Macroeconomic effects of the Barcelona Initiative

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Abstract

This paper compares *ex post* and *ex ante* assessments of the macroeconomic effects of trade liberalization in the Mediterranean. Using implications from a standard Ramsey growth model augmented for anticipation and implementation effects, we pool cross section and time series data to estimate *ex post* the effects of trade liberalization on a set of Arabic Southern Mediterranean Partner countries (SMPCs). We find significant and robust evidence for positive effects on major macro variables and discuss the appropriate policies. Second, we review a number of computable general equilibrium (CGE) studies which aimed at assessing the macroeconomic impacts for the same countries *ex ante*. CGE projections are very much at odds with the econometric findings and the biases seem to be systematic for all macro variables. Third, we use ANOVA techniques to identify possible shortcomings both with respect to design and target country of the CGE study. We find that even well-designed CGE studies targeted to an average type of country do not seem to yield reliable results. Overall, our analysis suggests that there is no sound statistical evidence to believe that CGE analysis has been useful in assessing the macroeconomic effects of trade liberalization in the Mediterranean. But we find considerable econometric evidence to support the view that free trade policies have enhanced growth in the MENA region.

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1 This is an updated and extended version of previous research by Lucke and Nathanson (2007).

1. Introduction

Recent unrest and political instability in arabic states has re-alerted European policy makers to the threat of uncontrolled immigration from its southern neighboring region. But the European Union (EU) is not as unprepared as it may seem, for it has, in the last 15 years, spent considerable effort on promoting economic integration and development in the Middle-East and North-Africa (MENA). Nevertheless, there is a widespread perception that this region is slow in responding and adjusting to globalization, cf. World Bank (2003).

The MENA region is a large developing market with more than 400 million customers (about the size of the EU27). Therefore, the EU initiated the Euro-Mediterranean Partnership, aimed at strengthening economic and political ties between the Common Market and MENA. A cornerstone of this so-called Barcelona Initiative (BI) was the gradual creation of a free trade area for industrial products between the EU and its Mediterranean Partners. In the following years, the EU negotiated bilateral Association Agreements (AA) with each partner country, typically allowing for a twelve-year transition phase of tariff and non-tariff barrier dismantling.

The effects of such preferential trade liberalization need not be mutually benefitial due to the possibility of harmful trade diversion effects. In the late 1990s and early 2000s a wave of applied economic research in Computable General Equilibrium (CGE) modeling was directed at assessing this issue from an *ex ante* perspective. A more recent CGE approach is Elshennawy (2012).[2]

By 2011, the Barcelona Initiative has been pushed on further into a "Union for the Mediterranean". But few academic studies exist which provide quantitative assessments of what has been achieved so far.[3] Nor has much attention been devoted to how well today's experience is in line with the ex-ante projection of CGE studies.

Of the few retrospective studies which exist, the most notable ones estimate gravity equations to test for a significant impact of trade liberalization on exports or imports. Peridy (2005), Peridy (2006) finds beneficial effects of lower EU tariffs on Mediterranean countries' exports. However, most of this tariff dismantling took place in the 1970s prior to the BI. Söderling (2005) explicitly studies the effects of the first AAs and finds that some MENA countries' exports seem to have benefited while others have not. Hagemejer and Cieslik (2009) conclude that imports of MENA countries have clearly increased while there is no significant effect of BI-induced trade liberalization on exports.

Unfortunately, the welfare implications of these results are far from clear. The economic well-being of MENA populations does not primarily hinge on foreign trade but on income (GDP), consumption, and investment (as a proxy for future consumption possibilities). Thus, 15 years after the launch of the BI little is known about whether its key element, unilateral trade liberalization, has made the MENA countries better or worse off. This question is of immediate policy relevance and it is the first to be addressed in this paper.

Econometric methods evaluate the macro impacts *ex-post* while the CGE exercises of the 1990s provided *ex-ante* evaluations of the BI's macroeconomic effects. Our study seems to be the first which aims at a serious comparison between these results.[4] But CGE-based claims should, according to good scientific practice, be falsifiable. Surprisingly, however, CGE-analyses have

[2] Lim and Saborowski (2010) do a similar exercise in the World Bank's partial equilibrium TRIST model.

[3] In fact, the final report of a EU-sponsored impact assessment of the Euro-Mediterranean Free Trade Area project (SIA-EMFTA, 2007) relies completely on the old *ex-ante* projections. (The authors take care to point out that they assess only the "*potential* magnitude of economic ... impacts" (our emphasis).)

[4] Hess (2005) presents a meta-study with a similar intent for CGE studies of Doha round effects.

flourished for decades without much econometric review. Filling this gap is the second issue addressed in this paper.

Since some countries have not yet completed tariff dismantling, we focus our analysis on the semi-elasticity of macro variables with respect to tariff rates. This magnitude can be estimated econometrically while trade liberalization is still under way, and it is also readily computed from CGE studies. Comparing projected and realized effects of trade liberalization then permits inference on the reliability of CGE models.[5]

2. The sequel of the paper is structured as follows:

In section II we introduce a theoretical model which explicitly distinguishes between anticipation and implementation effects of trade liberalization. We use this to derive the appropriate specification of our regression analysis. We discuss the data in section III. In section IV we apply dynamic panel estimators to our data set and find significant and robust evidence for positive effects on major macro variables. In section V, we review CGE studies and compute semi-elasticities for ready comparison with the econometric results. We find that CGE projections are very much at odds with the econometric findings. Hence, we use ANOVA techniques to identify possible shortcomings both with respect to design and target country of the CGE study. Controlling for these, we conclude that even well-designed CGE studies are unlikely to yield reliable and, hence, useful results.

3. Deriving the regression equation

Trade liberalization did not come unexpected for consumers and investors in SMPCs. Tariff dismantling was announced long before it was implemented. We start by specifying a theoretical model which takes this kind of informational structure into account. Unlike the related literature which emphasizes the importance of news in DSGE models (cf. Beaudry and Portier (2006), Beaudry and Lucke (2010)), we focus exclusively on a deterministic setting. Future tariff rates are known with certainty from a precise schedule of tariff dismantling as laid out in the Association Agreements (AA) with the EU.

Which implications do news about future tariff liberalization have for the specification of a proper regression equation? In the Web-appendix.[6] to this paper we use a standard Ramsey-type growth model to argue that dynamic optimization implies the existence of two jumps in consumption (and other controls) in response to changes in (trade) policy: Hence, observed changes in consumption at time t may either be explained by perceived future tariff changes (the announcement effect) or by simultaneous or lagged actual tariff changes (the implementation effect).[7] We know of no econometric study in this context which takes this theoretical insight into account. Most CGE-analyses do also not model anticipation effects of changes in tariff policy.[8]

[5] Our investigation focuses exclusively on the macro implications of CGE research. A great value of CGE models may lie in their potential to simulate highly disaggregated sectors of the economy. We do not evaluate the success of CGE models along these lines since the focus of the CGE-based research on the BI was clearly on aggregate variables. However, the macro effects were implied by simulations of disaggregated production sectors. Hence, if the macro projections are found to be problematic, this must have its origins in the underlying simulation results for the disaggregated economy.

[6] https://www.wiso.uni-hamburg.de/professuren/wachstum-und-konjunktur/forschung/prof-dr-bernd-lucke/daten-und-programm-fuer-einzelne-arbeiten/lucke-and-zotti/.

[7] In the theoretical model the implementation effect implies an instantaneous response of consumption to the change in the tariff rate. Realistically, we will also allow for lagged responses in the empirical analysis.

[8] Notable exceptions are Gaitan and Lucke (2007), Lucke, Gaitan Soto, and Zotti (2007) and Lucke and Zotti (2007).

To derive the regression equation formally, imagine an economy in which some imported consumption goods are subject to taxes. Such import taxes raise the price of the aggregate consumption bundle so that the consumer price of aggregate (per capita) consumption c is given by $1+\tau$, where τ is an appropriately weighted function of the import tariffs. Assuming that investment goods are not subject to tariffs, the condition for optimality is typically a standard Ramsey rule such as $\dot{c} = \sigma^{-1}(r-\rho)\,c$, where ρ is the time discount rate and σ^{-1} is the elasticity of substitution. As long as τ is constant, $\dot{c}$ will be independent of the tariff rate. However, if tariff rate changes are announced and implemented at different points in time, consumption growth will exhibit two discontinuities over time. Hence a properly specified regression equation has to include one measure of anticipated and one measure of actual (or lagged) tariff rates.

$$\Delta \ln c_t = \beta_0 + \gamma_l \tau_{t-l} + \gamma_f \tau_{t+f} + \cdots + u_t, \quad l \geq 0,\ f > 0 \tag{1}$$

Moreover, the Ramsey rule implies that a measure of the real interest rate is required. Since this is hard to obtain in applied work for developing countries, we use the neoclassical assumption that the marginal product of capital is a decreasing function of capital, i.e. $r = r(k),\ r' < 0$. In line with standard results we also assume that the policy function for consumption is strictly increasing in k, $c = c(k),\ c' > 0$. We can therefore invert the policy function and express r as a function of c. Thus, rather than using the real interest rate in the regression equation we include c as an additional regressor on the right hand side of (1) – with the expectation of a negative coefficient. Other macroeconomic aggregates like output and investment typically follow similar dynamics.

Finally, we add conditioning variables to capture other changes in the economic environment implicitly assumed constant in the Ramsey model. Hence, for a given endogenous variable z and additional conditioning variables x_k, the general form of the regression equation is

$$\ln z_t = \beta_0 + \beta_1 \ln z_{t-1} + \gamma_l \tau_{t-l} + \gamma_f \tau_{t+f} + \sum \beta_k x_k + u_t, \quad l \geq 0,\ f > 0. \tag{2}$$

Since future variables as regressors are unconventional in regression analysis, we note that, under an Association Agreement, the future tariff rate is a variable which is already fixed today in a binding contract. Thus, the future rate is a nonstochastic anticipation of future tariff rates. One contribution of our paper is that the design of (2) enables us to explicitly test for the existence of announcement effects.

4. Data

For southern Mediterranean countries, national accounts data is available only at the annual frequency. To obtain a reasonably sized sample which may allow for valid inference even in the face of noisy data we pool cross section and time series data of seven Arabic countries, Algeria, Egypt, Jordan, Lebanon, Morocco, Syria, and Tunesia. We generally use all available data for the analysis, i.e. we typically work with unbalanced panels.[9] To be able to single out the BI-effects, we include some pre-Barcelona observations by letting the sample run from 1992 to the most recent observation (usually 2008).

For all countries, we use the following data from the World Bank's World Development Indicators (WDI): Gross domestic product (*lny*), household final consumption expenditure (*lnc*), general government final consumption expenditure (*lng*), gross fixed capital formation (*lninv*),

[9] We occasionally refer to the data set as a panel although its structure is atypical in the sense that the time dimension is greater than the cross section dimension.

exports (*lnexp*) and imports (*lnim*) of goods in services, all in constant prices and local currency units, logged and per capita. We also use the growth rate of population ($d \ln pop$), the (consumer price) annual inflation rate (*inf*), the growth rate of the average official exchange rate per US$ (*dlner*), and net FDI inflows as a percentage of GDP, (*fdi*). Crude oil prices (*lnpoil*) (medium, Fateh 32 API, fob Dubai) are taken from the IMF's primary commodity price data base. This regressor is padded with zeros for countries which do not export (much) oil (Egypt, Jordan, Lebanon).[10]

Aggregate tariff rates can be computed as weighted or as unweighted means. The World Bank publishes estimates of tariff rates (calculated as unweighted means).[11] We use linear interpolation if not more than two observations are missing and the reported values before and after the missings are relatively close. Otherwise, e.g. Syria 1992–1995, we keep the missings. We denote the resulting variable by *tuw*. As an alternative measure of tariff rates we computed the weighted average of tariff rates and denote this by *tw*. For this purpose we use information on tariff revenues provided by the IMF and (in some cases) national statistical offices and divided by nominal imports.

Neither measure includes non-tariff barriers (NTBs). Nor does a measure of the tariff rate capture accompanying economic and institutional reforms which were clearly on the agenda of the Barcelona Initiative. But it is very likely that a country which embraces the Barcelona Initiative seeks to implement its objectives through a multifaceted reform process. The observable reduction in formal tariff barriers may well be correlated with reductions of NTBs, market-oriented economic reforms or efficiency-enhancing institutional change. Hence, the regressors *tuw* and *tw* should be interpreted as catchalls for variables which are difficult to measure but possibly equally important for the success of the Barcelona Initiative.

5. Econometric analysis

Eq. (2) is a dynamic equation – the lagged dependent variable is among the set of regressors. As is well known, standard fixed or random effects estimators are inconsistent in this context. The widely used Arellano–Bond (1991) estimator (AB), however, is consistent, irrespective of whether individual effects are fixed or random. This estimator uses a dynamic set of intruments applied to the first difference of the regression equation. It eliminates the individual effects which cause the asymptotic bias.

We will first consider real GDP per capita, i.e. *lny*. Panel unit root tests (not reported here) give conflicting results about the validity of the unit root null. We therefore use the framework proposed by Bhargava (1986) which nests a trend-stationary and a unit root with drift model. Bhargava's formulation is conveniently nested in (2) by including a linear time trend t among the conditioning variables x_k.

It is not clear from theoretical grounds if weighted or unweighted averages of tariff rates should be used. Both measures have certain disadvantages, cf. Anderson and Neary (2005). Generally, unweighted averages place more weight on high tariff rates and may therefore capture the stimulus of tariff dismantling better than weighted rates. We start our analysis with the former, but note that switching to weighted means makes generally makes no notable difference.

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[10] More details on data are found in the discussion paper version of this article. All data are available upon request

[11] See World Bank (2006).

Table 1
Arellano–Bond GMM results for (3) with $l = 2$.

	dynamic instruments start at lag 2 and end at								
	$\ln y_{it-4}$, tuw_{it-3}	$\ln y_{it-3}$, tuw_{it-4}	$\ln y_{it-3}$, tuw_{it-3}	$\ln y_{it-2}$, tuw_{it-3}	$\ln y_{it-3}$, tuw_{it-2}	$\ln y_{it-2}$, tuw_{it-2}	$\ln y_{it-4}$,	$\ln y_{it-3}$,	$\ln y_{it-2}$,
$\ln y_{it-1}$	**0.477**	**0.463**	**0.402**	**0.497**	**0.442**	**0.487**	**0.417**	**0.367**	**0.470**
	0.000	*0.000*	*0.000*	*0.000*	*0.000*	*0.006*	*0.000*	*0.000*	*0.026*
$\ln y_{it-2}$	**0.282**	**0.258**	**0.299**	**0.314**	**0.302**	**0.389**	**0.279**	**0.290**	**0.344**
	0.002	*0.004*	*0.001*	*0.010*	*0.002*	*0.029*	*0.002*	*0.001*	*0.097*
tuw_{it-2}	**−0.175**	**−0.166**	**−0.204**	**−0.211**	**−0.299**	**−0.253**	**−0.217**	**−0.242**	**−0.240**
	0.001	*0.001*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*
T	**0.007**	**0.007**	**0.007**	**0.005**	**0.006**	**0.004**	**0.007**	**0.007**	**0.004**
	0.000	*0.000*	*0.000*	*0.004*	*0.000*	*0.059*	*0.025*	*0.025*	*0.052*
$\hat{\sigma}$	0.036	0.036	0.035	0.037	0.036	0.037	0.035	0.035	0.036
Sargan P-value	0.040	0.029	0.213	0.658	0.459	0.417	0.030	0.141	0.487

Bold: Regression coefficients significant at 5% level. *Italics*: P-values.

Due to the rather limited amount of observations we use a specific-to-general approach estimating the restricted equation[12]

$$\ln y_{it} = \beta_{i0} + \beta_1 \ln y_{it-1} + \gamma_l tuw_{it-l} + \beta_K t + u_{it} \tag{3}$$

for various leads and lags of tuw. Here, i denotes country i and $l \geq 0$ allows lagged responses to implemented tariff rate changes. We initially neglect the announcement effect and conditioning regressors to which we turn later. Rather we focus on the correct lag specification for the implementation effect. AB-estimation of (3) for $0 \leq l \leq 4$ indicates that tuw is significant only for $l = 2$. Moreover, a second lag of the endogenous variable is significant.[13] To check robustness, we also estimate the equation using simple OLS and GLS estimators with fixed or random effects and obtain precisely the same finding. Detailed results are suppressed to save space, but are available upon request.

AB-estimators use dynamic instruments, i.e. the set of instruments varies with time. We generally instrument a lagged dependent variable by its own past starting in $t - 2$ while we instrument tuw_{it-2} by itself. Similarly, exogenous variables will be instrumented by themselves. As Table 1 shows, different choices of the set of dynamic instruments lead to similar conclusions: All regressors are highly significant, the sum of the coefficients of the lagged endogenous regressors is much smaller than 1 (indicating β-convergence), and, in particular, the semi-elasticity of the lagged tariff rate is roughly 0.2. We can interpret this result as saying that a decrease in the tariff rate by one percentage point has a positive impact on the level of real per capita GDP of 0.2 percent. Hence, as is familiar from standard growth theory, a change in a policy parameter has a permanent level effect, but only a temporary effect on the growth rate.

The specification in the third column of Table 1 is our preferred specification, as here the standard error of the regression and the standard error of tuw_{it-2} are minimal. Morover the point estimate of γ_2 is close to the mean estimate of this coefficient across all columns and the

[12] Note that (3) is equivalent to regressing the growth rate of real GDP per capita on its lagged level, i.e. (3) is a typical growth regression.

[13] Throughout the analysis, we apply a significance level of 5%.

specification passes Sargan's test. Note that there are no generally accepted measures of fits for equations estimated by the generalized method of moments (GMM).

We now amend the benchmark specification by further conditioning variables. We report here only those variables which were to some extent significant, but we note that we have also (with negative results) tested indicators of exchange rates, inflation, and FDI as well as interactions of these variables with country dummies.

We begin with the oil price *lnpoil* as an additional explanatory variable for real per capita GDP in Algeria, Morocco, Syria and Tunisia. It turns out that contemporaneous oil prices are insignificant, while oil prices lagged one or two years are significant with almost the same (positive) coefficient. We prefer the specification with lag 2, because this regressor has more explanatory power when lag 1 and lag 2 are used jointly. See columns (1)–(4) of Table 2.

We proceed to test whether the percentage change in the nominal exchange rate *dlner*, the inflation rate *inf* or foreign direct investment *fdi* are suitable conditioning variables which might either explain the growth experience of real per capita GDP or capture cross-sectional heterogeneity. For all these variables, we test for both a contemporaneous and a lagged influence up to two lags. We suppress the results here, since none of these regressors is significant.

Moreover, we amend the equation by the population growth rate *dlnpop*. This regressor turns out to be significant both contemporaneously and with a lag of one year. Since the estimated coefficients have opposite sign but nearly equal magnitude, we replace the growth rates by the contemporaneous change in the population growth rate $\Delta dlnpop$ and find that this regressor is highly significant with a coefficient of almost -1 and minimal standard error of estimate for all

Table 2
Arellano-Bond GMM results: Conditioning variables.

	Dependent variable: $\ln y_{it}$						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\ln y_{it-1}$	**0.365**	**0.330**	**0.331**	**0.328**	**0.342**	**0.383**	**0.389**
	0.000	*0.001*	*0.001*	*0.001*	*0.000*	*0.000*	*0.000*
$\ln y_{it-2}$	**0.291**	**0.262**	**0.260**	**0.249**	**0.313**	**0.194**	**0.246**
	0.001	*0.004*	*0.003*	*0.005*	*0.003*	*0.025*	*0.003*
tuw_{it-2}	**−0.167**	**−0.156**	**−0.179**	**−0.171**	**−0.195**	**−0.149**	**−0.170**
	0.002	*0.005*	*0.000*	*0.002*	*0.000*	*0.004*	*0.001*
$\ln poil_t$	0.017						
	0.272						
$\ln poil_{t-1}$		**0.039**		0.011			
		0.013		*0.511*			
$\ln poil_{t-2}$			**0.038**	0.032	**0.041**	**0.034**	**0.037**
			0.016	*0.072*	*0.005*	*0.021*	*0.011*
$d \ln pop_{it}$					**−1.215**		
					0.030		
$d \ln pop_{it-1}$						**1.151**	
						0.011	
$\Delta d \ln pop_{it}$							**−0.994**
							0.002
T	**0.008**	**0.008**	**0.009**	**0.009**	**0.007**	**0.010**	**0.008**
	0.000	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*	*0.000*
$\hat{\sigma}$	0.035	0.034	0.034	0.034	0.032	0.032	0.031

Bold: Regression coefficients significant at 5% level. *Italics*: *P*-values.

Table 3
Arellano-Bond GMM results: Announcement effects.

	Dependent variable: $\ln y_{it}$			
	(1)	(2)	(3)	(4)
$\ln y_{it-1}$	**0.709**	**0.779**	**0.798**	**0.605**
	0.000	*0.000*	*0.000*	*0.000*
tuw_{it-2}	**−0.105**	**−0.103**	**−0.209**	**−0.144**
	0.004	*0.017*	*0.000*	*0.001*
tw_{it+1}	**−0.459**			**−0.390**
	0.000			0.006
tw_{it+2}		**−0.506**		−0.097
		0.006		*0.599*
tw_{it+3}			**−0.388**	0.015
			0.018	*0.924*
$\Delta d \ln pop_{it}$	**−1.203**	**−1.343**	**−1.411**	**−1.140**
	0.000	*0.000*	*0.000*	*0.000*
T	**0.004**	0.002	0.002	**0.005**
	0.024	*0.320*	*0.305*	*0.011*
$\hat{\sigma}$	0.023	0.026	0.025	0.022

Bold: Regression coefficients significant at 5% level. *Italics*: *P*-values.

tested specifications, cf. columns (5)–(7) of Table 2. Column (7) becomes our new benchmark specification.

So far, we have focused on the implementation effect as measured by lags of the unweighted aggregate tariff rate *tuw*. (Very similar results obtain if we use lags of the weighted tariff rate *tw*.) We have, however, not yet accounted for the announcement effect emphasized in the theoretical section. This effect is due to forward looking behavior of individuals, and each individual will find future tariff dismantling important to the degree at which he or she will trade at the lower tariff rates. Thus, the weighted tariff rate *tw* seems appropriate to capture the announcement effect.[14]

Regression results for the benchmark equation plus future tariff rates at various lags are given in Table 3.

Clearly, tariff rates perceived to prevail in the future significantly influence today's real GDP per capita. This effect is almost independent of the implementation effect, since the coefficient of lagged *tuw* continues to be highly significant, albeit possibly with a slightly reduced semi-elasticity. But while the implementation effect is rather weak (semi-elasticity of approx. 0.1–0.2), the announcement effect is much stronger (semi-elasticity of 0.4–0.5). Future tariff rates are significant up to a lead of three years, but the underlying effects are clearly not orthogonal to each other, as column (4) of Table 3 shows. In fact, it seems that the announcement effect is well captured by a lead of one year as in column (1).

This underlines the importance of a credibly announced policy for the planning behavior of economic agents. The future tariff rate is inidicative of tariff policy over a long period of time, probably exceeding the the twelve-year horizon of the AAs. Our results say that this perspective stimulates economic activity more than a low tariff rate in any single year.

[14] Note that the weights are endogenous. We take this into account by instrumenting future weighted tariff *tw* rates by lagged unweighted tariff rates *tuw* (at least a lag of two periods relative to the dependent variable). Hence, the GMM estimate is consistent.

Table 4
Arellano–Bond GMM results for FDI and components of GDP.

	Endogenous variable:				
	$\ln c_{it}$ (1)	$\ln inv_{it}$ (2)	$\ln g_{it}$ (3)	$\ln imp_{it}$ (4)	fdi_{it} (5)
$end.var_{it-1}$	**0.583**	**0.670**	**0.517**	**0.474**	**0.326**
	0.000	*0.000*	*0.000*	*0.000*	*0.012*
$end.var_{it-2}$		**−0.241**	**−0.193**		
		0.019	*0.003*		
tuw_{it-2}	**−0.278**	**−0.516**		**−0.293**	
	0.001	*0.001*		*0.030*	
tw_{it+1}	**−0.974**	**−1.387**			**−0.258**
	0.000	*0.000*			0.001
$\ln poil_{t-2}$	**0.059**			**0.148**	
	0.048			*0.000*	
$\Delta d \ln pop_{it}$	**−2.181**	**−3.526**			
	0.000	*0.000*			
T			**0.013**	**0.017**	
			0.000	*0.000*	
$\hat{\sigma}$	0.048	0.098	0.042	0.087	

Bold: Regression coefficients significant at 5% level. *Italics*: *P*-values.

We now turn to the question by which channels lower protection stimulates real per capita GDP. For this purpose, we run the same type of regression for components of GDP (consumption, investment, government expenditure and imports[15]) and for foreign direct investment. All of these variables may be affected by decreased tariff barriers, because imported goods become cheaper, because economic prospects open up or – in the case of the government – because revenues fall. Hence they may function as transmission channels through which the effects of tariff rate reductions on GDP operate.

A summary of the results is given in Table 4. We set out with an regression equation containing all the regressors in the first column of this table and then delete the insignificant variables. Government consumption, column (3), is not significantly affected by tariff dismantling, thus it seems that the loss in revenues (which can be significant in SMPCs) was offset by either increased tax revenue elsewhere or by higher net borrowing but not by reduced expenditure.

Imports, column (4), are stimulated by implemented tariff rates decreases, but show no significant repsonse to announcements. The positive impact of the lagged oil price is probably due to a positive income effect from higher oil prices and the same interpretation applies, although only very weakly, to the consumption equation, column (1). Consumption and investment, column (2), respond positively to both announced and implemented tariff rate decreases. The semi-elasticities are clearly larger for investment. This suggests that there is a fairly strong supply-side reaction to actual and anticipated increases in competition. From column (5) we learn that net inflows of foreign direct investment respond positively to *announced* tariff rate decreases only. The fact that *implemented* tariff rate decreases are not significant in this equation reinforces the view that FDI is a forward-looking variable.

Summing up, we have derived regression equations for real per capita GDP, consumption, investment, government absorption and imports. These equations are similar to standard growth

[15] We do not emphasize exports here since conditions for exporters have hardly changed under the BI. We just note that analogous regressions for exports do not deliver significant results with respect to tariff rates.

Table 5
Estimated ex-post semi-elasticities.

	GDP	Consumption	Investment	Imports
controlling for level of	*Results for* tuw_{it-2}			
$\ln g_{it}$	0.105	0.278	0.354	0.293
$\ln inv_{it}$ and $\ln g_{it}$	0.105	0.278	-	0.200
neither	0.105	0.278	0.516	0.293
	Results for tw_{it+1}			
$\ln g_{it}$	0.459	0.974	1.211	0
$\ln inv_{it}$ and $\ln g_{it}$	0.459	0.974	–	0
neither	0.459	0.974	1.387	0

regressions with particular emphasis on tariff rates as a catchall for trade liberalization and accompanying economic reforms. The regression coefficients for the tariff rates are semi-elasticities computed *ex post*. It is interesting to compare these to semi-elasticities which were computed *ex ante* by using CGE models.

It is important to note that the ex-post estimates of the semi-elasticities are short-run elasticities, since the regression equations contain lagged endogenous variables. This matches well with the overwhelmingly static (i.e. short-run) CGE-studies which have tried to assess the effects of the BI *ex ante*.[16]

Many CGE studies analyse the effect of trade liberalization on output under the counterfactual assumption that government absorption and possibly also investment stay constant.[17] In this case we need to use ex-post semi-elasticities which were estimated in a regression controlling for the level of the variables held constant in the CGE model. We have therefore reestimated the benchmark specification for log output (Table 3, column (1)) with either contemporaneous government absorption or also contemporaneous investment as additional regressor. We instrumented with the same variables lagged two periods. In both cases these regressors are clearly insignificant and the point estimates of the tariff rates hardly change. Hence, for output there is no need to distinguish semi-elasticities of tariff rates with respect to these type of control variables.[18]

But CGE studies often also derive projections for other variables like consumption, investment, and imports. To account for assumptions of constant government absorption or constant investment we therefore reestimate the specifications in columns (1), (2) and (4) of Table 4 to control for the contemporaneous levels of these variables.[19] We report modified semi-elasticities when one of these variables is significant. For ready reference, we summarize the resulting semi-elasticities in Table 5:

We will now compare these ex-post semi-elasticities with the semi-elasticities implied by the ex-ante projections of CGE studies.

[16] In case the CGE study is calibrated on a base year data set prior to credibly announcing a schedule of tariff rate decreases, the ex-ante semi-elasticities must be compared to the sum of the announcement effect and the implementation effect. Otherwise (i. e. if the base year data set contains already the announcement effect), the ex-ante semi-elasticity must be compared only to the ex-post semi-elasticity with respect to tuw_{it-2}.

[17] We know of no work in which investment is held constant, but government absorption is not.

[18] Note that the insignificance of investment implies that the logged savings rate (measured as investment over output) is also insignificant.

[19] Clearly, in the investment equation we only control for the level of government expenditure.

6. CGE based semi-elasticities and their econometric counterparts

This section examines CGE studies which simulate trade liberalization measures for SMPCs. We use data and information reported in each paper to calculate what was, at the time of writing, an ex-ante perspective on the semi-elasticities of main macroeconomic variables[20] with respect to quantitative import barriers in the partner countries. In the following section, these figures will be compared with the econometric estimates.[21]

Results of CGE studies may crucially depend on certain assumptions built into the model. These properties (e.g. small open economy or world economy assumption, static, sequential or dynamic models, perfect or imperfect competition, full employment or fluctuations in factor usage, assumptions on factor mobility, production technology, current account balance, exchange rate regime etc.) are suppressed to save space, but are available in the Web-appendix.

These CGE studies deliver *ex ante* estimates of semi-elasticities, documented in detail in the Excel sheets on the paper's web page and the discussion paper version of this article. The econometric results of the preceding section can be used to construct analogous estimates ex-post. In doing so, a number of issues merit attenion. First, the most prominent measure in CGE analysis is welfare. Unfortunately, welfare projections are hard to falsify. Econometrically, semi-elasticities for GDP, consumption, investment and imports are much more rewarding objects, but many CGE studies do not bother to report comprehensive results for these variables.

Second, the econometric estimates in the preceding section condition on the level of lagged endogenous variables. Hence, the estimated semi-elasticities must be thought of as short-run elasticities. Static CGE models are typically silent on their horizon in real time, but since capital is held constant, the short-run econometric estimate is the appropriate counterpart. For sequential and dynamic models, the situation is more protracted. If these models report the values of macro variables in the new steady state, we compare the implied semi-elasticity to the short-run semi-elasticity from the econometric estimate divided by $1-\hat{\rho}$, where $\hat{\rho}$ is the estimated coefficient on the lagged endogenous variable(s). If simulation results are given for a finite simulation period of n years, we multiply the short-run semi-elasticity from the econometric estimate by $(1-\hat{\rho}^{n+1})/(1-\hat{\rho})$.

Third, CGE models make different exogeneity assumptions. If (in the simulations) government expenditure is held fixed we use econometric estimates from regressions controlling for government expenditures. We proceed analogously for investment. Finally, in line with our theoretical analysis, we compute the semi-elasticity based on both the announcement and the implementation effect if the CGE model is calibrated to a benchmark data set prior to signing the AA, while otherwise we use only the estimate of the implementation effect.

The right block of columns of Table 6 displays the results of these computations, while the left block of columns contains the CGE-based counterparts. This allows a ready comparison between *ex ante* and *ex post* measures of the macroeconomic effects of BI-induced trade liberalization.

[20] These are: private consumption, investment, government expenditure, imports and FDI.

[21] While the details of trade liberalization scenarios as specified in the AAs are quite complex, most CGE studies include (or even focus on) a scenario of abolishing tariffs vis-à-vis the EU in manufacturing only. To ensure maximum comparability, we use results from this (benchmark) scenario whenever possible. Otherwise we use the scenario closest to the benchmark. We implicitly scale the results using the appropriate volume-based weights by calculating how the reduction of tariffs (for EU products, say) translates to a reduction of the overall average tariff rate by weighing with the appropriate share of EU trade. We calculate the semi-elasticities with respect to this average tariff rate to make them consistent with the definition of the regressors used in the econometric analysis.

Table 6
Semi-elasticities computed from.

Model	CGE studies				Econometric estimates			
	GDP	C	I	IM	GDP	C	I	IM
E2	0.741		1.856	3.425	0.564	0.000	1.565	0.293
E3	0.205	−0.103	0.617	2.987	1.930	3.002	3.931	0.557
E4	0.347	−0.108	1.071	1.975	1.930	3.002	3.931	0.557
E6	0.116				0.564			
E7				0.252				0.200
E9				0.244				0.200
E10	−0.130			0.944	0.105			0.200
J4	−0.001			−0.002	0.564			0.293
J5	−0.045	1.096	0.501	1.028	0.105	0.278	0.516	0.293
L1	0.001	0.001	0.002	0.005	0.105	0.278	0.354	0.293
L2	0.775	0.354	0.857	0.452	1.938	3.002	3.333	0.557
L3				0.064				0.293
M2		0.080	−1.149	0.148		0.278	0.516	0.293
M3	−0.307			6.491	0.105			0.200
S2	0.011	0.003	0.065	0.142	0.564	1.252	1.903	0.293
S3	0.015	0.006	0.054	0.084	0.564	1.252	1.903	0.293
S4	1.360	1.020	1.700		1.938	1.252	3.932	
S5	−0.031	0.707	−0.072	2.357	0.564	1.252	1.903	0.293
T3				1.789				0.200
T4				1.465				0.293
T5	0.096	−0.908	4.060	7.595	0.564	1.252	1.903	0.293
T7	1.016				0.105			

CGE Studies on SMPCs: Egypt: E1: Augier and Gasiorek (2003); E2: Bayar (2001); E3, E4: Dessus and Suwa-Eisenmann (1998a), Dessus and Suwa-Eisenmann (1998b); E5: Hoekman and Konan (1998); E6: Konan and Kim (2004); E7,E8: Konan and Maskus (1996,2000); E9: Maskus and Konan (1997); E10: McDonald, Evans, Gasiorek, and Robinson (2006); Jordan: J1: Augier and Gasiorek (2003); J2: Feraboli, Lucke, and Gaitan (2003); J3: Feraboli, O. (2007), J4: Hosoe (2001); J5: Lucke and Lucke (2001); Lebanon: L1: Dessus and Ghaleb (2008); L2: Lucke, Gaitan Soto, and Zotti (2007); L3: Martin (2000); Morocco: M1: Augier and Gasiorek (2003); M2: Bayar (2001); M3: Mc Donald et al. (2006); M4: Rutherford (1997); Syria: S1: Augier and Gasiorek (2003); S2, S3; Chemingui and Dessus (2004), Chemingui and Dessus (2008); S4: Gaitan and Lucke (2007); S5: Lucke and Lucke (2001); Tunisia: T1: Augier and Gasiorek (2003); T2: Bayar (2001); T3: Brown, Deardorff, and Stern (1996); T4: Chatti (2003); T5: Chemingui and Thabet (2008); T6: Cockburn, Bernard, and Benoît (1998); T7: Konan and Kim (2004); T8: Konan and Maskus (2004). CGE models not listed here lack results for macroaggregates.

Ideally, *ex ante* and *ex post* semi-elasticities in Table 6 should be fairly close because econometric estimates account for changes in the economic environment by appropriate choice of conditioning regressors and hence isolates the effects of changes in the tariff structure in principle in the same way as a CGE analysis does.

Unfortunately, Table 6 shows that the *ex ante* and *ex post* assessments of BI – induced trade liberalization are very much at odds with each other. To see this, a quick look at the deviations (henceforth denoted DEV(X) for variable X) between CGE-based and econometrically estimated semi-elasticities is revealing. As the descriptive statistics in Table 7 make very clear, not only is the standard deviation of these deviations tremendous for all macro aggregates, but, possibly worse, the deviations seem to be systematically biased: CGE-based assessments for effects on GDP, consumption, and investment seem to be systematically smaller than their econometric counterparts, while the reverse is true for imports. (Observe that the means of the deviations are significantly nonzero for all four types of deviations).

Table 7
Descriptive statistics for deviations between *ex ante* and *ex post* semi-elasticities.

	DEV(*GDP*)	DEV(*C*)	DEV(*I*)	DEV(*IM*)
Mean (*t*-stat.)	−0.50 (−3.17)	−1.27 (−3.19)	−1.34 (−2.98)	1.34 (2.81)
Std. dev.	0.63	1.32	1.56	2.13
Minimum	−1.73	−3.11	−3.31	−0.30
Maximum	0.91	0.82	2.16	7.30
% observ. <0	0.13	0.09	0.17	0.65

Unless there is a fundamental flaw in our econometric analysis, these results suggest that CGE modeling may be a very unreliable and possibly worthless way to assess the effects of trade liberalization policies. Of course, further analysis is required before such a far-reaching conclusion might be drawn. On the one hand, one might argue that Table 7's results are due to a number of qualitatively minor CGE studies, while other, better-designed studies have delivered useful projections. On the other hand, one might also object that some countries among the SMPCs are particularly difficult to model, either because data quality is low or because their governments pursue economic policies more or less far off the free market paradigm on which most CGE models are based.

We will therefore test if either inappropriate design of CGE studies or the analysis of certain "difficult" countries may explain the enormous deviations between *ex ante* and *ex post* assessments. To this end, we use standard ANOVA techniques. To capture differences in the modeling design, we define a number of variables as follows:

DSMOPEC = 1, if small open economy model, 0 if not.
DSTATIC = 1, if static model, 0 if not.
DPERFCOMP = 1, if perfect competition, 0, if imperfect competition, 0.5 if competition is perfect on some and imperfect on other markets.
DFULLEMP = 1, if full employment, 0, if flexible employment, 0.5, if employment is fixed on some markets but fluctuates on others.
DFACMOB = 1, if all factors are fully mobile across sectors, 0, if not.
DCA = 1, if current account zero, 0, if not.
DGEX = 1, if government expenditure is exogenously fixed, 0, if not
DIEX = 1; if private investment is exogenously fixed, 0, if not.
DCALI = 1, if base year precedes signing of AA, 0, if not

Moreover, we define country fixed effects variables *DJOR*, *DLEB*, *DMOR*, *DSYR*, *DTUN* to single out countries for which CGE analysis might be more or less difficult than for the default country, Egypt.

We first check if large deviations between *ex ante* and *ex post* semi-elasticities are due to the design or the target country of specific studies. For this purpose, we regress the absolute values of their deviations on the dummy variables defined above. We run separate regressions for the absolute values of DEV(*GDP*)and DEV(*IM*). For consumption and investment, however, we have only few observations, so we combine these in a single regression, i.e. we also use the absolute value of DEV(*C,I*) as dependent variable. ANOVA results are given in Table 8, where we display only those variables which are found significant in any of the regressions.

Table 8
ANOVA results for absolute value of deviations.

	Dependent variable:		
	abs(DEV(*GDP*))	abs(DEV(*C,I*))	abs(DEV(*IM*))
Constant	**0.595**	0.578	**5.873**
	0.039	*0.201*	*0.000*
DJOR		−**1.113**	
		0.043	
DLEB	−**0.491**		
	0.030		
DSYR	**0.238**		
	0.042		
DTUN	**0.358**		**3.302**
	0.020		*0.001*
DSTATIC	−**1.323**	−**1.301**	
	0.000	*0.001*	
DPERFCOMP	**1.059**	**2.252**	
	0.001	*0.000*	
DFACMOB			−**4.683**
			0.002
DSMOPEC			−**3.194**
			0.002
DCALI			**1.913**
			0.019
$\bar{R}^2$	0.892	0.607	0.616

Bold: Significant regression coefficients. *Italics*: *P*-values.

Looking at $\bar{R}^2$, it turns out that between 60% and 90% of the variance can be attributed to either country-specific factors or to the design of the studies. Syria and Tunisia seem to be countries more difficult to study than the average, while the converse is true for Jordan and Lebanon. Static models seem to yield more reliable projections than dynamic models and assuming elements of imperfect competition apparently increases the quality of forecasts for the GDP, consumption and investment. For imports, the small open economy assumption seems to work better than a model with simultaneous changes elsewhere in the environment and assuming restrictions on factor mobility seems to be counterproductive. Finally, CGE studies calibrated to data sets prior to signinig the AA are prone to relatively high errors. This finding makes sense since CGE models typically assess merely the implementation effect but not the announcement effect.

Thus, specifics of the design or the target country of CGE studies may partially be responsible for the quality of the CGE projections. However, since the constant terms are large and significantly positive in two of the three regressions, the fundamental problem is not resolved: CGE studies do not seem to be very reliable in the light of econometric evidence. In fact, given the results from Table 8, we can now control for a number of adverse influences on particular CGE studies and can hence pose the central question more specifically: How reliable is a CGE-based assessment of trade liberalization policies, if the CGE study is well-designed and devoted to a country of average difficulty in terms of data quality, market strucutre and economic policy? By controlling for the regressors found significant in the ANOVA analysis, we can answer this question in a simple regression of the *ex ante* assessment of a CGE model (semi-elasticities from Table 6 for variable *X* are denoted CGE(*X*)) on the *ex post* econometric counterpart (denoted ESTIM(*X*)). Ideally, then, the regression coefficient of *estim* should equal one.

Table 9
Regressing CGE(X) on ESTIM(X) and controls from Table 8.

	Dependent variable *CGE*(X):		
	CGE(*GDP*)	CGE(C,I)	CGE(*IM*)
ESTIM(X)	**−0.816**	−0.335	−1.501
	0.016	*0.315*	*0.601*
$\bar{R}^2$	0.356	−0.004	0.634

Bold: Significant regression coefficients. *Italics*: *P*-values.

The results of this exercise, given in Table 9, are quite revealing. Even when controlling for possible sources of errors, CGE-based assessments of trade liberalization have no reasonable relationship with the econometric evidence. In fact, rather than being positive and equal to one, the estimated coefficient is negative for all three regressions, it is insignificant in two of the regressions and even significantly negative in the regression for GDP.

7. Conclusions

Summing up, we find strong and robust econometric evidence for positive effects of BI-induced trade liberalization. GDP, consumption, investment and imports respond positively to lower tariff rates and they do so not only for implemented tariff changes but also in response to credible announcements of future trade liberalization.

These results are highly relevant from a policy perspective. Not only do lower applicable tariff rates stimulate growth of GDP, consumption and investment, but the mere (credible) announcement of lower rates of protection promotes economic activity. In fact, the announcement effect is considerably stronger than the implementation effect, i.e. a discretionary change in tariff rates is not as effective as a binding commitment to decrease tariffs over a longer time horizon. Prior to the Barcelona Initiative, tariff policy was often intransparent and seemingly arbitrary in MENA countries. Our results underline that a shift to a systematic and foreseeable policy is of key importance for economic success, cf. Baldwin (2009). It is also noteworthy that FDI does not seem to react at all to discretionary tariff rate changes but only to the credible pledge to engage in a systematic and long-lasting policy of trade liberalization.

Moreover, for GDP, consumption and investment the stimulating effects of trade liberalization are on average stronger than projected *ex ante*. Hence policymakers should be encouraged to adopt more open policies, even if consultants working with CGE models do not expect much potential. Policymakers need to take into account that an economy reacts in a dynamic and multifaceted way to liberalization, changing features of an economy which formal model projections may falsely assume as constant.

For example, technology, markets and their degree of competition is fixed in CGE models, while it seems reasonable to expect that opening a country to foreign trade introduces new ideas, increases competition, and opens new markets and profit opportunities. We have found some support for the conjecture that CGE models fare better if they allow for imperfect competition, so that opening up may endogenously increase competition in these models. We have also found that assuming full factor mobility across sectors improves CGE projections, which seems plausible if we interpret factor movements between existing markets as capturing at least part of the factor movements to newly developping markets that are likely to happen in reality.

From the point of view of economic policy this implies that the potential for economic growth can be greatly enhanced by a broad approach to liberalize the economy – or at least not oppose changes which go along with more openness, cf. Tanzi (2004). Policymakers should support and foster market dynamics by deregulating product and labor markets. Moreover, since estimated semi-elasticities are considerably larger for investment than for consumption, we conjecture that supply-side effects are an important channel stimulating the economy. Static CGE models ignore these effects and thus underestimate the growth potential of policies aimed at opening up and liberalizing a developing economy.

Interestingly, ex-ante projections overestimate the response of imports while underestimating the responses of GDP, consumption and investment. The reason for the former is likely due to overly optimistic calibration: The elasticities of substitution between domestic and imported commodities are usually an arbitrary choice of the modeler. Neither can they be derived from the benchmark data set using familiar calibration techniques, nor are they typically the result of serious econometric estimation. Rather, the choice of a particular value is often merely justified by the fact that similar values have been used in other studies – without digging any further. If the modeler wants to get something interesting out of his model, he may be tempted to choose relatively high elasticities of import demand to get his model going.

Overall, CGE assessments of the likely macroeconomic effects of the Barcelona Initiative seem to have been very unreliable in the sense that there is no statistically significant relationship between the *ex ante* assessments and the econometric evidence derived more or less *ex post*. This, as already mentioned in the introduction, is not meant to discard CGE analysis completely, since the ability of CGE models to project developments on a highly disaggregated basis may nevertheless be very useful. But since the CGE-based macro projections are just the aggregate of sectoral projections, it is clear that some rather fundamental problem must be present in the latter, too.

For instance, CGE analysis has often stressed the fiscal costs of lower tariff rates. Our analysis does not support this view. Government consumption is on average unaffected by trade liberalization (and debt levels in the Mediterranean are low), so that we reach a reassuring conclusion for policymakers in favor of liberalization: The growth stimulus on the macroeconomy is large enough to compensate the government budget for loss of tariff revenue by increased revenue elsewhere in the growing economy. Here again, policymakers should not be scared away by ex-ante projections derived from CGE models. Rather, consultants should use econometric evidence from countries which opened up earlier to convince the government of the prospects of trade liberalization and accompanying economic policies.

While we must caution that data limitations and the fairly low standards of documentation in the CGE literature leave plenty of caveats to our analysis, we are confident that we have worked as throroughly and as carefully as possible without finding any evidence which would support the view that CGE analysis is worth the effort. This is bad news also for ourselves, as some of the studies we have reviewed are our own. If the reader is concerned that we may have had vested interests she may find it reassuring that the result we present is certainly in the opposite direction.

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