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COULD THE BEHAVIORAL RESPONSES JUSTIFY THE ABSENCE OF DIRECT TRANSFERS TO FIGHT POVERTY IN MENA REGION?

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Abstract

While cash transfers through proxy means tests have increased over the last decade in Latin America to fight poverty, consumption subsidies remain the common form of redistributive income in most MENA countries. There may be many reasons for this such as their administrative costs, effects on labor supply and private transfers, and the absence of political support for more targeted programs. The literature offers several *ex post* approaches to capture the behavioral responses cost of implemented socio-demographic targeting transfers. In this paper, we suggest an *ex ante* approach to judge whether the behavioral incidence of counterfactual socio-demographic targeting transfers may explain the absence of this policy intervention. Our methodology is illustrated using a household survey from Tunisia and counterfactual direct transfers based upon socio-demographic targeting. The results suggest that poverty could be decreased robustly by adopting the counterfactual design even if it leads to stronger behavioral responses when compared to the consumption subsidy program. However, more targeted transfers will not automatically meet with the approval of the majority of citizens.

ملخص

في الوقت الذي زادت فيه التحويلات النقدية من خلال البحوث الاجتماعية بالتوكيل خلال العقد الماضي في أمريكا اللاتينية من اجل محاربة الفقر, ظل دعم الاستهلاك هو الشكل الشائع للدخل المعاد توزيعه في معظم دول الشرق الأوسط وشمال إفريقيا. وربما يعود ذلك إلى عدة أسباب مثل تكلفتها الإدارية وتأثيراتها علي عرض العمل والتحويلات الخاصة وغياب الدعم السياسي للكثير من البرامج المستهدفة. وتعرض الكتابات المتعلقة بهذا الموضوع أساليب استرجاعية لاستعراض تكلفة الاستجابات السلوكية للتحويلات التي تستهدف النواحي الاجتماعية والسكانية المطبقة حاليا. ونقترح في هذه الورقة أسلوبا استشرافيا لنقرر هل يمكننا إرجاع حدوث تحويلات افتراضية تستهدف النواحي الاجتماعية والسكانية إلى عدم تطبيق هذه السياسة. ومنهجنا في البحث يستخدم مسحا اسريا من تونس وتحويلات مباشرة مفترضة مبنية علي عملية استهداف اجتماعي وسكاني. وتوحي النتائج بانه يمكن الحد من الفقر بصورة كبيرة عن طريق اتباع تصميم افتراضي حتى ولو أدى ذلك إلى استجابات سلوكية اقوي عند المقارنة ببرنامج دعم الاستهلاك. ومع ذلك فان المزيد من التحويلات المستهدفة لن تلقي موافقة غالبية المواطنين بصورة تلقائية

1. Introduction

Attempts to target public transfers efficiently are typically constrained by the lack of information. To choose recipients and non-recipients of public support, policymakers are thus forced to select among imperfect targeting schemes such as foodstuff subsidization, socio-demographic characteristics, land ownership and access to public services (health, education, etc.).

Consumption subsidies (CS) are, nevertheless, a common form of indirect transfers particularly in the Middle East and North Africa (MENA) region. The World Bank (1999) describes the food subsidy programs of a number of MENA countries in which CS ranged from 1% to 5% of GDP in 1995. Further, 13 out of 14 MENA antipoverty interventions consist of food subsidies.[1] For instance, partly in order to offset some of the adverse effects of falls in tariffs and custom duties, Egypt has recently expanded its CS program to include rice, pasta, tea, fava beans, margarine and lentils, thereby almost doubling Egypt's total subsidy bill.[2] As initial CS often grow with population and political pressure, their budgetary and inefficiency cost can become hard to sustain, and numerous calls for reforms have indeed been heard for many years in many contexts – see, for instance, World Bank, 1991 (Algeria), Adams, 2000 (Egypt), Laraki, 1989 (Morocco), and Bibi and Duclos, 2007a (Tunisia).

CS can be viewed as a form of self-selection since they will benefit households only if households choose to consume those commodities that are subsidized. Their targeting has, however, long been criticized as bad. For instance, the benefits of Egypt's CS program are uniformly distributed across all income levels (Coady et al., 2004), representing "an unplanned form of neutral targeting" (Fan et al., 2005 p.17). World Bank (1999) reports that with inadequate targeting, higher income groups benefit more in absolute terms than the poor because the rich tend to consume greater quantities of subsidized goods. In Morocco, for example, those in the top quintile consume twice the value of subsidized foods as do those in the lowest quintile. In Yemen, the top decile spends ten times more than the lowest on subsidized wheat and flour. On average, 60 to 80 percent of public CS expenditures in MENA go to higher income groups.

The efficiency costs of CS can also be quite significant. They manifest themselves mainly as over-consumption and waste of resources as well as a significant burden on public expenditures, imposing heavy financing requirements that affect both the tax structure and the overall macro-economy. This can deter growth, crowd out other forms of public expenditures and cause higher inflation, significant current account deficits and economic instability – all of which are often detrimental to the poor. To provide an order of magnitude, the fiscal cost of food subsidies is 150% the size of Morocco's health budget and four times Yemen's public health budget (World Bank, 1999).

The costs of other forms of transfers can nevertheless be just as daunting. The feasibility of one major alternative, which consists of directly targeting transfers, depends on the existence of reliable income tax records in the target population and requires considerable administrative capacity to process and update detailed accurate information on households' income. Such information is often unavailable in developing countries characterized by a large informal sector and inefficient system of direct taxation. Thus, it is not easy to directly identify the poor, although such identification is required to make the fight against poverty administratively feasible and cost-effective (Besley and Kanbur, 1993).

[1] See Coady, Grosh and Hoddinott (2004).

[2] See Oteify (2004) and Fan et al. (2005).

Given the administrative difficulties associated with a direct observation of the true households' welfare, income or need, the idea of using other households' characteristics (namely geographic and socio-demographic characteristics) as proxies for income to target direct transfers to the poor could be appealing. While socio-demographic targeting transfers (SDTT) have proliferated in several Latin America countries (Grosh and Baker, 1995 and Soares et al., 2007), they have not yet found a notable success in the MENA region. In reality, attempts to move from indirect to direct transfer programs seem to be constrained by serious political economy considerations. Over the years, there has been systematic, widespread and significant resistance against the abolition or even the reduction of the importance of CS. Explanations for this could be due to:

1. the administrative cost which could be higher for the implementation of SDTT;
2. the fact that CS could be more horizontally equitable than SDTT;[3]
3. the presence of strong resistance on the part of interest groups and potential losers from the removal of CS;
4. the fact that political support for SDTT can be weak due to their much narrower range of beneficiaries. Because the pool of CS beneficiaries is so large (for instance, about 80% of the population in Egypt, Jordan and Tunisia), eliminating CS would indeed imply welfare losses for a large portion of the population;
5. and the fact that the ways in which participants and other agents respond to a program based on proxy means tests can matter greatly to its distributional outcomes. For instance, recipients of SDTT program may change their labor supply or receive less private transfers,[4] such that the net income gain is less than the amount of the socio-demographic targeting transfer.

Some of these reasons have been already studied while some others continue to be important issues for future research. For instance, relying on the Latin American countries experiences, the first possibly reason is not justified according to Grosh (1994), Grosh and Baker (1995), Tabor (2002), and Soares et al. (2007).[5] The second reason was extensively studied by Bibi and Duclos (2007b). They found that although horizontal inequity would be more important under a counterfactual SDTT than under targeting by CS, the expected net benefit at each income level for the less well-off of the population would be higher under the counterfactual scheme. This suggests that the horizontally equitable CS could not be deemed preferable to a system based on SDTT that would nevertheless involve greater vertical equality and lower leakages to the non-poor.

The aim of this paper is to tackle mainly the fifth point, that is, the behavioral response issue, and, to a lesser extent, the problem of political support for SDTT programs. As it is well known, the ways in which targeted households respond to an anti-poverty program can matter greatly to its distributional outcome. For example, recipients of a direct transfer may reduce

[3] The principle of horizontal equity stipulates that equal individuals (in terms of welfare, income or need) be treated alike by the social policy while the vertical equality principle demands to differentiate appropriately between the unequals. See Bibi and Duclos (2007b) for more on this.

[4] Private transfers generally include inter-households transfers (remittances, within-family transfers, etc.), transfers through religious groups, rotating fund societies, community associations, etc. Since private transfers made through channels other than households and family may be subjected to the same targeting errors of public transfers, we exclude them from the set of private transfers.

[5] According to Grosh (1994), once the administrative system is in place, the cost of SDTT is likely to be lower than that of an equivalent CS program. Thus, we assume that the administrative costs of implementing SDTT scheme are at most equal to those required to manage the CS program.

their labor supply, such that the mean income gain for the population is less than the *per capita* cost of the social program for the government.

The principal approaches followed in the literature to incorporate behavioral responses tend to be *ex post* in that they study social policies that are already in force.[6] In this paper, we develop an alternative *ex ante* approach to incorporate behavioral responses which would occur following an alternative social policy design.[7] This will enable us to judge whether seriously taking into account the behavioral incidence of SDTT could justify the absence of SDTT and the preference of CS as the principal instrument to fight poverty in the MENA region.

The rest of the paper is structured as follows. Section 2 presents the measurement techniques. Section 3 applies the methodology to the 1990 Tunisian Household Survey. Section 4 offers some concluding remarks.

2. The Theoretical Framework

The way a social policy impacts on households is important for an understanding of how this policy affects their welfare. The living standards change experienced by each household can then be aggregated to infer poverty change at the macro level both in the *short run* and in the *long run*. Further, anti-poverty comparisons need to be normalized from the arbitrariness inherent in the choice of the poverty line and poverty index. We consider each of these issues in turn.

2.1. The Individual Impact of a Social Policy

Let a given change of social program be defined as a mapping from an original position, where each household has a per capita income y_h^o and faces a vector of prices p^o, to a post-reform position characterized by (y_h^p, p^p). Thus, comparing the outcomes of CS program and SDTT scheme requires the specification of an indicator of individual well-being that is sensitive to price variations. A useful formulation is King's (1983) equivalent income function, $y_e(p^o, p, y)$, which is defined implicitly by:

$$v(y_e(p^o, p, y), p^o) \equiv v(y, p) \tag{1}$$

where $v(.)$ is the consumer's indirect utility function, p^o is a vector of reference prices that we suppose would prevail in the absence of any anti-poverty program, and y_e is the income level which yields the same utility level under p^o as y provides under p. Notice that y_e is an exact monetary metric of actual utility since it is an increasing monotonic transformation of $v(y, p)$. y_e can also be usefully interpreted as a real income function defined in reference to the prices p^o. Inverting (1) yields $y_e(p^o, p, y)$.[8]

We assume that before implementing the CS program, each household h has an exogenous income y_h^o and, then, faces the price system p^o. With the implementation of the CS program, each household has the same *nominal* income, y_h^o, but faces the price system p^s. CS program is then equivalent to each household to an equivalent gain per capita, Γ_h, equal to

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[6] See, among many others, Ravallion et al. (1995), Jalan and Ravallion (2003), and van de Walle (2003). The focus on *ex ante* evaluations of Todd and Wolpin (2006) and Bourguignon and Ferreira (2003) is an exception to this. However, their approach also requires micro-data on labor supply and these datasets are scarcely available in MENA region.

[7] In line with Todd and Wolpin (2006), *ex ante* evaluation of social changes may help to avoid the high costs of implementing alternative programs that are later found to be ineffective. It can also yield an idea of what range of poverty reduction to expect after the implementation of the change.

[8] It is clear from (1) that $y_e(p^o, p^o, y) = y$.

$$\Gamma_h = y_e(p^o, p^s, y_h^o) - y_e(p^o, p^o, y_h^o) \quad (2)$$

Normally, the average equivalent gain from CS is equal to the per capita social program costs for the government net of the deadweight loss and the cost of the behavioral responses. However, we assume that the cross elasticity between food commodities and leisure are very weak so that the cost of the behavioral responses is negligible. Further, to focus on the behavioral incidence of socio-demographic targeting, we ignore the cost of the deadweight loss. This makes the average equivalent gain from CS equal to the per capita social program costs for the government. This procedure then overestimates the distributional effects of CS program: it thus underestimates the superiority of SDTT scheme whenever they are more effective in reducing poverty.

It would thus seem instructive to compare the outcome of the CS program with that of an alternative one based, for instance, on SDTT. For this, we will use an illustrative SDTT scheme that involves household proxy-means tests subject to the same aggregate budget as that for CS program. The alternative program first estimates a pertinent model to predict the households' consumption using easily observable variables, like the region of residence and the demographic structure of the households.[9] The simulated program then assigns to the *predicted* poorest person a transfer until his income equals the next *predicted* poorest person, then transfer is attributed to these two *predicted* equal poor persons equally until each person's income equal the third predicted poorest person's income. This pattern is repeated until finishing the total available budget currently devoted to CS program. This procedure yields a detailed schedule of transfers that depend on observable socio-demographic characteristics. Whenever the participants of this counterfactual program do not modify their behavior, in terms of labor supply for instance, the *short run* effect they experience would be:[10]

$$\hat{T}(x_h) = y_e(p^o, p^o, y_h^o + \hat{T}(x_h)) - y_e(p^o, p^o, y_h^o) \quad (3)$$

where x_h is a vector of socio-demographic characteristics of the household h that are directly and cheaply observable.

The net impact of substituting a SDTT scheme to CS program on each individual in the *short run* is then

$$\Lambda_h^{S.R} = \hat{T}(x_h) - \Gamma_h \quad (4)$$

where $\Lambda_h^{S.R}$ could be either positive or negative according to whether the household h would gain or lose following the reform. Whenever more than 50 percent of the population will experience a positive $\Lambda_h^{S.R}$, such a reform could be politically sustainable. In the alternative case, and mainly when the loss of the losers as a share of their income is important, such a reform could be hardly advocated by the policymakers even if it would enable more poverty reduction than CS program.

[9] The estimation method could be based on Tobit model, quantile regression model, or ordinary least square (OLS) regressions. Bibi (2003) and Bibi and Muller (2006) have shown that OLS regressions are not appropriate for poverty analysis.

[10] Generally, the behavioral responses have been ignored by the authors who have studied the likely effects of SDTT on inequality and poverty. See, among many others, Grosh and Baker (1995), Bibi (2003), and Bibi and Muller (2006). Such an assumption may hold in the *short run*. However, it is hard to advocate it in the *long run* both in theory [see, among many others, Besley and Kanbur (1993)] and practice [see, for instance, van de Walle (2003)].

2.2. The Impact of the Reform on Poverty in the Short Run

To describe how poverty is affected by changes in the anti-poverty program, we must also obviously address the measurement of poverty. Sen's (1976) influential work has generated a considerable literature on this.[11] We start with the popular Foster-Greer-Thorbecke (1984) (FGT) family of poverty indices, because the use of these peculiar indices is also useful for predicting how many other indices will react to social policy changes. Let z be a real poverty line, that is, a line measured in terms of the reference prices, p^o.[12] The FGT family is then defined as

$$P_\alpha(z, y_e) = \int_0^{+\infty} \left(\frac{z - y_e}{z} \right)_+^\alpha dF(y_e), \tag{5}$$

where $x_+ = \max(0, x)$ and where $F(y_e)$ is the distribution of real or equivalent income, y_e. The parameter α captures the "aversion to poverty" or the distribution sensitivity of the poverty index.[13] As is well known, $P_0(z)$ is the poverty headcount (the "incidence" of poverty), $P_1(z)$ is the normalized average poverty gap measure (the "intensity" of poverty), and $P_2(z)$ is often described as an index of the "severity" of poverty – it weights poverty gaps by poverty gaps.

For $\alpha > 1$, $P_\alpha(z)$ is sensitive to the distribution of living standards among the poor, and when α becomes very large, $P_\alpha(z)$ approaches a Rawlsian measure of social welfare.[14]

We assume that behavioral responses hold only in the *long run*. Thus, a natural measure of its social impact of the policy change in the *short run* could be given by the decline of a pre-specified poverty index:

$$\Delta P_\alpha^{S.R}(z) = P_\alpha\left(z, y^o + \Gamma\right) - P_\alpha\left(z, y^o + \hat{T}(x)\right). \tag{6}$$

Whenever $\Delta P_\alpha^{S.R}(z) > 0$, the SDTT scheme will necessarily be judged to reduce poverty by more than CS policy for the selected poverty line, z, and for the specified value of the parameter α.

2.3. The Behavioral Incidence of the Alternative Program

The key issue for all incidence analysis is how to define the counter-factual of what the pertinent welfare indicator of households will be in the absence of the social program. Studies of the incidence of social programs that are in force typically subtract the total amount of the (equivalent or cash) transfers from household income to approximate pre-intervention policy. As argued by van de Walle (2003), such an assumption ignores the replacement income households would have had through their behavioral responses had they not benefited from the social program. Indeed, experiences from industrialized economies show that cash transfers programs favor consumption of leisure instead of labor, stimulate shirking behavior, discourage job searchers and introduce rigidities into the labor market (Kanbur, Keen, and Tuomala, 1995). Further, in most developing countries, low-income households may derive an important share of their income from private transfers. Implementing SDTT could then

[11] For comprehensive surveys of the literature on the axiomatic foundations and the design of poverty indices, see, for instance, Zheng (1997, 2000).

[12] In terms of (1), if v_z is the minimal level of utility required to live a decent live, then $v(z, p^o) \equiv v_z$.

[13] See Zheng (2000) for a more elaborate discussion of this.

[14] See Rawls (1971).

reduce labor supply and transfers made between households so that their effective impact on poverty will be less important than their predicted impact.[15]

To capture the cost of these behavioral responses, one may need panel household surveys including pertinent information and covering periods before and after the implementation of the social program. Using a panel of households from Vietnam, van de Walle (2003) estimates the propensity to consume out of an anti-poverty program (awarding cash transfers). Her principal findings show that the replacement income approximates 50 percent of the program cost and ranges between 30 and 70 percent of the households benefit.[16]

As a result, the net impact of substituting a socio-demographic targeting to CS program on each individual in the *long run* should then be given by

$$\Lambda_h^{lr} = (1-\lambda)\hat{T}(x_h) - \Gamma_h \tag{7}$$

when λ integrates the behavioral responses of participants to SDTT through the reduction of labor supply and inter-households transfers.[17]

Clearly however, it is not possible to estimate the value of λ for a counterfactual transfer scheme. Further, even for social policies in force, panel data covering periods before and after the implementation of the social program and including pertinent information on the households' labor supply, the public transfers and inter-households transfers are scarcely available, especially in the MENA countries. Thus, we suggest an *ex ante* approach to numerically infer the maximum value of λ policymakers could tolerate in the *long run* to be indifferent between the SDTT scheme and the CS program:

$$\Delta P_\alpha^{L.R}(z) = P_\alpha\left(z, y^o + \Gamma\right) - P_\alpha\left(z, y^o + (1-\lambda_{\max})\hat{T}(x)\right) = 0 \tag{8}$$

If the calibrated value of $\lambda_{\max}$ is not greater to all the estimate values found in the literature mentioned above, then the preference toward the CS program in the MENA region will appear to be consistently justified. However, in the opposite case, the hesitation of the MENA policymakers to move toward a more targeted program will seem to be unsubstantiated.

2.4. Robustness Analysis

The policy implications of the above methodology can potentially depend arbitrarily upon the choice of a poverty line, *z*. and of a poverty index. Since both of these choices are somewhat arbitrary, so will be the policy implications that will be identified using them. However, the application of well-known results from the stochastic dominance literature shows that if $\Delta P_\alpha^{S.R}(z) > 0$ for a range of poverty lines that start at 0 and extends to z^+, then policy 2 will unavoidably be judged to reduce poverty by more than policy 1 for any choice of poverty line within $[0, z^+]$ and for any choice of poverty index within a class of ethical order α+1.[18]

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[15] Cox et al. (1994) and Morduch (1999) found that private transfers made between households (like intergenerational transfers) are not very important. By contrast, private transfers through religious groups and other forms of community association are substantial. Cox et al. (1995) conclude that the headcount ratio would be 25 percent higher among those receiving private transfers had they not received them.

[16] These values are confirmed by Jalan and Ravallion (2003) for the Argentinean workfare program using propensity score matching method.

[17] The parameter λ could (if need be) include the supplementary administrative costs generated by the substitution of SDTT scheme to CS program.

[18] See, for example, Atkinson (1987), Ravallion (1994), and Zheng (2000).

The implementation of the stochastic dominance literature is then straightforward in the *short run*. To address this issue in the l*ong run*, we calibrate the value of $\lambda_{\max}$ for a large range of plausible poverty lines and for a class of acceptable poverty indices. For each ethical order selected, this enables us to define the policy indifference curve as the locus points in (z, $\lambda_{\max}$) space such that the policymakers are indifferent between the CS and the counterfactual SDTT scheme.

3. Empirical Illustration

Arguably, household data from many countries of the MENA region should be used to illustrate the methodology developed above. For expositional simplicity and data availability however, the empirical illustration is limited to the Tunisian case. As several studies show that the poor in Tunisia receive slightly more of total outlays on CS than their counterparts in most MENA countries,[19] the Tunisian experience may then be very instructive for predicting both the potentialities and the difficulties related to the move from universal to more targeted social policies.

3.1. Tunisian Data and Distribution of Poverty under CS Program

We illustrate the use of the methodology presented above using a 1990 Tunisian survey, "*Enquête Nationale sur le Budget et la Consommation des Ménages* 1990" (National Household Budget and Expenditure Survey). This household survey is multipurpose and nationally representative and provides reliable information on consumption expenditures for various items as well as extensive socio-demographic information on 7734 households. No information on income is available. In line with much of the literature on poverty in the developing countries, we thus use total household expenditure (divided by household size) for valuing and comparing individual well-being in our Tunisian data.[20] Observations are weighted by their sample weights multiplied by the household size.

Arguably, spatial price indices should be applied to rural and urban distributions prior to any aggregation procedure. Unfortunately, Tunisian data does not provide price indices at the regional level. To get around this issue, the expenditure distribution has been adjusted by the relevant *upper* poverty line estimated by the World Bank (1995).[21] For expositional simplicity, all the distributions y_e are normalized by the pertinent equivalent poverty line so as $y_e(p^o, p, y_h) = 100$ whenever the equivalent income of a household h is equal to the correspondent equivalent poverty line. Table (1) shows the sample distribution over the different regions of Tunisia, the estimated population share of these regions, as well as that of some poverty indices under the current CS policy.

[19] See, among many others, Tuck and Lindert (1996) and the World Bank (1999).

[20] Dividing total expenditures by household size is not, of course, the only "equivalization" approach as there are likely economies of scale to household consumption. Because it is not central to our argument, we do not address this issue here.

[21] The estimation procedure of the poverty lines followed by the World Bank (1995) applies a version of the food share method suggested by Ravallion (1994). Applied to the 1990 Tunisian household survey, this procedure yields an upper poverty line equal to TND 305 for the urban area and TND 240 for the rural area per person and per year under the current CS scheme. In terms of the benchmark price system, namely p^o, these lines rise to TND 331 for the urban area and 267 TND for the rural area. Thus, an urban household having an expenditure level *per capita* of TND 331 without CS has the same utility level with TND 305 and subsidized prices: $y_e(p^o, p^o, 331) = y_e(p^o, p^s, 305)$.

3.2 Estimation and Inference

Government expenditures on CS program in Tunisia have been substantial throughout the 1980s and the 1990s, amounting to 4.1% of GDP in 1984, 2.9% in 1990, and 2% in 1995.[22] It would thus seem instructive to compare the outcome of this program with that of an alternative one based on socio-demographic targeting transfers (SDTT). For this, we use an illustrative SDTT scheme that involves household proxy-means tests subject to the same aggregate budget as that allocated for CS targeting. The program first estimates a quantile regression model to predict the (*per capita*) households' consumption using easily observable variables, namely, the region of residence, the demographic structure of the households, and the dwelling characteristics.[23] The explanatory variables used – shown in Table (2) and Table (3) – present the main regression results of the (non-adjusted) *per capita* household, meaning y_h, expenditures on these explanatory variables. The program then assigns to the household with the lowest ***predicted*** welfare a *per capita* transfer that pushes its *per capita* income to the next poorest one. This is followed by a transfer to these first two households that increases their predicted welfare to that of the next poorest household. This pattern is repeated until all available funds for CS program are disbursed. This procedure yields a detailed schedule of transfers that depend on observable socio-demographic characteristics.

3.3. Poverty Impact in the Short Run

Quantitative estimates of the comparative policy effectiveness of the two targeting schemes are summarized in Table (4).[24] Poverty indices have been multiplied by 100 for easy interpretation. Using the World Bank (1995) upper poverty line, the initial headcount ratio would be 20.8 percent. It is reduced to 15.6 percent under CS program and to 9.7 percent under the counterfactual SDTT scheme. Yet the headcount ratio only records people who have been lifted out of poverty. Thus, the effectiveness of this counterfactual policy cannot be accurately evaluated using only the incidence of poverty; since this policy is not only a poverty-eliminating program but also a poverty-alleviating scheme. Looking at the poverty gap, this policy would enable reducing the deficit of poverty from its original level from 4 percent of the poverty line under CS to 1.67 under SDTT. For a distribution-sensitive poverty measure, the effects would be outstanding, since the severity of poverty would approximately be divided by three. This means that the poorest should profit more from this change than the less-poor. SDTT would then be more effective in reducing poverty than CS program in the absence of behavioral responses (and complementary administrative costs).

This is confirmed by the stochastic dominance tests of Figure 1. Figure 1 compares the difference between the incidence of poverty ($\alpha = 0$) and the deficit of poverty ($\alpha = 1$) of CS and SDTT over a range of poverty lines that extends to 300 percent of the reference poverty line. For any poverty line no greater than 180 percent of the reference poverty line, the headcount ratio under CS would always exceed that under SDTT. However, for $\alpha = 1$, the SDTT deficit of poverty curve would always lay below that of CS. SDTT would be thus first-order dominant for a range of poverty lines that never exceeds 180 percent of the reference poverty line and second-order dominant no matter when the poverty line is set. As lower

[22] Details about this program can be found in Tuck and Lindert (1996).

[23] The choice of the quantile in the quantile regression is motivated by the focus on the poor population and the available budget to combat poverty. As the CS funds enable the total eradication of poverty if perfect targeting was possible, this approach corresponds to specifying a quantile close to the headcount ratio in the standard regression. More details are in Bibi and Muller (2006).

[24] Distributive Analysis Stata Package (DASP) developed by Araar and Duclos (2007) has been used to calculate the different poverty indices and to produce the different figures.

dominance order always entails higher dominance order, there is no need to test higher order of dominance to prove the superiority of SDTT in the *short run*.

3.4. Poverty Impact in the Long Run

We now turn to the effectiveness of the two targeting schemes under the assumption that SDTT would introduce consumption bias toward leisure instead of labor in response to the change in transfer receipts and narrow the extent of private transfers between households. For this end, Table (4) reports the outcome of SDTT for λ equal to 0.5 and 0.75.[25] Interestingly enough, even with λ equal to 0.7 the performance of SDTT remains much better than CS for distribution–sensitive indices of poverty ($\alpha \geq 2$). The standard errors seem to confirm that SDTT perform significantly better than CS.

As expected however, performances of SDTT are found to be less important for positive values of λ. For instance, without SDTT, only 1.2 additional percent of the population remain in poverty for λ equal to 0.5 and for λ equal to 0.7, SDTT could even increase the incidence of poverty by 1.4 percentage points from its original level yielded under CS program. Including the behavioral responses should then lead to normalizing the large expectations from SDTT not to give them up, especially when the aim is to enhance the living standard of the extremely poor.

This is illustrated in Figure 2 for a range of poverty lines that starts at 0 and extends to 800 percent of the World Bank's (1995) upper poverty line. The area underneath the continuous line corresponds to the set of $\Delta P_{\alpha}^{L.R}(z)$ which are significantly positive (revealing then the superiority of SDTT) whereas the area above the discontinuous line indicates the set of $\Delta P_{\alpha}^{L.R}(z)$ which are significantly negative. Figure 2 shows that the former area is increasing in α. This is because all distribution-sensitive poverty measures would be more affected by the gains that SDTT would yield to the poorest than by the losses that they would cause to the higher-standard classes. Therefore, higher ethical criteria would clearly lead to promoting SDTT even if they generate strong behavioral responses.

3.5. Political Support

Policymakers usually desire political support for the social changes they plan to implement, in addition to their impact on the less well-off of the population. Political support is then fundamental to reach the objectives of social changes and is easier to get when the suggested changes improve the well-being of the majority of citizens. Therefore, it is interesting to estimate the percentage of gainers from the implementation of SDTT financed by the CS funds ($\Lambda_h > 0$). This is shown in Figures 3 by plotting the percentage of individuals who gain from the reform under various scenarios of behavioral responses. This figure shows that the proportion of winners never reaches the threshold of 50 percent of the population, even in absence of consumption bias toward leisure (it would meet with the approval of at most 37 percent of the total population when $\lambda = 0$).

Given the population preferences toward a more equitable distribution, SDTT programs are more likely to get sustained support if policymakers explain and prove that the loss of losers as a share of their income is weak, while the benefits of the poor are large. This is precisely what Figure 4 illustrates. Indeed, while the gain of the poorest is always greater than 20 percent of their income and could exceed 120 percent (for $\lambda = 0$), the loss of the higher-income classes range between one and five percent.

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[25] Recall that according to many empirical studies that have used an *ex post* approach, the share of the replacement income lies between 0.3 and 0.7. See for instance Jalan and Ravallion (2003).

4. Conclusion

Efforts to downgrade the importance of CS have often been hindered by "bread riots" – violent protests have erupted in response to CS cuts in a number of MENA countries in the last decades, including Egypt, Morocco, Tunisia, Algeria, Jordan, and Sudan (see the World Bank (1999) for a fuller discussion). Further, experience from many countries suggests that adverse labor market and incentive effects arise with the move to SDTT and therefore these effects reduce their expected benefits.

Thus, the aim of this paper is to check whether the behavioral responses to SDTT could explain their quasi-absence in MENA countries despite the fact that they are well established in Latin America. For this, we suggest an *ex ante* approach to judge whether the behavioral incidence of SDTT, based on proxy means tests, may explain why CS are preferred.

The methodological and statistical tools are illustrated using a household survey from Tunisia. The Tunisian case offers an example of how the methodology could be applied on other MENA countries by illustrating the information requirements necessary for an *ex ante* evaluation of social changes. A number of broad and interesting results emerge from the Tunisian experience. For instance, the results do not support the idea that SDTT are a panacea to eradicate poverty. However, if the aim is to substantially improve the living standard of the people on the lower part of the income scale, SDTT appear to be more effective in achieving that aim than CS, even if the former leads to stronger behavioral responses than the latter. Unfortunately, more focused targeted transfers to the poor will not automatically meet with the approval of the majority of citizens.

This supports the fact that the effectiveness of any change in reducing poverty is a necessary albeit not a sufficient condition to ensure that the change realizes its objectives. To get it, policymakers should prepare the country for the reform. The inequitable aspect of the CS should be well explained as well as the fact that the proposed change does not aim to save on CS funds. It rather aims to better channel them to the truly needy.

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Table 1: Sample Distribution of Individuals across Tunisian Regions and Distribution of Poverty under CS Program

	Number of observations	Population shares	$P_0(z)$	$P_1(z)$	$P_2(z)$
Great Tunis	1362	20.67	6.31	1.42	0.47
			(1)	(0.3)	(0.1)
Northeast	990	13.75	13.15	3.1	1.1
			(2)	(0.7)	(0.3)
Northwest	1393	14.98	27.2	7.7	3.2
			(1.9)	(0.7)	(0.4)
Middle–East	1409	20.97	11.4	2.6	0.9
			(1.4)	(0.4)	(0.2)
Middle–West	1083	14.6	22.7	6.7	2.8
			(1.8)	(0.7)	(0.4)
Southeast	833	9.19	13.8	2.4	0.7
			(2.2)	(0.5)	(0.2)
Southwest	664	5.8	24.7	6.2	2.3
			(3.3)	(1.1)	(0.5)
Tunisia	7734	100	15.6	4	1.51
			(0.7)	(0.2)	(0.1)

Estimated standard errors in parentheses.
Poverty indices have been multiplied by 100 for easy interpretation.

Table 2: Definition of the Explanatory Variables Used in the Quantile Regression

Labels	Definition
Rural	1 if the household lives in a rural city , 0 otherwise
Great Tunis	1 if the household lives in Great Tunis, 0 otherwise
Northeast	1 if the household lives in Northeast, 0 otherwise
Northwest	1 if the household lives in Northwest, 0 otherwise
Middle–East	1 if the household lives in Middle–East, 0 otherwise
Middle–West	1 if the household lives in Middle–West, 0 otherwise
Southeast	1 if the household lives in Southeast, 0 otherwise
Southwest	1 if the household lives in Southwest, 0 otherwise
Age	Age of the household head
Age2	Squared age of the household head
Nc-2	Number of children in the household that are less than 2 years old
Nc-3-6	Number of children in the household aged between 3 and 6 years
Nc-7-11	Number of children in the household aged between 7 and 11 years
Nc-12-18	Number of adults in the household aged between 12 and 18 years
Na-19-44	Number of adults in the household aged between 19 and 44 years
Na-45-64	Number of adults in the household aged between 45 and 64 years
Ne-65	Number of adults in the household that are more than 65 years old
NCPPS	Number of children in public primary school
NAPSS	Number of adults in public primary school
Nb-room-pc	Number of rooms per capita
Detached House	1 if the household lives in a detached house, 0 otherwise.
Flat	1 if the household lives in a flat, 0 otherwise.
Arab house	1 if the household lives in an Arab house, 0 otherwise.
Hovel	1 if the household lives in a hovel, 0 otherwise.

Table 3: Estimation Results for the Socio-demographic Targeting Scheme (quantile = 0.15)

Variables	Coefficients	P-value
Rural	-60.5*	0.00
Great Tunis	-	-
Northeast	-3.02	0.79
Northwest	-74.6*	0.00
Middle–East	-24.7*	0.02
Middle–West	-46.1*	0.00
Southeast	-23.5	0.06
Southwest	-97.0*	0.00
Age	1.07	0.56
Age2	-0.01	0.47
Nc-2	-13.4*	0.02
Nc-3-6	-15.3*	0.00
Nc-7-11	-10.2	0.08
Nc-12-18	-7.8*	0.03
Na-19-44	20.4*	0.00
Na-45-64	14.3*	0.00
Ne-65	-14*	0.04
NCPPS	-3.4	0.51
NAPSS	6.3	0.18
Nb-room-pc	337.9*	0.00
	-	-
Detached House	101.5*	0.00
Flat	-121.2*	0.00
Arab house	-189.5*	0.00
Hovel		
Pseudo R2	0.176	

* Estimated coefficient is statistically significant at 5% level.

Table 4: Comparative Policy Effectiveness of the Two Social Policies

	$P_0(z)$	$P_1(z)$	$P_2(z)$
Benchmark	19.4	5.33	2.16
	(0.7)	(0.26)	(0.14)
CS program	15.6	4	1.51
	(0.66)	(0.22)	(0.11)
Socio-demographic ($\lambda = 0$)	9.7	1.67	0.47
	(0.53)	(0.12)	(0.04)
$\Delta P_\alpha^{S.R}(z)$ ($\lambda = 0$)	5.94*	2.33*	1.04*
	(0.43)	(0.16)	(0.09)
Socio-demographic ($\lambda = 0.5$)	14.4	2.95	0.9
	(0.64)	(0.17)	(0.07)
$\Delta P_\alpha^{L.R}(z)$ ($\lambda = 0.5$)	1.2*	1.03*	0.61*
	(0.27)	(0.09)	(0.06)
Soci-demographic ($\lambda = 0.75$)	17.0	4	1.39
	(0.7)	(0.22)	(0.09)
$\Delta P_\alpha^{L.R}(z)$ ($\lambda = 0.75$)	-1.4*	0	0.12*
	(0.21)	(0.04)	(0.03)

*Poverty difference between CS program and counterfactual Socio-demographic design is statistically significant at 5% level.
Poverty indices have been multiplied by 100 for easy interpretation.
Estimated standard errors in parentheses.

Figure 1: First and Second Order Dominance in the *Short Run*

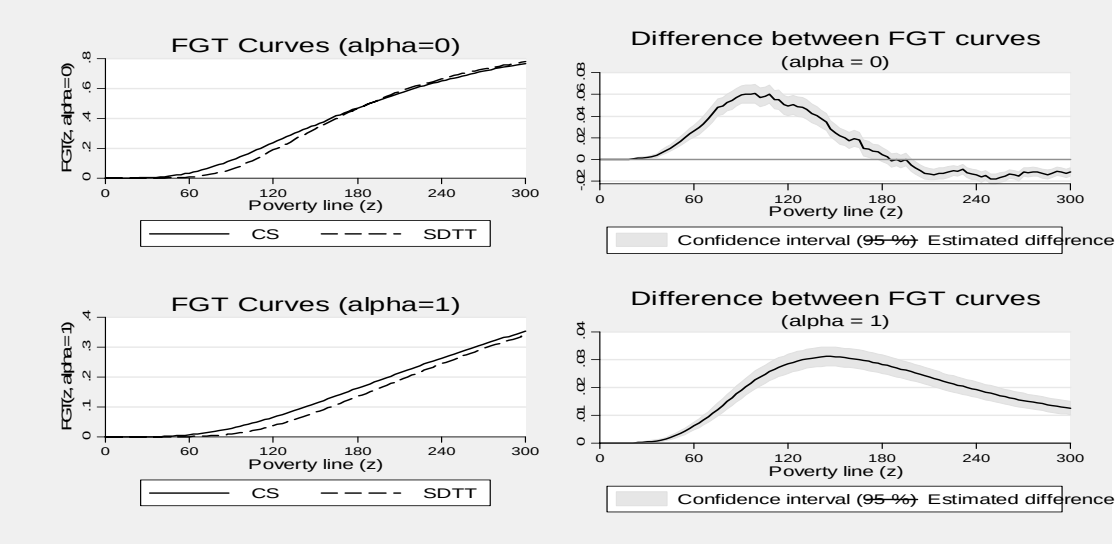

Figure 2: Difference between FGT Curves in the *Long Run*

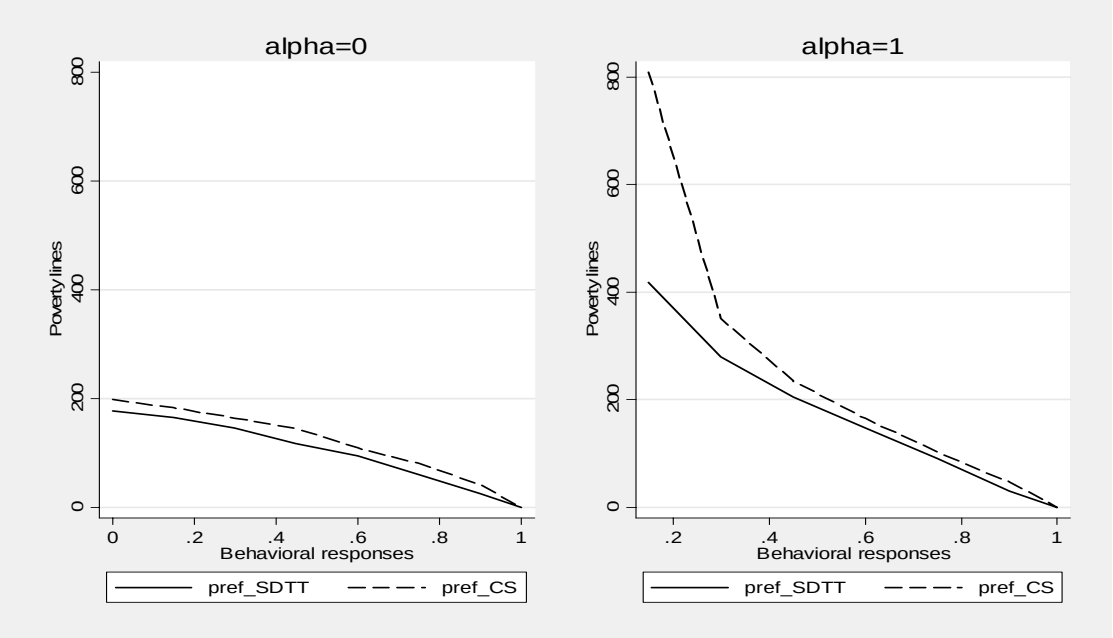

Figure 3: Proportion of Winners under Different Assumptions about Behavioral Incidence

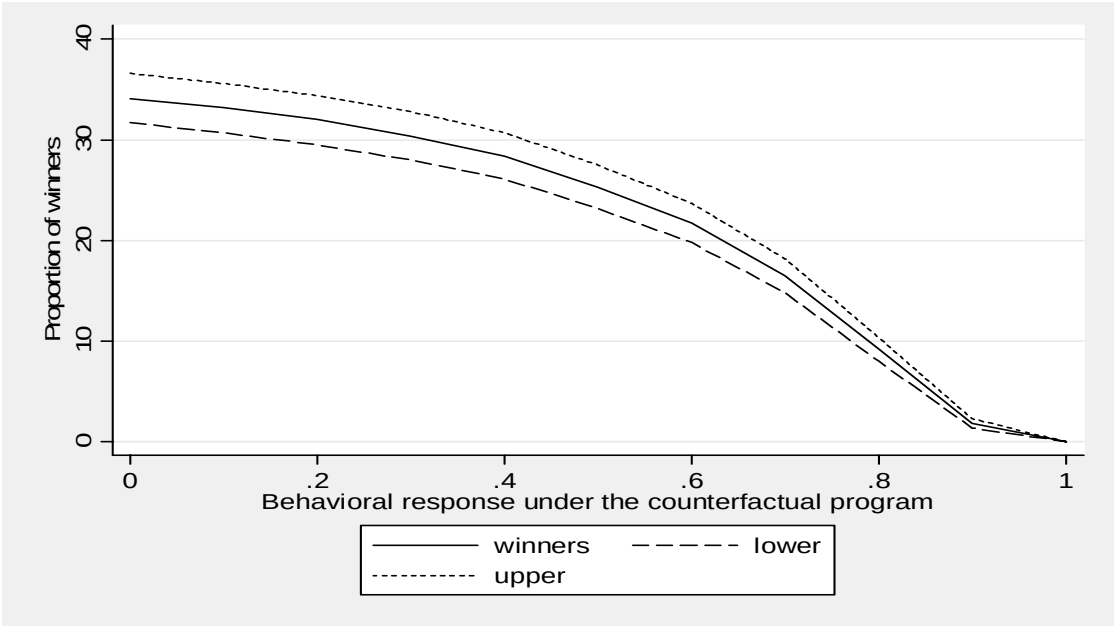

Figure 4: Expected Net Gain as a Share of Income