural Gravity Model (FE)					
	PPML	LASSO	RIDGE	ELASTICNET	PLPPML
Min	-3.68	-3.46	-1.78	-3.48	-3.62
Median	-0.93	-1.13	-1.47	-1.14	-0.95
μ	-0.95	-1.13	-1.48	-1.14	-0.96
Max	0.61	0.25	-1.39	0.33	0.60
MSE (%)	31.15	26.96	23.59	26.69	30.65
Augmented Structural Gravity Model (FE+R+RTA)					
	PPML	LASSO	RIDGE	ELASTICNET	PLPPML
Min	-3.25	-2.95	-2.58	-2.92	-3.24
Median	-0.96	-1.05	-1.13	-1.05	-0.98
μ	-0.97	-1.06	-1.13	-1.06	-0.98
Max	0.36	0.01	-0.20	-0.08	0.31
MSE (%)	26.56	16.58	12.90	16.34	25.43
Augmented Basic Gravity Model (R+RTA)					
	PPML	LASSO	RIDGE	ELASTICNET	PLPPML
Min	-3.45	-3.09	-2.77	-3.08	-3.48
Median	-0.97	-1.02	-1.10	-1.03	-0.98
μ	-0.97	-1.03	-1.09	-1.04	-0.98
Max	0.38	0.05	-0.09	0.01	0.39
MSE (%)	28.06	17.36	12.93	17.12	27.00

All models include 100 zero-mean noise terms, with .9 correlation with distance.
Sample size is 52521 for all specifications.
MSE is computed, in % terms, dividing $\sum(\hat{\delta_1} - \delta)^2/$ by the number of replications (200).

Table 4.7: Estimates of the bilateral accessibility using the dataset of Silva and Tenreyro (2006)

	Basic Gravity	Structural Gravity
PPML	-0.75	-0.78
	(0.041)	(0.055)
LASSO	-0.72	-0.74
RIDGE	-0.42	-0.52
ELASTICNET	-0.71	-0.74
PLPPML	-0.80	-0.75

.

Table 4.8: List or Exporters and Importers

Exporters	Importers
Exporters	Importers
Armenia	Armenia
Australia	Australia
Austria	Austria
Azerbaijan	Azerbaijan
Bangladesh	Bangladesh
Belgium	Belgium
Brazil	Brazil
Brunei	Brunei
Cambodia	Cambodia
Canada	Canada
China	China
Czech Republic	Czech Republic
Denmark	Denmark
Fiji	Fiji
Finland	Finland
France	France
Georgia	Georgia
Germany	Germany
Greece	Greece
Hong Kong, China	Hong Kong, China
Hungary	Hungary
Iceland	Iceland
India	India
Indonesia	Indonesia
Iran, Islamic Rep.	Iran, Islamic Rep.
Italy	Italy
Japan	Japan
Kazakhstan	Kazakhstan
Kiribati	Kiribati
Korea, Rep.	Korea, Rep.
Kyrgyz Republic	Kyrgyz Republic
Luxembourg	Lao PDR
Macao	Luxembourg
Malaysia	Macao
Maldives	Malaysia
Continued on next page	

Table 4.8 – continued from previous page

Exporters	Importers
Mexico	Maldives
Mongolia	Mexico
Nepal	Mongolia
Netherlands	Nepal
New Zealand	Netherlands
Norway	New Zealand
Pakistan	Norway
Papua New Guinea	Pakistan
Philippines	Papua New Guinea
Poland	partnername
Portugal	Philippines
reportername	Poland
Russian Federation	Portugal
Samoa	Russian Federation
Singapore	Samoa
Slovak Republic	Singapore
Solomon Islands	Slovak Republic
South Africa	Solomon Islands
Spain	South Africa
Sri Lanka	Spain
Sweden	Sri Lanka
Switzerland	Sweden
Tajikistan	Switzerland
Thailand	Tajikistan
Tonga	Thailand
Turkey	Tonga
Turkmenistan	Turkey
United Kingdom	Turkmenistan
United States	United Kingdom
Vanuatu	United States
Vietnam	Uzbekistan
	Vanuatu
	Vietnam

Chapter 5

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