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Agricultural (dis)Incentives and Food Security: Is there a Link?

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Abstract

This paper analyzes the impact of agricultural (dis)incentives on food security for a wide sample of countries over the period 1990-2010 using the World Bank database on distortions to agricultural incentives. We adopt a continuous treatment approach applying a generalized propensity score matching to reduce potential biases stemming from difference in observed country characteristics. The results provide strong evidence of self-selection and heterogeneous food security impacts at different levels of policy intensity. Estimates of the dose-response functions show that both discrimination against agriculture and large support lead to poor performances in several dimensions of FS.

Keywords: Food security, agricultural incentives, impact evaluation, GPS, cross-country analysis

JEL classification: C210, F140, F600, O500, Q170

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1. Introduction

Throughout the world and over much of history, the agricultural and food sector has been subjected to some of the most heavy-handed governmental interventions. In 2004, existing agricultural and trade policies accounted for an estimated 70 percent of the global welfare cost of all merchandise trade distortions, even though the agricultural sector contributes only 6 percent of global trade and 3 percent of global GDP (Anderson *et al.*, 2010). Agricultural (dis)incentives have been newsworthy since 2008 as food prices have spiked upward. Responses by food-surplus developing countries typically have involved restrictions on exports, while those by food-deficit developing countries have involved a lowering of import barriers. Policymakers' ostensible motivation to adopt agricultural (dis)incentives has been to protect its domestic consumers and to prevent a decline in national food security (FS) (Anderson *et al.*, 2013b). While there is some consensus on the explanation of the empirical evidence on agricultural protection/taxation (Swinnen, 2010), the debate on the impact of these measures is still animated. Most of the literature is currently focusing on the impact on price level and volatility (Anderson and Nelgen, 2012a,b; Anderson *et al.*, 2013a), yet, surprisingly little hard evidence exists regarding the link between agricultural (dis)incentives and potential gains or losses in FS (FAO, 2015). The main challenge relates to the fact that this link depends on a variety of underlying factors that produces large differences in country experiences. Accordingly, the literature produced several typologies mapping countries at different levels of development and with different agro-climatic conditions and/or natural endowments for food production (Matthews, 2013; Yu *et al.*, 2010). This implies that the policy intervention is not random and the tendency of countries with different FS outcomes to adopt different policies is likely to drive the results of the empirical analysis if one does not control for possible sources of self-selection bias. The most recent empirical literature addresses this source of endogeneity by relying on impact evaluation methods and, in particular, using non-parametric matching techniques (Heckman *et al.*, 1998; Moffitt, 2004; Angrist and Pischke, 2009; Abadie and Imbens, 2006). In this respect, several matching techniques are used to adjust for differences in outcomes unrelated to treatment that give rise to selection bias.

To this end, we present a convenient non parametric functional reduced form to ascertain the presence of causal aggregate relationship between agricultural incentives and FS that goes beyond the evidence provided by single country case studies. Our analysis takes advantage of the World Bank data set providing the annual estimates of a set of standardized measures on distortions to agricultural incentives (Anderson and

Nelgen, 2012c).[1] This data set overcomes the need to convert different instruments (e.g., tariffs, quotas, anti-dumping duties, technical regulations, etc.) into a common metric. We present our empirical analysis for a sub-sample of 64 countries along the sub-period 1990-2010 (see Table A.1 in the Appendix for the list of countries included in our empirical exercise). As matching technique we apply a generalization of the binary treatment propensity score [2], namely the Generalized Propensity Score (GPS) originally proposed by Hirano and Imbens (2004) and Imai and van Dyk (2004), where matching is performed among countries with different treatment intensity. Differently from the standard matching applications, GPS does not look for non-treated units (or counterfactuals) but takes advantage of the multivalued nature of agricultural policy measures to assess the relationship between agricultural (dis)incentives and FS. This seems particularly appropriate for the agricultural policy where its intensity largely varies. The GPS method has been recently applied to various impact evaluation problems lacking experimental conditions: e.g., the impact of labor market programs (Kluve, 2010; Kluve *et al.*, 2012), regional transfer schemes (Becker *et al.*, 2012), foreign direct investments (Du and Girma, 2009), the EU preferential margins (Magrini *et al.*, 2014), and also the relationship between migration and trade (Egger *et al.*, 2012). It has been also applied to FS but with a different treatment, namely the impact of improved maize technologies on smallholders welfare

The outcomes of our estimates confirm the likely presence of a self-selection bias in the causal aggregate relationship between agricultural incentives and various dimensions of FS. Moreover, a positive level of support to the primary sector is associated with better performances in most of the FS dimensions (i.e., food availability, access and utilization). This impact is not consistent with the usual claims by free trade supporters that most policies are either ineffective or harmful not only in terms of efficiency but also with respect to the achievement of specific goals. While our analysis clearly shows that agricultural (dis)incentives are effective in influencing FS, it does not allow normative implications to be drawn, since it does not consider the costs of the treatment. However, the evidence of a positive impact in terms of FS is certainly important, and it justifies further analysis of the actual policy mix adopted in correspondence to different treatment levels.

The paper is organized as follows: Section 2 briefly summarizes the conceptual framework linking agricultural (dis)incentives and FS; Section 3 discusses the methodological approach adopted to study this

[1]The World Bank database on agricultural incentives is not the only attempt to measure policy-induced effect on agricultural market prices. Other initiatives are the OECD Producer and Consumer Support Estimates database for OECD countries; the FAO Monitoring and Analyzing Food and Agricultural Policies database for Sub-Saharan countries; and the IDB Producer Support Estimates database for Latin America and Caribbean. We prefer to use the World Bank data set because none of the other data sets provide the country or period coverage required to perform the present analysis.

[2]The propensity score is the probability that an agent takes treatment conditional on the available co-variates.

relationship. Section 4 presents the GPS estimator and Section 5 describes variables and data. The results of the empirical analysis are discussed in Section 6, and Section 7 concludes.

2. The conceptual framework

The international community agrees on a definition of FS that emphasizes its multidimensionality. Specifically, FS is described as the condition that "exists when all people, at all times, have physical, social and economic access to sufficient, safe and nutritious food to meet their dietary needs and food preferences for an active and healthy life" (CFS, 2009). According to the above definition four pillars of FS have been identified, namely *availability*, *access*, *utilization* and *stability* (CFS, 2009). Availability is a measure of the amount of food physically available in a population during a certain period of time (most likely related to production and market availability) (Cafiero, 2013). The accessibility dimension embraces Sen's framework of the capability approach (Sen, 1981) emphasizing that food availability does not guarantee that everyone is free from hunger. The third dimension - utilization - is a measure of a population's ability to obtain sufficient nutritional intake and nutrition absorption during a given period. The last dimension - stability - refers to the risk component of the above three (such as natural events, man-made shocks, malfunctioning international markets, etc.) (Pangaribowo *et al.*, 2013).

Agricultural incentives affect all the four pillars of FS directly through the impact on food availability, and indirectly through the effects on food accessibility, utilization and stability. In fact, changes in the policy regime have a direct effect on food availability as well as on both rural and urban incomes and employment, and through these on income distribution and utilization. In addition, there is an effect on government revenues through, for example, a change in the level of revenue from import levies as well as on the domestic or foreign nutritional contents embodied in production, exports and imports

These features go a long way in explaining why there is little or no consensus about the empirical relationship between agricultural incentives and FS and a thorough analysis of the exact channels of transmission is a complex undertaking. For instance, a recent review (McCorriston *et al.*, 2013) on the evidence for links between agricultural trade liberalization in developing countries and FS found that of 34 studies, 13 reported that FS would improve, 10 that it would decline, while the remaining 11 reported a mixed outcome with FS metrics varying across segments of the population, regions and time or with alternative FS metrics indicating different outcomes for specific countries.

An ex post evaluation of the effectiveness of the agricultural incentives should assess whether and to what extent they really changed the FS performance. Ex post studies observe actual performance in countries that have undergone policy reforms in the past few years searching for explanatory factors. Adebua *et al.* (2002); Pyakuryal *et al.* (2010); Huchet Bourdon and Laroche Dupraz (2014a) are examples of cost of living approaches using household expenditure data to examine the impact of policy related changes in commodity markets on FS indicators.

A difficulty in this approach is that it does not provide a formal econometric assessment of the causal effect of different treatment intensity among observations that can be considered similar, conditional on a set of common characteristics [3].

3. The methodological approach

Despite the wide empirical literature on the assessment of the overall net impact of agricultural incentives, the nexus with FS has rarely been investigated using the rich toolkit of matching econometrics, i.e. the set of the parametric and non-parametric methods used to control for self-selection bias controlling for unobserved heterogeneity in treatment propensity related to outcome performances.

The advantage of matching econometrics is that it does not require separability of outcomes or choice equations, exogeneity of regressors or exclusion restrictions, or adoption of specific functional forms of outcome equations such as a model relating the outcome unobservables to the observables, including the choice of treatment. These positive features are conditional on the validity of a set of assumptions such as the the randomness of the treatment (i.e. the "unconfoundedness" or ignorability of the treatment") which assumes that, conditional on observable characteristics, the treatment can be considered as random. Other crucial assumptions are the SUTVA (Stable Unit Treatment Value Assumption) condition - which implies the "unique treatment assumption" (i.e., the treatment is identical for each treated observation) and the "non-interference assumption" (i.e., observation on one unit should be unaffected by the particular assignment of treatments to the other units) - and the "overlap assumption", i.e., the need to maintain an adequate balance of observations between treatment and control groups.

Three major issues emerge in adopting this methodological approach to address the policy impact on FS. The first possible issue of the agricultural (dis)incentives when considered as a "treatment" is that they should be associated to a clearly recognized ex-ante target, i.e. an outcome variable with respect to which the treatment/policy impact can be evaluated. In this respect, it remains questionable whether and how FS can be univocally expressed by macro variables, that eventually express the effect of the policies. Indeed,

[3] For an analysis that attempts to control for the relevant co-determinants of FS see (Bezuneh *et al.*, 2014).

our impact evaluation addresses the issue through methods and measures that are aggregate or indirect or partial effects of the treatment. This is however, on the one hand, highly consistent with the efforts made by the relevant literature in the field to build up an aggregate conceptual framework to analyze the full set of interactions between policy treatments and FS outcomes (Diaz-Bonilla and Ron, 2010; Diaz-Bonilla *et al.*, 2002; Smith, 1998; Huchet Bourdon and Laroche Dupraz, 2014b). On the other hand, it takes advantage of the available sets of aggregate indicators and measures of FS widely used by international organizations and policy makers for policy analyzes.

A second issue is that the treatment under study must be clearly identified and observable. Apparently, the World Bank database on distortions to agricultural and food markets has substantially increased the clarity and the identifiability of the treatment associated to the agricultural incentives. Many and heterogeneous coupled instruments have been transformed into a unique index. However, still a complication arises in considering the agricultural incentives as a "treatment" because, unlike other typical policy program evaluations (e.g., job training programs), the treatment associated to the policies is not a binary treatment. The agricultural incentive is a continuous treatment where units are treated but with different intensity. Therefore, in the case of the agricultural (dis)incentive the treatment effect reasonably depends on the treatment intensity and not only on being treated or not. Nonetheless, in the following section we will see that the multivalued nature of the treatment, rather than being a limitation, actually represents the key opportunity to identify and estimate the treatment effect of the policies.

The final and more important issue represents the key limitation that may prevent the application of matching econometrics to the evaluation of the agricultural incentives. The economies affected by agricultural incentives may also undergo other treatments that can affect, directly or indirectly, production and consumption choices, thus confusing the impact of the policies considered. Counterfactual observations must exist to apply the model logic; that is, observations where the outcome variable(s) is (are) observed without the treatment (control group or counterfactuals). In the case of the agricultural incentives, however, finding a proper strategy to identify counterfactuals and compare them to treated units represents an often unsolved research challenge. On the one hand, the non-treated units (that is, countries not adopting any agricultural incentives) are rare. On the other hand, they may not be treated just because of their peculiar situation. Even though a non-treated sample could be observed, it can be hardly considered a proper counterfactual sample because of this peculiarity, that is, those unobserved characteristics that affect, at the same time, the outcome and the treatment assignment. In other terms, the specificity of the agricultural (dis)incentives as a treatment makes almost impossible to get rid of the selection-on-(un)observables bias (Esposti, 2014).

When dealing with the agricultural incentives, all these issues simultaneously arise. In such uncomfortable and unconventional condition (multiple outcomes, multiple and multivalued treatments, no natural counterfactuals), the GPS brings a number of clear advantages: firstly, it lets us control for the endogeneity bias due to the fact that agricultural incentives are not exogenous random variables and are likely to be endogenously determined by and correlated with the governmental objectives; secondly, it allows for agricultural incentives characterized by different intensities; thirdly, it helps us to isolate the impact of sectoral policies from any other confounding event and takes into account of the presence of non-linearities in the relationship among policies and related outcomes. Finally and most importantly, working with heterogeneity in the policy intensities it does not need a control group with similar characteristics - which is mandatory with the binary treatment matching techniques.

4. The GPS estimator

In many observational impact studies, as in the case of agricultural (dis)incentives, one may be interested in estimating the dose-response function in a setting with a continuous treatment. To this end, Hirano and Imbens (2004) developed a generalized version of the Propensity Score methodology to estimate the marginal effect of a specific treatment level on the outcome of interest by using the dose-response function (average treatment effect) in a continuous treatment. The GPS is a non-parametric method to correct for selection bias in a setting with a continuous treatment by comparing units that are similar in terms of their observable determinants of "treatment intensity" within the treatment group. Hence, it does not require control groups. The basic setup of the GPS method is described below based on Hirano and Imbens (2004) and Imai and van Dyk (2004).

Let us use index $i = 1, ..., N$ to refer to a random sample of units. The GPS method is based on the following assumptions: for each i we postulate the existence of a set of potential outcomes, $Y_i(t)$, for $t \in \Gamma$, where Γ is a continuous set of potential treatment values[4]. Hirano and Imbens (2004) refer to $Y_i(t)$ for $t \in \Gamma$ as the *unit-level* dose-response function (DRF). We are interested in estimating the *average* DRF, $D(t)$, across all units i that illustrates the expected value of the outcome variable conditional on continuous treatment as follows:

$$D(t) = E[Yi(t)] \tag{1}$$

Estimation of the $D(t)$ uses information on three sets of data: a vector of covariates X_i, the treatment received

[4]Here Γ is an interval $[t_0, t_1]$.

T_i and the potential outcome corresponding to the level of the treatment received, $Y_i = Y_i(T_i)$. Following Hirano and Imbens (2004) we assume that: $Y_i(t)_{t\in\Gamma}$, T_i and X_i are defined on a common probability space; T_i is continuously distributed with respect to a Lebesgue measure on Γ; and that $Y_i = Y_i(T_i)$ is a well defined random variable. To simplify the notation, we will drop the i subscript in the sequel.

Let $r(t, x)$ be the conditional density of the treatment given the covariates:

$$r(t, x) = f_{T|X}(t|x) \tag{2}$$

Then the GPS is:

$$R = r(T, X) \tag{3}$$

The main purpose of estimating GPS is to create covariate balancing. However, the validity of R as a measure of similarity or dissimilarity across observations depends crucially on the validity the set of assumptions which are standard in the impact evaluation literature (see Section 3). Firstly, the randomness of the treatment, i.e. the assumption of "unconfoundedness" or "ignorability of the treatment". Imbens (2000) shows that if the treatment assignment is weakly unconfounded given the observed covariates, then the treatment assignment is weakly unconfounded given the GPS. In other words, the GPS has the following property:

$$X \perp 1\left\{T = t\right\} | r(t, X)(4)$$

The GPS removes the bias associated with differences in covariates in three steps. In the first step, the GPS is estimated, and its balancing property is checked[5]. If balancing holds, observations within GPS strata can be considered as identical in terms of their observable characteristics, independently of their actual level of treatment.[6]

The validity of the balancing property should be coupled with the SUTVA condition. This assumption, has already explained in Section 3, has two parts: the "unique treatment assumption" (i.e. the treatment

[5]Using a normal distribution for the treatment given the covariates:

$$T_i|X_i \sim N(\beta_0 + \beta_1' X_i, \sigma^2) \tag{5}$$

we can estimate the parameters $\widehat{\beta}_0$, $\widehat{\beta}_1$ and $\widehat{\sigma}^2$ by maximum likelihood. Hence the estimated GPS is:

$$\widehat{R}_i = \frac{1}{\sqrt{2\pi\widehat{\sigma}^2}} exp\left[-\frac{1}{2\widehat{\sigma}^2}(T_i - \widehat{\beta}_0 - \widehat{\beta}_1'^2 X_i)^2\right]. \tag{6}$$

[6]Please note that as long as sufficient covariate balance is achieved, the exact procedure for estimating the GPS is of secondary importance (Kluve *et al.*, 2012).

is identical for each treated observation) and the "'non- interference assumption"', i.e. observation on one unit should be unaffected by the particular assignment of treatments to the other units. Notwithstanding the presence of some degree of heterogeneity in policy coverage, here we use a standardized measure that prevents violation of the unique treatment assumption. This measure synthesizes the actual impact of the set of the standard trade policy incentives, both tariff and non-tariff, that characterizes policy distortions across countries in a uniform manner and in a comparative setting, thus softening the possible violation of SUTVA.

Instead, in order to soften the likely risk of interference and spillovers due to treatments assigned to the main exporter and importer countries, we removed from the sample the countries reporting the highest quotas in terms of trade value at the global level in the agricultural sector during the period 1990-2010, using Direction of Trade Statistics (DOTS). In particular, we remove both the top 10 exporters and the top 10 importers.[7]

Then, two additional steps are needed to eliminate the bias associated with differences on the covariates (see Hirano and Imbens (2004) for a proof). The first one is estimation of the conditional expectation of the outcome as a function of two scalar, the treatment level T and the GPS R as follows:

$$\beta(t, r) = E[Y|T = t, R = r] = \psi[t, r; \alpha] \tag{7}$$

where α are the parameters to be estimated. This is generally assumed to be a flexible parametric specification between the three variables at different order of the polynomial terms. The statistical significance of the GPS parameters is a sign that selection bias is actually an issue. Interaction terms between the treatment level and the GPS are also applied to control for the marginal impact of the treatment relative to the GPS.

The final step is to estimate the average dose-response function (DRF) of the outcome (i.e. the different dimensions of FS) by averaging the conditional expectation over the GPS at any different level of treatment, as follows:

$$D(t) = E[\beta(t, r(t, X))] \tag{8}$$

Furthermore, we can calculate the varying marginal effects of the treatment by estimating the treatment

[7] Note that 8 countries out of 10 are both main exporters and importers, namely the United States, Germany, France, Italy, Spain, the Netherlands, Belgium and China. Brazil and Canada are the other two main exporters, while Japan and the UK are the other two main importers. We also performed the same exercise removing all the EU member countries, but the final results (available upon request) did not change significantly. Unfortunately, the same sample selection could not be applied to the sensitivity analyzes carried out for rice and wheat because of the limited number of observations that would be available after the correction.

effect function (TEF), which is the first derivative of the corresponding DRF as follows:

$$\theta(D) = D(t+\delta) - D(t) \tag{9}$$

5. Variables and data

In our exercise we use index i to indicate countries and assume the unit-level dose-response of potential outcomes of FS, Y_{it} as a function of the treatment t, where t is the annual Nominal Rates of Assistance to producers (NRA). To this end, three different sets of data are used: i) the NRA (i.e. the treatment, T_i) derived from the World Bank data set ("Updated National and Global Estimates of Distortions to Agricultural Incentives, 1955 to 2010") by Anderson and Nelgen (2012c); ii) the observable characteristics able to explain the probability of reaching a specific level of NRA (i.e. the covariates, X_i); iii) the outcome in terms of the various dimensions of FS ($Y(t)$). Table A.2 in the Appendix describes variables and data sources used in our empirical exercise, while Table A.3 provides summary statistics for the covariates and the outcomes.

5.1. The treatment: the Nominal Rates of Assistance

The Anderson and Nelgen (2012c)'s World Bank data set provides the annual values of a set of standardized measures of policy-related agricultural trade distortions for a total of 82 countries (that together account for over 90% of global agricultural output) and 70 products for the overall period 1955-2011. It contains aggregate and by product NRA measures defined as the percentage by which government policies directly raise (or lower) the gross return to producers of a product above the world price. The focus is on border and domestic measures that are due exclusively to governments' actions, and as such can be altered by a political decision and have an immediate effect on consumer choices, producer resource allocation, and net farm incomes (Anderson and Valenzuela, 2008). In practice, there are divergences among farmer, processor and wholesaler, consumer, and border prices for reasons other than policies. These costly value chain activities have been explicitly recognized and netted out to derive estimates of government policy-induced distortions. More specifically, NRA is computed as follows:

$$NRA = [E.P(1+d) - E.P]/E.P$$

where E is the exchange rate, d is a distortion due to government interventions and P is the foreign price of an identical product in the international market (Anderson, 2006). Positive values of NRA denote a rise in domestic producers' gross return (the distorted price is higher than the undistorted equivalent because of the presence of an output subsidy and/or a consumption tax, e.g. a tariff), while negative values denote a lower gross return for domestic producers (the producers receive less than the price would be for a like product in the absence of government interventions, e.g., an export tax).

Across countries and time periods, governments have used a broad array of policy instruments. They include distortions to input markets (largely subsidies, plus controls on land use), production quotas, marketing quotas, target prices, price subsidies or taxes in output markets, and especially, border measures that directly tax, subsidize, or quantitatively restrict international trade. The major vehicles responsible for these losses are trade-policy instruments such as export and import taxes and subsidies or quantitative restrictions, along with multiple exchange rates. These trade-policy instruments account for no less than three-fifths of agricultural NRAs globally. In contrast, internal domestic agricultural policies that directly subsidize or tax outputs and inputs contribute only minimally to NRAs (Anderson *et al.*, 2013b).

Two main hurdles, conversion and aggregation problems, need to be stressed. On the one hand, given the continuing and possibly growing importance of agricultural NTBs, protection can take many different forms - tariffs, quotas, anti-dumping duties, technical regulations - so that we need to convert the different instruments into a common metric (Cipollina and Salvatici, 2008). The WB database deals adequately with this issue by undertaking careful domestic-to-international price comparisons for the key farm products for a large set of OECD and developing countries, thereby capturing also the domestic price effects of NTBs (Lloyd *et al.*, 2010). This was computed by comparing domestic and border prices of like products (at similar points in the value chain) for each of the farm industries covered, drawing on national statistical sources supplemented - where necessary - by producer prices and unit values of exports and imports from FAO.

On the other hand, policy incentives are set at a very detailed level, and this information needs to be summarized into one aggregate and economically meaningful measure. The World Bank's database solves these problems as follows: "the weighted average NRA for covered primary agriculture can be generated by multiplying each primary industry's value share of production (valued at the farm gate equivalent undistorted prices) by its corresponding NRA and adding across industries. The overall sectoral rate, denoted as $NRAag$, can be obtained by adding the actual or assumed information for the non-covered commodities and, where it exists, the aggregate value of non-product-specific assistance to agriculture" (Anderson and Nelgen (2012c) p. 577).

5.2. The covariates

In regard to the set of covariates, we selected the following set of pre-treatment variables: the log of real per capita GDP, its squared and cubic power (to control for non linearities in the anti-trade behavior of the most advanced economies and to facilitate the balancing property as suggested by Dehejia and Wahba (1999) and Dehejia (2005)); the log of total population (to control for country size); the log of per capita arable land (to control for the relative agricultural comparative advantage); the food production index (to control for the actual productivity of the agricultural sector); the share of the value of food imports over total exports and its squared power (to control for the level of country food dependence on abroad); the absolute percentage (positive and negative) deviations from the trend in food international prices (to control for the presence of asymmetric policy responses to sizable changes in price levels); a measure of international food price volatility (to control also for the second moment of the relationship between international prices' dynamics and trade distortions). Furthermore, we add a set of dummies to control for net exporter status and the recent food crisis (2007/2008), and a set of unobservable factors for the groups of countries belonging to the same regional area - African developing countries (base); Asian developing countries; European transition economies; Latin American developing countries and high income countries. We are perfect aware that other determinants could be considered in our matching exercise. However, as argued by Bryson *et al.* (2002), there is always a trade-off between increasing the explanatory power due to the use of additional covariates and the risk of over-parametrized models that could, in turn, exacerbate the support problem and increase the variance of the propensity score estimates.

5.3. The outcome: dimensions of FS

Last but not least, we need to deal with the task of retrieving suitable and workable measures of FS since it covers a complex set of concepts and dynamics. FS is characterized by multiple dimensions and can be defined either at the national, local, household or individual level. This clearly poses a challenge, and for lack of an obvious measure that encompasses all these aspects, the literature has used more than 450 indicators (Hoddinott, 1999). As underlined by Cafiero (2013) and Pangaribowo *et al.* (2013), each dimension can be represented by a specific set of variables and indicators. Unfortunately the set of choices at our disposal are limited by actual data availability. Specifically, there is a lack of annual panel data. suitable to be directly compared with our treatment and covariates data-sets, for most of the variables suggested by the specialized literature to assess the different dimensions of FS. Facing this data constraint, we selected the following variables in our empirical exercise: supply of food commodities in kilo-calories per person per year (as proxy for food availability); depth of the food deficit (for food access); infant mortality (for food utilization) and per-capita food supply variability (for stability) (see Table A.2 in the Appendix) for additional details and sources' availability. Instead of using a composite indicator of FS, our identification strategy looks at the specific causal relationship between agricultural incentives - proxied by NRA - and each dimension of FS. To this end, we take advantage of the use of the GPS, which is a non parametric and flexible empirical technique that lets us to identify different empirical DRFs for each FS dimension. We will see that the high consistency of the empirical results confirms the goodness of the adopted identification strategy.

6. Empirical results

We carry out the empirical exercise for each dimension of FS. Because of data constraints in FS measures, we are forced to limit our data set to the sub-period 1990-2010. Furthermore, to avoid possible sources of bias we omitted from the aggregate analysis the main importer and exporter countries (see Section 4).[8] Hence, our sample of countries reduces to 64 countries (see Table A.1 in the Appendix). Among the estimated trade distortion measures, we use $NRAag$ for the (aggregate) exercise (see Table A.2 in the Appendix). As in Anderson and Nelgen (2012c) and Anderson and Nelgen (2012a), NRA data have been converted to a nominal assistance coefficient ($NAC = 1 + NRA$) in order to transform NRAs negative values (i.e. when producers receive less than the price at the border in the absence of government intervention) into NAC values between zero and one (one becomes the threshold between a positive and negative NRA). Movements of domestic prices above the world ones, for instance through import taxes or export subsidies, lead to increases in NAC above 1, while trade liberalization is signaled by reductions in the NAC. However, when policies are targeted on domestic producers, as in the case of import subsidies or export taxes, the dynamics are reversed. In these cases, world prices are higher than domestic ones: NCAs are lower than 1 and they would increase due to trade liberalization. The bottom line is that care should be taken in interpreting the NAC dynamics when different types of border policies are taken into account. NAC observations before the 5 percentile and after the 95 percentile have been removed from the sample in order to clean our data set from potential outliers.

[8] It is worth noting that including them did not significantly change the empirical outcomes and the corresponding DRF. Similar outcomes were obtained on omitting the EU member countries from the analysis. The results are available from the authors upon request.

6.1. Regression outcomes

We first apply a regression type analysis to control for the possibility of reverse causality between FS and trade policy distortions. Specifically, we estimate the relation between the food availability dimension and NRA by using the following panel regression:

$$f_{id} = \alpha + \beta_1 t_{id} + \beta_2 x_{id} + \epsilon_{id} \tag{10}$$

where f is our proxy for food availability (food supply in kcal/capita/day); t is NRA; x is a bundle of observable country level covariates and ϵ is the error component. Countries are indexed $i = 1, ..., N$ and observed once per period $d = 1, ..., T$. To avoid the risk of endogeneity bias between food availability and agricultural incentives, i.e. the possibility that policy measures may be influenced by the level of food availability, following Serrano-Domingo and Requena-Silvente (2013) we apply a 2SLS instrumental variable procedure. As a valid instrument we use the simple moving average of NRA in the previous decade which is supposed to be correlated with the current level of NRA but uncorrelated with any other determinants of the future food availability.[9]

Table 1 reports both the OLS and the IV estimates of our panel analysis, with country and time fixed effects and robust standard errors. The coefficients of the food availability determinants[10] are highly significant and quite similar in both the OLS and IV estimates. NAC coefficients in the OLS and IV models are both significant as well, evidencing the consistency of both estimates. They loose significance in the IV model when the squared and cubic powers are introduced. The Hausman test does not reject the null hypothesis of the consistency of the parameters in the two models, confirming that the relationship between agricultural incentives and food availability does not suffer from reverse causality. However, regression-type analyzes do not rule out the risk of misspecification because of self-selection bias due to incomparable observations. For instance, the net food importer and exporter countries can be considered more likely to adopt agricultural trade distortions during the food price spikes and/or developing countries characterized by higher risks of food insecurity. Moreover, the most developed countries show on average relatively higher rates of protection. To deal with this issue, we thus apply the GPS approach.

[9] All the multivariate F-tests of excluded instruments reject the null hypothesis that the instruments are not correlated with the endogenous regressors. For column (4), the F-test is equal to 151.62 (p-value 0.000), for column (5) to 80.85 (p-value 0.000) and for column (6) to 54.67 (p-value 0.000).

[10] The covariates are selected according to the empirical literature on the macro drivers of food availability (Misselhorn, 2005; Feleke *et al.*, 2005; Garrett and Ruel, 1999; Iram and Butt, 2004; Rose, 1999; Pangaribowo *et al.*, 2013).

Table 1: **Regression type estimates: dependent variable *food availability***

	OLS			IV		
	(1)	(2)	(3)	(4)	(5)	(6)
NAC	0.026***	0.142***	0.410***	0.044**	0.159	-2.240
	(0.008)	(0.032)	(0.134)	(0.021)	(0.114)	(2.59)
NAC^2		-0.041***	-0.234**		-0.038	1.645
		(0.011)	(0.094)		(0.039)	(1.843)
NAC^3			0.043**			-0.370
			(0.021)			(0.412)
lnreal pc GDP	0.419***	0.425***	0.428***	0.411***	0.392***	0.403***
	(0.059)	(0.059)	(0.059)	(0.067)	(0.067)	(0.079)
lnreal pc GDP^2	-0.023***	-0.023***	-0.024***	-0.023***	-0.022***	-0.021***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
lnpc arable land	0.036***	0.036***	0.035***	0.037***	0.037***	0.042***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.01)
food prod. index	0.002***	0.002***	0.002***	0.002***	0.002***	0.002***
	0.000	0.000	0.000	0.000	0.000	0.000
pop	0.504***	0.515***	0.519***	0.507***	0.523***	0.458***
	(0.095)	(0.095)	(0.094)	(0.097)	(0.097)	(0.132)
pop^2	-0.036***	-0.036***	-0.037***	-0.036***	-0.037***	-0.034***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.007)
No. of observations	1098	1098	1098	1091	1091	1091
R^2	0.579	0.586	0.588	0.578	0.581	0.423
Hausman test:				0.409	0.386	0.150
country FE	yes	yes	yes	yes	yes	yes
year FE	yes	yes	yes	yes	yes	yes

Note: $(NAC) = (1 + NRA)$
Standard errors in parenthesis. ***,**,* denote significance at the 1, 5 and 10 per cent level, respectively.

6.2. GPS estimation and balancing property

As discussed in section 4, we first regress our measure of agricultural (dis)incentives on a set of pre-treatment observable characteristics and then estimate the GPS. Since the joint Jarque-Bera normality test strongly supports the null hypothesis of normal distribution of our treatment variable[11], we apply an OLS approach in the first stage estimation.

It is worth noting that the literature on matching provides no guidance on the choice of the conditioning variables that generate identification. More importantly, conventional model selection criteria sometimes used to pick the variables in the conditioning set do not necessarily work and adding variables that are statistically significant in the treatment choice equation is not guaranteed to select a satisfying set of conditioning variables (Heckman and Navarro-Lozano, 2004). On the other hand, it is also worth recalling that in impact evaluation exercises the functional form of the relationship as well as the interpretation and statistical significance of the individual effects of the covariates are of less importance than obtaining a powerful GPS (i.e. a GPS that works well in balancing the covariates by respecting the condition in 6).

[11] A zero-skewness log transformation has been applied to normalize the NAC distribution. The p-value is 0.611, well above the standard 5% threshold of statistical significance.

In this regard, the R-squared of our first stage regressions are quite high and consistent with those of similar GPS empirical exercises (Becker *et al.*, 2012; Serrano-Domingo and Requena-Silvente, 2013; Magrini *et al.*, 2014).

Table 2 presents the outcomes of the first stage equation.

Table 2: **Generalised Propensity Score Estimates**

Covariates	Coef.	SE (robust)
L.lnreal pc gdp	0.988**	0.456
L.lnreal pc gdp^2	-0.117**	0.055
L.lnreal pc gdp^3	0.005**	0.002
L.lnreal pc gdp^4		
L.ln pc arable land	-0.036***	0.006
L.pos dev food prices	-0.531***	0.142
L.neg dev food prices	-0.343***	0.107
food price volatility	-1.345***	0.468
food crisis	-0.048***	0.015
L.food prod. index	0.000	0.000
group 2 -Asian DCs	-0.065***	0.017
group 3 - Latin American DCs	-0.062***	0.018
group 4 - European Transition Economies	0.037**	0.019
group 5 - High-income Countries	0.014	0.028
L.net food exporter	-0.057***	0.008
L.food import/total exports	-0.678*	0.397
L.food import/total exports^2	2.816	1.760
L.pop	-0.094***	0.025
L.pop^2	0.005***	0.001
cons	-2.379*	1.285
No. of observations	1072	
R^2	0.436	

$(NAC) = (1 + NRA)$.
All time variant variables with one lag.
Standard errors in parenthesis. ***,**,* denote significance at the 1, 5 and 10 per cent level, respectively.

Anyway, inspection of the individual effects of the covariates shows very reasonable results: NAC tends to be higher, the higher a country's per capita income (even if at a decreasing rate) and the lower the country's comparative advantage in agriculture (proxied by the percentage of arable land). A negative relationship between NAC and country dimension (proxied by the population size) is apparent as well.

The above empirical evidence is consistent with the well-known "development pattern" whereby richer countries tend to maintain higher protection for domestic producers, while developing countries as well as bigger countries (proxied by population in thousands), tend to keep lower levels of NAC. Countries characterized by high dependence on food imports with respect to their total exports tend to maintain lower levels of NAC as well since they tend to reduce the domestic prices of importable (Valdés and Foster, 2012). The 'anti-trade pattern' is also confirmed because countries with a comparative advantage in agriculture as well as net food exporters tend to protect less. The impacts of positive and negative deviations of international food prices from their trend are consistent with the goal of governments to use policies in order to stabilize the domestic markets. NACs are negatively correlated with positive international food price deviations from their trend, since food import restrictions tend to be eased during price spikes, and export tax tends to be raised. Consistently, NACs are negatively correlated with negative international food price deviations from their trend, since overall food import restrictions tend to be stressed during price drops while net exporter countries adopt a pro-trade behavior (Anderson, 2013; Anderson and Nelgen, 2012c,a). Looking at the absolute value of the coefficient it appears that policy makers react more to price spikes than troughs. To be noted is that international food price volatility always impacts negatively on NACs, highlighting a strong correlation with trade distortions that imply lowering gross returns for domestic producers, probably because of the well-known depressive impact on consumption behavior of price volatility. This impact is significantly influenced by the most recent observations in the database. Finally, as expected, net food exporting countries and developing countries in Asia and Latin America show, on average and *ceteris paribus*, lower NACs (i.e. higher export protection). Last but not least, lower NACs are recorded, on average and *ceteris paribus*, during the years of "food crisis".

The second step of our impact evaluation exercise is to test the "balancing property". To this end, we compare the covariates across groups with and without the GPS correction. We first perform a series of two-sided t-tests across groups for each covariate (t-values reported in bold face indicate the presence of statistically significant differences at the 5% level). Four groups of approximately the same size are formed on the basis of the actual levels of NAC [12]. As is apparent from Table A.4 in the Appendix, before controlling for the GPS, there are differences across the treatment groups with respect to the covariates. The average t-stat is 4.85 (well above the 1.96 threshold) and 52 out of 72 tests reject the balancing assumption. Once we condition on the value of the GPS score - building 8 strata - and impose the common support condition, the improvement in the balancing of the covariates is evident. The average t-stat reduces to 0.88 and the balancing is still rejected in only 3 out of the 52 tests. Table A.5 in the Appendix reports the final group-strata structure of the data. Figures A1-A4 in the Appendix provide a succinct overview of the differences in the common support before and after GPS correction. Figure A5 in the Appendix show the map of sample countries with the percentage of observations excluded by the common support.

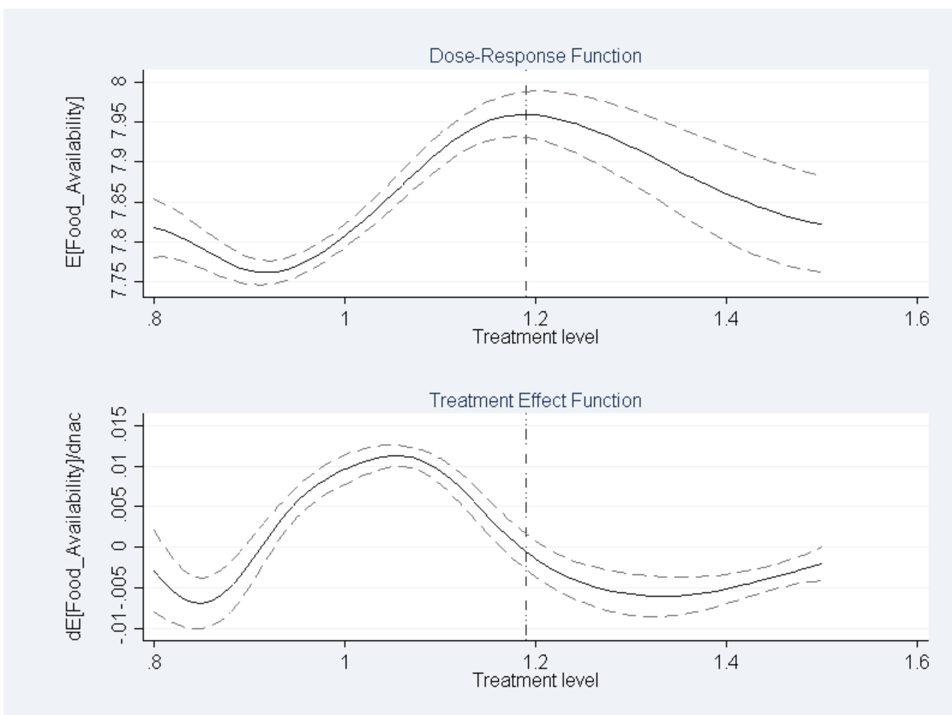
6.3. The Dose-Response Function

The last step is to estimate a dose-response function (DRF) illustrating, given the estimated GPS, if and how there is a causal link between NAC changes and each of the FS dimensions (eq.7). This requires

[12] We run t-tests for different numbers of groups before choosing the best combinations in terms of balancing properties

testing a polynomial parametrization of the conditional expectation of the outcome as a function of observed treatment and estimated GPS as a flexible function of its two arguments (see section 4). It is useful to recall that, as Hirano and Imbens (2004) point out, also the estimated coefficients in this regression have no direct economic meaning except that of testing whether the covariates introduce any bias. While the GPS coefficients control for selection into treatment intensities, the interaction term shows the marginal impact of the treatment relative to the GPS. If selectivity matters, we expect both the GPS and the interaction coefficients to be statistically significant. This means that the GPS method highlights possible bias in outcomes that are actually controlled by looking over GPS strata as well as across them, by using the interaction term. If GPS is statistically significant, we denote the likely presence of self-selection bias (i.e. unobserved heterogeneity in treatment propensity that may be related to the variables of outcomes) for unmatched observations. As in Bia and Mattei (2008) we use bootstrap methods to obtain standard errors and confidence intervals of the DRF that take estimation of the GPS and the α parameters into account.

Figure 1: DRF and TEF: Food availability dimension



Once we had tested our DRF at different order of the polynomial terms, as in Egger *et al.* (2012) we chose to disregard those polynomial terms that proved to be insignificant in our OLS regression estimates.

Figure 2: DRF and TEF: Food utilization dimension

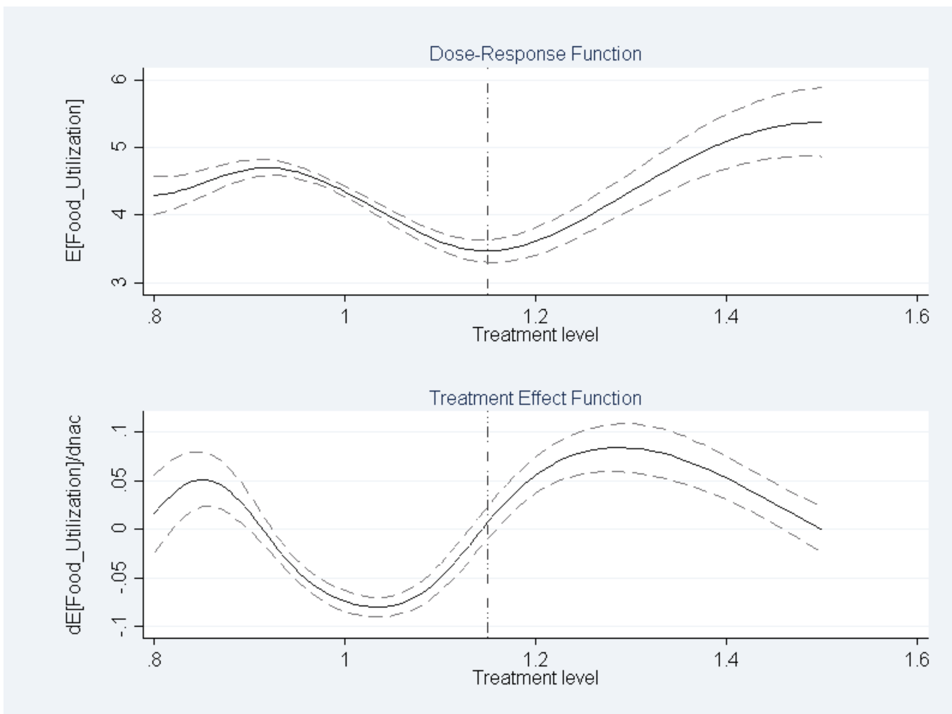


Figure 3: DRF and TEF: Food access dimension

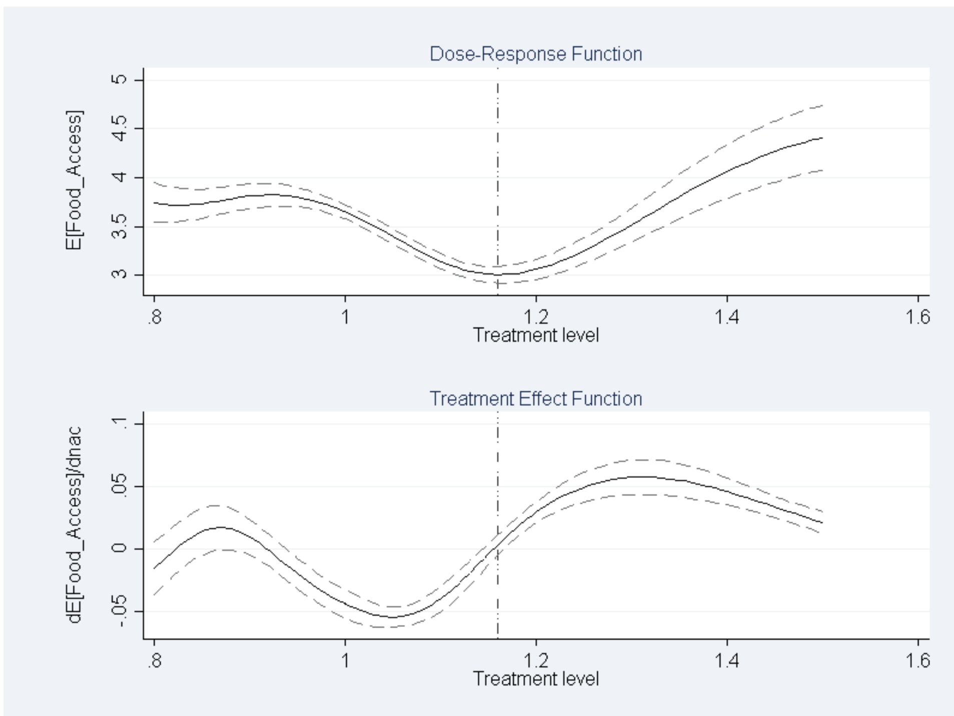
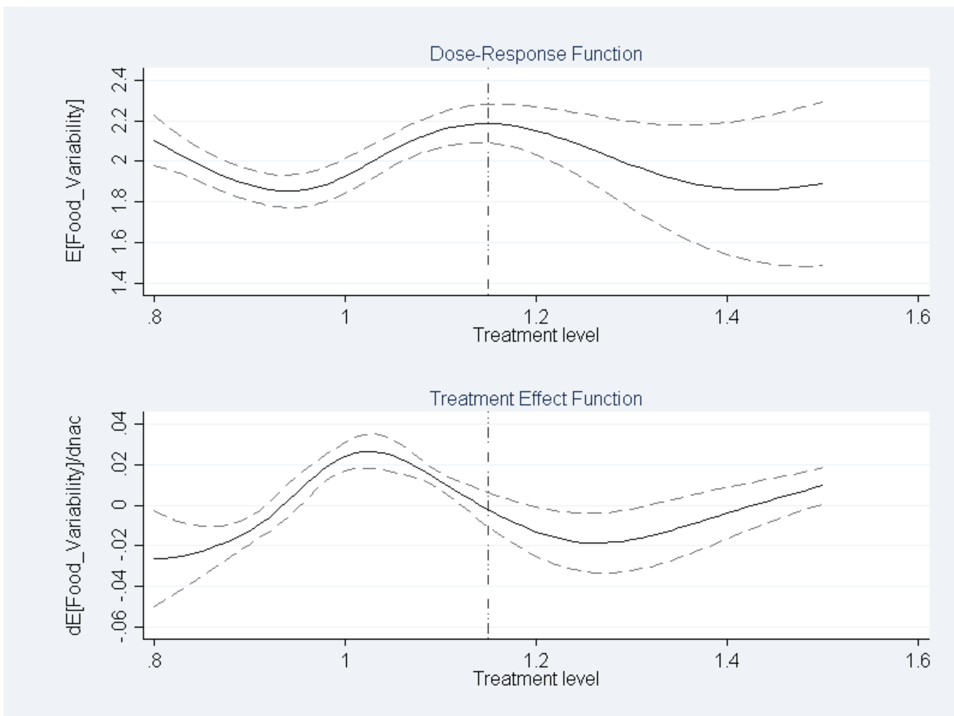


Figure 4: DRF and TEF: Food variability dimension



The corresponding results for the parsimonious, semi-parametric dose-response functions are summarized in Tables A.6; A.7; A.8; A.9 in the Appendix. To be noted is that also in this case R-squared is relatively high given the parsimonious specification and consistently with similar GPS empirical exercises (Becker *et al.*, 2012; Serrano-Domingo and Requena-Silvente, 2013; Magrini *et al.*, 2014).

The upper panels of Figs. 1 - 4 report the graphical representation of the point estimates of the DRF for the various dimensions of FS, i.e. the non-parametric functional form of the relationships between the FS dimensions and NAC, while the bottom panels of the same figures depict the Treatment Effect Function (TEF), i.e. the first derivative of the respective DRF. The corresponding standard errors and 90% confidence intervals of both functions are also reported in the figures (dots line) and were estimated via bootstrapping (see section 4).

In the case of the food availability dimension of FS, we may presume that the aim of policy intervention is to increase food supply. Indeed, according to the estimated DRF (see Figure 1, upper panel), higher levels of food supply (in kcal/capita/day) are associated with positive values of agicultural (dis)incentives (specifically NAC values ranging from 0.9 to 1.2). However, the estimated DRF shows that NAC values greater than 1.2 (equivalent to appreciable output subsidy and/or consumption tax) as well as a more severe export taxation (i.e., NAC values below 0.9) have lower impacts on expected FS (as showed by the negative values of the TEF, see Figure 1, bottom panel). In the other FS dimensions (utilization and access), the policy goals are associated with minimizing the response value. As far as food utilization is concerned, the depth of the food deficit is minimized for a NAC equal to 1.15 (see Figure 2, upper panel). This implies that some support has a positive impact, while the opposite holds in the case of (even moderate) taxation, as well as in the case of larger support rates, although in the latter case the confidence intervals become quite large. A similar pattern is registered in the case of food access, since the lowest value for the infant mortality rate corresponds to a treatment of NAC equal to 1.18, while the worst performances are registered with NAC equal to 0.92 or NACs higher than 1.38 (see Figure 3, upper panel).

To conclude, notwithstanding the adoption of a flexible non parametric relationship, which is different for each FS dimension, results appear to be consistent across the different dimensions examined so far. All the DRFs/TEFs show broadly similar patterns in the estimated aggregate relationship while the thresholds marking the changes of the relationship between treatment levels and expected FS are not significantly different.

A note of caution should be made in the case of the stability dimension. The specialized literature relates it to the risk component of the all FS dimensions (such as natural events, man-made shocks, malfunctioning international markets, etc.) (Pangaribowo *et al.*, 2013). Due to data constraint we proxied it by using only the variability of per capita food supply (i.e., the availability dimension). In this respect, our DRF shows that the lowest expected per capita food supply variability is associated with a moderate taxation (NAC equal to 0.95) or a very large support (but not too large, see the values of the TEF in the lower panel of (see Figure 4) though in the latter case the confidence intervals are huge. It is worth noting that treatment levels in the range associated with the best performances in the other FS dimensions lead in this case to the worst result.

Taking advantage of the consistency across the above empirical DRFs it can be instructive to cluster our sample of countries according to their past observed levels of NAC to derive some insights on their actual levels of agricultural insulating policy and the corresponding expected levels of FS, all other things being equal. To this end, Table 3 provides a synthetic view of the average frequency of NAC for our sample of countries and regional groups available in the WB data set for the investigated period (1990-2010). Specifically each column of Table 3 reports, for each country in the sample, the percentage values of the observed frequencies of NAC in the reported range (below 1; between 1 and 1.2 and above 1.2). These ranges have been determined by applying the thresholds estimated empirically by using our DRFs. As expected, among regional groups African countries show the highest frequency of NAC below 1 (i.e., they show overall an expected negative agricultural policy bias), while the developed ones show the highest frequency above 1.2 (i.e., they show overall an expected positive agricultural policy bias). Among the sampled developing and transition countries 26 show a NAC below 1 most of the time (Ivory Coast, Tanzania, Zimbabwe, Argentina, Nicaragua and Zambia virtually all the time). The prevailing regime of agricultural price distortions of this group of countries is likely to determine lower expected outcomes in terms of FS, ceteris paribus. Similarly, 10 developing and transition countries show a NAC higher than 1.2 most of the time (Morocco and South Korea all of the time). Also the prevailing policy regime of this group of countries is supposed to determine lower expected outcomes in terms of FS, ceteris paribus. It is worth noting that only the group of South American countries presents, on average, a prevailing levels of agricultural price distortions which is compatible with the highest expected levels of FS, all other things being equal, according to our estimated aggregate DRF. Moreover, in the investigated period, only 14 of developing and transition countries included in our sample (Chile, South Africa, Malaysia; Mozambique; Colombia; Sri Lanka; Poland; Uganda; Dominican Republic; Kazakhstan; Kenya; Mexico; Latvia and Estonia) actually present most of the time an observed level of agricultural price distortions which is compatible with higher expected FS according to our analysis. In this respect, they are supposed to have made the best choice in their agricultural insulating policy in terms of

FS.

Table 3: **NAC frequencies by countries and regional groups, percentage (1990-2010)**

country	Below 1	Btw 1-1.2	Above 1.2	country	Below 1	Btw 1-1.2	Above 1.2
Australia	0	100	0	Denmark	0	29	71
Chile	0	100	0	France	0	29	71
New Zealand	0	100	0	Italy	0	29	71
USA	0	100	0	Portugal	0	29	71
Brazil	19	81	0	Spain	0	29	71
South Africa	19	76	5	Turkey	0	29	71
Malaysia	25	75	0	Ecuador	68	26	5
Mozambique	25	75	0	Egypt	75	25	0
China	24	71	5	Madagascar	75	25	0
Colombia	5	70	25	Finland	0	24	76
Sri Lanka	30	70	0	Germany	0	24	76
Poland	11	63	26	Slovenia	0	21	79
Uganda	38	63	0	Burkina Faso	80	20	0
Dominican Rep.	35	60	5	Mali	80	20	0
Kazakhstan	40	60	0	Austria	0	19	81
Kenya	40	60	0	Ireland	0	19	81
Mexico	14	57	29	Netherlands	0	19	81
Latvia	11	56	33	Sweden	0	19	81
Estonia	16	53	32	UK	0	19	81
Canada	0	52	48	Vietnam	56	19	25
Bulgaria	53	47	0	Togo	85	15	0
Czech Rep.	0	47	53	Zambia	94	6	0
Hungary	0	47	53	Nicaragua	95	5	0
Bangladesh	55	45	0	Argentina	95	5	0
India	40	45	15	Sudan	75	5	20
Nigeria	40	45	15	Ivory Coast	100	0	0
Philippines	5	45	50	Ethiopia	85	0	15
Romania	0	42	58	Iceland	0	0	100
Russia	11	42	47	Japan	0	0	100
Indonesia	60	40	0	South Korea	0	0	100
Thailand	60	40	0	Morocco	0	0	100
Chad	63	38	0	Norway	0	0	100
Slovakia	0	38	63	Switzerland	0	0	100
Lithuania	16	37	47	Tanzania	100	0	0
Ukraine	63	37	0	Zimbabwe	100	0	0
Cameroon	65	35	0	Africa	65	28	7
Benin	69	31	0	Asia	38	44	18
Senegal	63	31	6	South America	40	52	8
Ghana	55	30	15	Transition Ctrs	14	43	42
Pakistan	70	30	0	Developed Ctrs	0	32	68
				Total	31	37	32

7. Conclusions

Both theory and casual observation suggest that agricultural incentives may have an impact on FS. However, given that agricultural policy is endogenous, a simple comparison of descriptive statistics has no causal interpretation. As a matter of fact, differences may not be the result of policy adoption, but it might be due to other factors such as differences in national endowments and observable and/or unobservable characteristics. Therefore, we need to conduct robust multivariate analysis to test the impact of policy stance on FS. To this end, we have used a non-parametric method for causal inference in quasi-experimental settings with continuous treatment under the (weak) unconfoundedness assumption, namely the GPS method recently developed for estimating the dose-response function and the marginal treatment effects in a continuous treatment framework. This empirical strategy is adopted to verify whether countries following different policy strategies actually differ in their FS status. Our results, then, account for heterogeneities in the average and marginal treatment effects stemming from variations in the intensity of policies.

Our analysis shows the likely presence of a self-selection bias in the causal relationship between agricultural policies and FS, cross-country and by product. Moreover, we show empirical evidence of a significant impact of agricultural trade policy distortions on various dimensions of food security . Results indicate that agricultural incentives do matter and their impact on FS varies with the level of intensity. Indeed, countries supporting the primary sector tend to be better off in most dimensions of FS (food availability, access, and utilization) while taxation of the primary sector tends to make the situation worse. The policy implications may be reversed if we consider the variability dimension but, more generally, it is worth recalling that our analysis does not allow normative implications to be drawn, since we have not considered the costs of the treatment. However, the evidence of different impacts in terms of FS is certainly important, and it justifies further analysis of the actual policy mixes adopted in correspondence to different treatment levels. The literature to date leaves many open questions regarding the type of interventions that might be most effective to increase FS. This is due in part to the multitude of approaches to measurement of FS, and in part due to methodological concerns that limit causal inference in many of the existing studies. Likely the optimal policy will also be strongly context-specific, and understanding the sensitivity of impacts to contextual changes hence is equally important. Many of the existing studies also point to the key role of targeting, an aspect of the policy design that might be particularly important to understand for FS outcomes. Future work addressing these questions will likely be particularly useful if it also allows shedding light on the channels and mechanisms through which programs can increase FS and the heterogeneity of such impacts across households.

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Appendix A. Tables & Figures

Table A.1: **List of sampled countries and summary statistics for the NRA**

Country	mean	sd	min	max	Country	mean	sd	min	max
Argentina	-0.116	0.099	-0.236	0.004	Lithuania	0.199	0.216	-0.198	0.653
Australia	0.024	0.016	0.005	0.064	Madagascar	-0.043	0.047	-0.127	0.063
Austria	0.420	0.228	0.066	0.821	Malaysia	-0.008	0.047	-0.134	0.037
Bangladesh	-0.020	0.070	-0.154	0.138	Mali	-0.020	0.028	-0.099	0.016
Benin	-0.013	0.019	-0.069	0.005	Mexico	0.140	0.130	-0.151	0.413
Bulgaria	-0.016	0.103	-0.232	0.183	Morocco	0.516	0.087	0.328	0.667
Burkinafaso	-0.026	0.054	-0.199	0.021	Mozambique	0.027	0.037	-0.050	0.090
Cameroon	-0.004	0.016	-0.03	0.049	Newzealand	0.023	0.014	0.004	0.064
Chad	-0.006	0.012	-0.038	0.012	Nicaragua	-0.089	0.079	-0.229	0.052
Chile	0.059	0.035	0.004	0.102	Nigeria	0.065	0.180	-0.087	0.722
Colombia	0.162	0.090	-0.036	0.341	Norway	0.977	0.243	0.613	1.240
Czech Rep.	0.212	0.118	0.066	0.484	Pakistan	-0.027	0.076	-0.216	0.123
Denmark	0.340	0.181	0.063	0.697	Philippines	0.202	0.129	-0.059	0.411
Dominican Rep.	0.036	0.132	-0.203	0.281	Poland	0.175	0.139	-0.017	0.596
Ecuador	-0.043	0.125	-0.212	0.219	Portugal	0.264	0.110	0.082	0.438
Egypt	-0.036	0.082	-0.202	0.104	Romania	0.331	0.249	0.036	0.798
Estonia	0.151	0.154	-0.196	0.488	Russia	0.176	0.134	-0.197	0.419
Ethiopia	-0.081	0.275	-0.226	0.892	Senegal	-0.015	0.115	-0.172	0.226
Finland	0.479	0.377	0.068	1.260	Slovakia	0.246	0.123	0.066	0.426
South Korea	0.924	0.285	0.483	1.244	Slovenia	0.564	0.286	0.092	1.056
Sri Lanka	0.041	0.114	-0.221	0.192	South Africa	0.051	0.070	-0.067	0.213
Ghana	0.045	0.136	-0.064	0.468	Sudan	0.107	0.316	-0.209	0.826
Hungary	0.205	0.121	0.065	0.446	Sweden	0.460	0.298	0.066	1.128
Iceland	0.992	0.287	0.597	1.228	Switzerland	0.676	0.196	0.469	0.976
India	0.055	0.124	-0.128	0.260	Tanzania	-0.112	0.058	-0.174	0.000
Indonesia	-0.014	0.106	-0.218	0.138	Thailand	-0.004	0.061	-0.093	0.149
Ireland	0.572	0.263	0.078	1.051	Togo	-0.015	0.019	-0.072	0.003
Ivory Coast	-0.197	0.019	-0.233	-0.169	Turkey	0.247	0.113	0.013	0.432
Kazakhstan	0.009	0.097	-0.146	0.100	Uganda	0.001	0.007	-0.022	0.009
Kenya	0.029	0.063	-0.078	0.158	Vietnam	0.045	0.177	-0.231	0.322
Ukraine	-0.027	0.093	-0.192	0.135	Zambia	-0.079	0.128	-0.224	0.134
Latvia	0.217	0.168	0.032	0.541	Zimbabwe	-0.191	0.042	-0.227	-0.142
					Total	0.126	0.258	-0.236	1.260

Table A.2: **Variables and Data**

Type	**Variable** (annual data)	**Source**
Agricultural incentives (treatment)	Aggregate Nominal Rates of Assistance (NRAag): Value of production-weighted average NRA all (primary) Agriculture,	
	total for covered and non-covered and non-product-specific assistance.	World Bank dataset (Anderson and Nelgen, 2012,
		"Updated National and Global Estimates of Distortions to Agricultural Incentives, 1955 to 2010")
Observable characteristics (covariates)	Real per capita GDP (2005 International dollar per person).	Penn World Table (Heston, Summers and Aten, 2012, "Penn World Table Version 7.1",
		Center for International Comparisons of Production, Income and Prices at the University of Pennsylvania, November)
	Population (in thousands).	
	Arable land (hectares per person).	World Bank - World Development Indicators
	Food production index (it covers food crops that are considered edible and that contain nutrients).	
	(Country) Food imports over total exports.	FAOSTAT; IMF DOTS
	Deviation of international food prices from trend (positive and negative, %).	World Bank - GEM Commodity Price Data
	International food price volatility.	
	(Country) Net food exporter dummy.	FAOSTAT
	Regional group dummy: African Developing Countries (Group 1); Asian Developing Countries (Group 2);	
	Latin American Developing Countries (Group 3); European Transition Economies (Group4); High-income Countries (Group 5).	World Bank data set (Anderson and Nelgen, 2012
	Food crisis dummy (1 if year 2007 and 2008).	Authors calculation
Food Security dimensions (outcome):		
Availability	Food supply (in kcal/capita/day).	FAO - Food Balance Sheets
Access	Depth of food deficit (kilocalories per person per day).	World Bank - World Development Indicators
Utilization	Mortality rate, infant (per 1,000 live births).	World Bank - World Development Indicators
Stability	Per capita food supply variability.	FAO

Table A.3: **Summary statistics of covariates and outcomes**

Variable	**Mean**	**Std. dev.**	**Min**	**Max**	**Observations**
Real per-capita GDP	9714.136	11148.370	323.260	51791.630	1072
Population	52636.59	138142.9	269	1156898	1072
Per-capita arable land	0.328	0.348	0.030	2.807	1072
Food production index	89.646	15.895	35.020	148.220	1072
Food import/total exports	0.016	0.024	0.001	0.260	1070
Pos. deviation of int.l food prices	0.010	0.030	0.000	0.142	1072
Neg. deviation of int.l food prices	0.050	0.041	0.000	0.138	1072
Food price volatility	0.021	0.009	0.006	0.050	1072
Net food exporter	0.484	0.500	0.000	1.000	1072
Group 1 - African DCs	0.332	0.471	0.000	1.000	1072
Group 2 -Asian DCs	0.170	0.376	0.000	1.000	1072
Group 3 - Latin American DCs	0.119	0.324	0.000	1.000	1072
Group 4 - European Transition Economies	0.205	0.404	0.000	1.000	1072
Group 5 - High-income Countries	0.174	0.379	0.000	1.000	1072
Food crisis	0.090	0.287	0.000	1.000	1072
Food supply	2725.312	524.468	1557.000	3826.000	1044
Infant mortality	40.190	36.564	1.900	155.100	1072
Depth of food deficit	97.958	110.173	1.000	615.000	985
Food variability	12.671	13.378	0.509	81.396	1045

Table A.4:

Differences in the treatment levels before and after balancing on the GPS: t-stats for equality of means.

	Prior to balancing on the GPS				After balancing on the GPS			
Covariates	**Group 1**	**Group 2**	**Group 3**	**Group 4**	**Group 1**	**Group 2**	**Group 3**	**Group 4**
L.lnreal pc gdp	**13.921**	**8.091**	**5.536**	**17.173**	1.957	1.120	1.373	0.506
L.lnreal pc gdp$\hat{2}$	**13.993**	**7.770**	**5.064**	**17.485**	1.906	0.999	1.338	0.385
L.lnreal pc gdp$\hat{3}$	**13.984**	**7.405**	**4.570**	**17.681**	1.852	0.867	1.293	0.265
L.lnreal pc gdp$\hat{4}$								
L.ln pc arable land	**2.357**	**3.953**	**2.347**	0.768	1.226	1.532	1.873	0.872
L.pos dev food prices	0.879	1.184	**3.357**	1.279	0.463	0.313	0.996	0.095
L.neg dev food prices	0.743	0.370	**1.979**	0.863	0.434	0.143	0.300	0.515
food price volatility	1.082	0.483	**2.295**	0.725	0.385	0.166	0.621	0.928
food crisis	0.307	1.045	**3.146**	1.784	0.130	0.405	0.613	1.542
L.food prod. index	**6.334**	**6.356**	**4.523**	**8.227**	0.150	1.115	1.371	1.008
group 2 -Asian DCs	**3.301**	0.094	0.657	**4.067**	0.555	0.154	0.631	0.019
group 3 - Latin American DCs	**2.397**	**2.836**	**5.058**	**4.609**	0.007	1.361	1.693	0.084
group 4 - European Transition Economies	**6.033**	**7.520**	**6.401**	**7.144**	0.246	**2.382**	0.400	0.379
group 5 - High-income Countries	**8.973**	1.210	**2.708**	**13.604**	0.988	**2.010**	0.530	0.479
L.net food exporter	**3.905**	**2.048**	0.352	**6.393**	0.673	0.405	0.958	0.159
L.food import/total exports	**5.381**	**6.281**	**4.645**	**7.036**	1.257	1.253	1.001	0.768
L.food import/total exports$\hat{2}$	**2.194**	**4.570**	**2.870**	**3.883**	1.075	0.969	0.724	0.684
L.pop	**6.572**	1.203	1.924	**7.340**	1.406	**1.972**	0.696	1.088
L.pop$\hat{2}$	**6.333**	1.587	**2.046**	**6.819**	1.395	1.957	0.696	0.984
No. of observations	268	268	268	268	243	246	217	114
Mean t-value		4.848				0.876		

Note: t-values reported in bold face indicate null rejections at the 5% level significance.

Table A.5: **The final group-strata structure**

Strata	Control1	Group1	Control2	Group2	Control3	Group3	Control4	Group4
1	200	31	88	31	218	28	410	15
2	134	30	75	31	96	27	77	14
3	52	31	71	31	42	27	28	14
4	35	30	70	30	44	27	72	14
5	41	30	60	31	54	27	28	15
6	43	31	63	31	67	27	38	14
7	46	30	84	31	31	27	18	14
8	26	30	63	30	51	27	34	14

Table A.6: **DRF estimation for food availability**

Food availabity	Coef.	SE (robust)
NAC	5.620***	1.889
NAC^2	-4.640***	1.499
NAC^3	1.220***	0.384
GPS	-0.354***	0.070
GPS^2	-0.071**	0.040
GPS^3	0.014**	0.007
NAC*GPS	0.404***	0.034
cons	5.704***	0.755
No. of observations	806	
R^2	0.3062	

Note: $(NAC) = (1 + NRA)$
***,**,* denote significance at the 1, 5 and 10 per cent level, respectively.

Table A.7: **DRF estimation for food access**

Food Access	Coef.	SE (robust)
NAC	-42.914***	10.376
NAC^2	35.971***	8.395
NAC^3	-9.460***	2.191
GPS	1.468***	0.347
GPS^2	0.421**	0.195
GPS^3	-0.061**	0.034
NAC*GPS	-2.104***	0.161
cons	19.858***	4.085
No. of observations	820	
R^2	0.2156	

Note: $(NAC) = (1 + NRA)$
***,**,* denote significance at the 1, 5 and 10 per cent level, respectively.

Table A.8: **DRF estimation for food utilization**

Food utilization	Coef.	SE (robust)
NAC	-78.439***	23.487
NAC^2	70.240***	19.509
NAC^3	-19.813***	5.251
GPS	3.236***	0.626
GPS^2	0.583*	0.341
GPS^3	-0.104*	0.058
NAC*GPS	-3.893***	0.278
cons	31.989***	9.000
No. of observations	749	
R^2	0.2659	

Note: $(NAC) = (1 + NRA)$
***,**,* denote significance at the 1, 5 and 10 per cent level, respectively.

Table A.9: **DRF estimation for food variability**

Food Variability	Coef.	SE (robust)
NAC	-6.118***	1.843
NAC^2	2.484***	0.706
NAC^3		
GPS	-1.127***	0.187
GPS^2		0.196
GPS^3		
NAC*GPS	1.115***	1.078
cons	5.582***	
No. of observations	808	
$R\hat{2}$	0.0847	

Note: $(NAC) = (1 + NRA)$
***,**,* denote significance at the 1, 5 and 10 per cent level, respectively.

Figure A.1: **Common support before and after GPS: group 1**

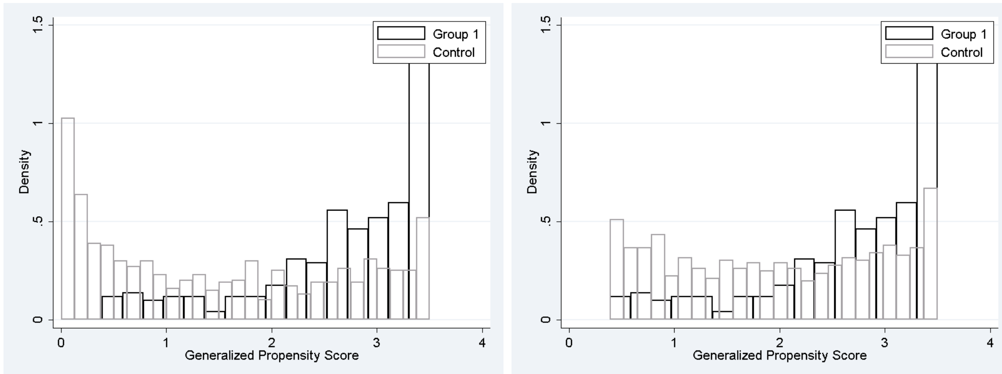


Figure A.2: **Common support before and after GPS: group 2**

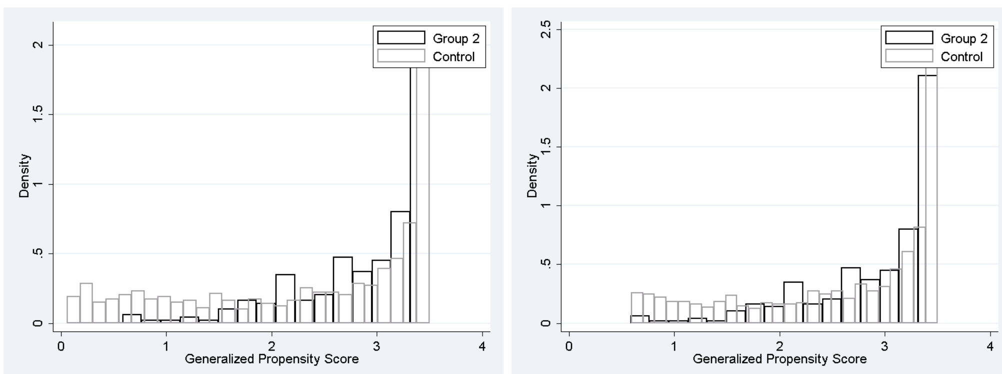


Figure A.3: **Common support before and after GPS: group 3**

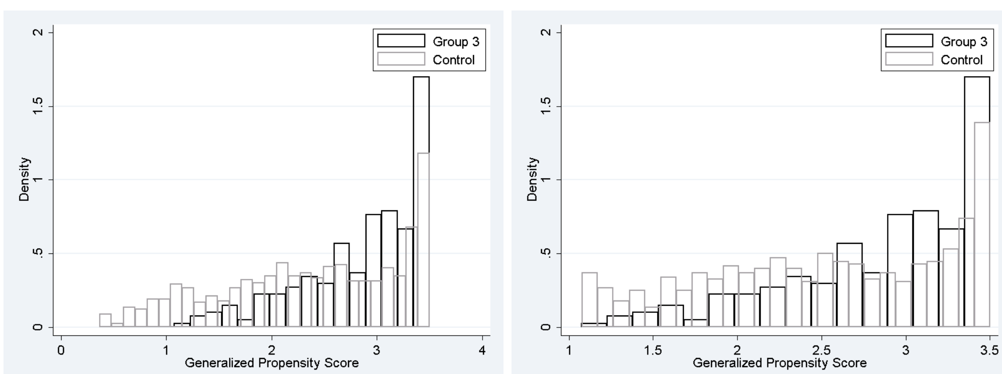
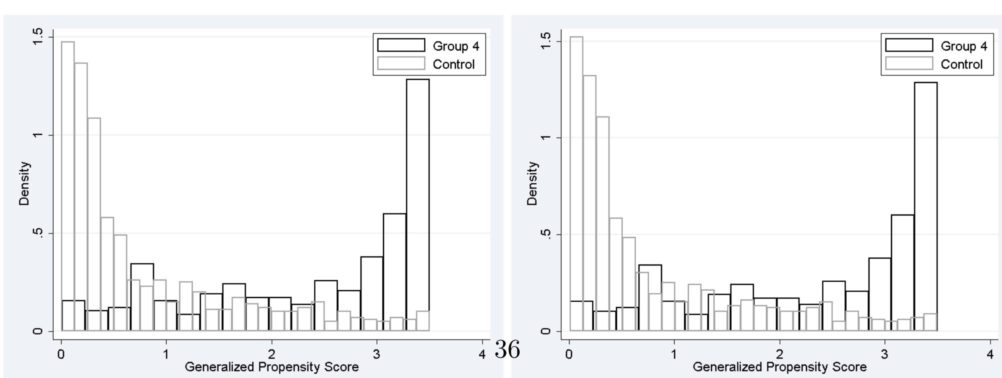
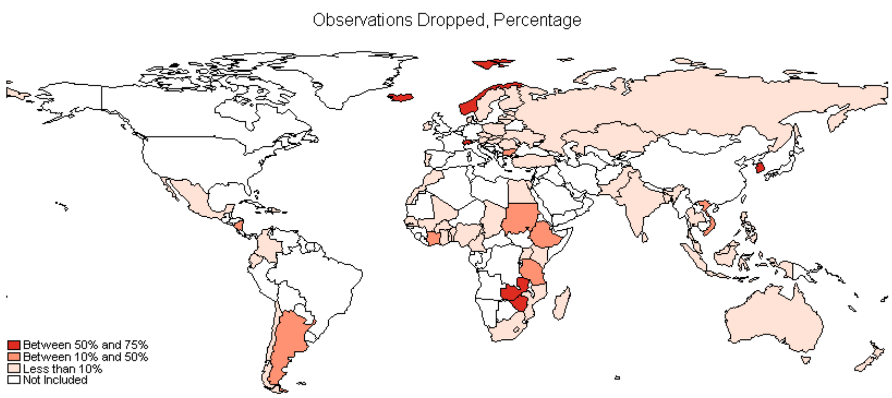


Figure A.4: **Common support before and after GPS: group 4**



Source: Authors' calculations

Figure A.5: **Map of countries (% of observations excluded by the common support)**



Source: Authors' calculations