When trade stops: Lessons from the Gaza blockade 2007–2010 ☆

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ABSTRACT

This paper uses detailed household expenditure and firm production data to study the welfare consequences of the blockade imposed on the Gaza Strip between mid-2007 and mid-2010. Using the West Bank as a counterfactual economy, we find that welfare declined by 14%–27%. Moreover, households with larger pre-blockade expenditure levels experienced larger welfare losses. We show that this large decline in welfare may be due to a combination of resource reallocation and reduced productivity. Workers were reallocated from manufacturing to services, and from industries that use imported inputs intensively, or export. In addition, labor productivity fell by 20% on average.

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1. Introduction

While almost all economists agree that international trade is beneficial, measuring just how beneficial it is is difficult. As Irwin (2005) explains: "In theory, the gains from international trade are represented by comparing welfare at the free-trade equilibrium with welfare at the autarky equilibrium. In practice, such a comparison is almost never feasible because the autarky equilibrium is almost never observed." This paper studies the consequences of a rare episode in modern history, in which the Gaza Strip came close to being autarkic as a result of an Israeli and Egyptian blockade that was imposed on it between September 2007 and June 2010.

The first part of the paper studies the welfare implications of the blockade on Gaza. An important advantage of the analysis is the existence of a natural counterfactual economy, the West Bank, which was not blockaded. At the time the blockade began, the West Bank and Gaza had similar economic and political institutions, and, importantly, both before *and after* the blockade on Gaza, the two regions had very similar trends in prices and consumption. Using detailed expenditure data at the household level, we calculate the monetary equivalent of the welfare loss caused by the blockade based on the concept of compensating variation, and using the West Bank as the counterfactual economy for Gaza. That is, the compensating variation we calculate is *not* the sum of money that will make a household in blockaded Gaza as well off as it was before the blockade, but rather the sum that will make it as well off as it would have been had it been located in the open West Bank rather than in the blockaded Gaza during these years. We find that the average welfare loss for a household in Gaza was equal to between 14% and 27% of the value of its pre-blockade expenditure. Moreover, we find that all measures of welfare losses are disproportionally larger for wealthier households.

We contrast these results with the welfare effects predicted by some trade models. Using the formulas in Arkolakis et al. (2012) (henceforth ACR), which give the predicted welfare change as a result of a trade shock for an important class of trade models, and the one suggested by Ossa (2012), we calculate that the predicted welfare loss in Gaza is at most 10.1% according to the ACR formula, but possibly as high as 24.3% according to the Ossa (2012) formula. We discuss some possible reasons for the difference between our results and the results predicted by ACR. Importantly, since the framework used in these models is a static one, while the results we report are based on a relatively short-lived event, we do not see our results as conflicting with those derived in their work. But, we argue, our results can help interpret the ACR formula for gains from trade.

The second part of the paper studies the economic mechanisms that led to this large welfare loss. Using detailed firm-level data, we document two key facts about the adjustment of production in Gaza during the blockade.

First, there was a large reallocation of workers away from manufacturing, where employment fell by 33%, and into services, where employment rose by 24%. A more disaggregated analysis suggests that the loss of access to world markets was the cause for this reallocation: Workers were reallocated away from industries that exported a large share of their output or imported a large share of their inputs.

Second, the average worker's productivity in Gaza, as measured by real value added per worker, declined by 20% during the blockade. This decline differed greatly between the manufacturing and the services sectors: a 36% decline in manufacturing, and only a 0.6% decline in services. Moreover, a more disaggregated analysis of 72 industries reveals that the overall decline was predominantly the result of a decline in productivity within industries, and not of reallocation of workers between industries.

These findings suggest a strong complementarity between imported inputs and labor, especially in the manufacturing sector. In many models of international trade, the important margin of adjustment is between import-competing and exporting industries and firms. In Gaza, however, manufacturing as a whole depended on access to world markets. Lacking this access, both import-competing and exporting industries experienced a large decline in productivity and in employment, and workers were reallocated to the less productive services sector.

Since the blockade was substantially eased after three years, and Gaza had been an open economy for a long time before, our analysis captures the relatively short-run effects of moving from a trading equilibrium to near-autarky. The difference between short-run and long-run effects of trade shocks may be substantial (see for example Trefler, 2004): While Gaza may not have fully adjusted to its new state of near-autarky by 2009, it was still able to use machinery, and possibly some old inventories of raw materials, that were previously imported, and not produced domestically. While the first consideration suggests that the short-run welfare losses we calculate may exceed the long-run welfare costs, the second consideration implies the converse. The question of which effect is likely to be larger is beyond the scope of this paper. At any rate, it is important to study the short-run effects of autarky for a few reasons. First, short-run effects are key to the analysis of trade policy. Economic sanctions, or the threat of using them, are still very much a part of international relations, and the study of the Gaza experience improves our understanding of their possible implications. Extreme changes to trade policy can also lead to a large decline in trade volume, and the study of the short-run effects of the collapse of trade in Gaza can serve as a cautionary tale against the risks of trade wars.

Second, studying the short-run effects of the blockade on Gaza can inform our thinking about the long-run consequences of trade. While Gaza did not yet fully adjust to its new state of near-autarky, the fact that the large adjustments that already took place—the pattern of reallocation of workers, the fall in productivity, the decline in expenditure inequality—are all in line with standard theory is worth noting.

The rest of the paper is organized as follows: Section 2 surveys the relevant literature. Section 3 gives an historical account of the blockade on Gaza. Section 4 describes the welfare calculations we perform based on consumption and price data, and contrasts them with welfare predictions of an important class of trade models. Section 5 documents changes to production in Gaza following the blockade, focusing on the reallocation of workers and changes to their productivity. Section 6 concludes. The data we used is described in the data appendix.

2. Related literature

This paper contributes to four strands of the literature: the study of historical autarky episodes, the study of the effects of economic sanctions,

the study of the relationship between international trade and productivity, and the study of gains from trade based on quantitative models.

The most closely related literature is the study of historical episodes of moving between autarky and trade. To our knowledge, only two historical episodes in which autarky equilibrium was observed have been analyzed to date, and both are from the nineteenth century. Bernhofen and Brown (2005) examine Japan's forced opening to trade in the 1850s, and find an upper bound of 8% for gains through the channel of comparative advantage. Irwin (2005) explores the self-imposed "Jeffersonian Embargo" in the U.S. between December 1807 and March 1809, and concludes that losses from the embargo in the U.S. amounted to 5% of 1806 GDP. Since in both cases no data on consumption or production is available, these papers use data on prices and on trade flows to estimate bounds on the gains from trade. The contribution of this paper is threefold. First, our welfare calculations are based on household-level data, and not economy-wide aggregates, so our results do not depend on assuming a representative agent. Moreover, household-level data allow us to study the distribution of the welfare changes. Second, having firm-level data allows us to study the adjustment of the production process to being removed from world markets. And finally, an important advantage of this historical episode is that it provides us with a natural "control group"—the West Bank.

Some natural experiments short of a move between full autarky and free trade have also been used to evaluate gains from trade. Feyrer (2009a) uses the closing of the Suez Canal between 1967 and 1975 as an exogenous (for most countries) shock to trade costs, to explore the relations between trade and income. Feyrer (2009b) uses the advancement in air transportation technology, which had a differential effect on countries with short air routes but long sea routes between them, and countries for which both routes are of similar length. Both papers find a substantial and positive effect of trade on income. However, since they analyze relatively small changes, it is not easy to extrapolate from them to the overall gains from trade.

The literature on quantifying the effects of economic sanctions is not large. This is an unfortunate fact, since, as Davis and Engerman (2003) note, their use has "become a standard and routine policy tool of nations and international organizations …". According to Hufbauer et al. (2007), the use of economic sanctions increased in the post Cold War era from 1.8 new sanctions a year in 1945–69 to 3.8 a year in 1970–89, and to 6.3 new sanctions a year in 1990–2000. Hufbauer et al. (2007) also supply some estimates of the welfare costs of economic sanctions imposed by the US. They base these estimates on assumed elasticities of substitution between the banned US goods and substitutes from other countries. This paper is the first analysis of the welfare cost of sanctions based on detailed microeconomic data and a comparison to a counterfactual economy.

The importance of imported inputs for domestic production has been documented in (Amiti and Konings, 2007) and (Topalova and Khandelwal, 2011), who use establishment-level data and find that trade liberalization in India and Indonesia led to productivity increases in domestic firms both through increased competition and through access to imported inputs. Goldberg et al. (2010) also find that greater access to imported inputs led to an increase in the variety of domestically produced final goods, and Yi (2003) uses the importance of trade in inputs, to argue that vertical specialization can explain the large response of trade volume to relatively small tariff reductions. Our results are consistent with these findings, showing that in the extreme case of an almost complete absence of imported inputs, productivity in the manufacturing sector falls substantially.

Lastly, since quantifying the gains from trade is an important question, while natural experiments are rare, another strand of the literature uses quantitative trade models in order to evaluate these gains without observing autarky. One of the most commonly used frameworks is the one developed in Eaton and Kortum (2002). Based on their model, they calculate the gains from trade, and find remarkably low gains ranging from 0.2% for Japan, to 10.3% for Belgium. Though these gains seem

surprisingly small, many other quantitative models predict gains that are no larger. Arkolakis et al. (2012) have shown that an important class of trade models, including some variations of Melitz (2003), Krugman (1980) and the Armington (1969) models for example, all yield the same results for the overall gains from trade, conditional on two sufficient statistics: the share of imports out of total expenditure, and the elasticity of imports with respect to variable trade costs. In section 4 we discuss in detail how our results fit into this literature.

3. The blockade on Gaza

This section gives a brief historical outline of the blockade on Gaza, focusing on Gaza's relationship with the West Bank, and the direct economic effects of the blockade.

3.1. The political background of the blockade on Gaza

The relevant parts of the history of Gaza and the West Bank can be divided into two periods: The Oslo Accord period between 1993 and 2005, and the years after the Israeli withdrawal from Gaza in 2005.

In 1993, after 26 years of direct Israeli control, the Oslo Accord was signed. Under the terms of the agreement, the newly created Palestinian National Authority (PNA), assumed control over most civilian matters in both the West Bank and Gaza, while the Israeli authorities maintained control over security issues.[1] This arrangement remained in force until September 2005, when the Israeli army completed a unilateral withdrawal of all military forces from the Gaza Strip, and the evacuation of about 8000 Israelis who lived in settlements there, effectively drawing a border between the Gaza Strip and Israel. From that point, the Gaza Strip was under the complete control of the PNA, while in the West Bank, the PNA was limited to dealing only with civilian matters, as before. The events that led directly to the blockade began unfolding in January 2006, when internal political tensions between the religious Hamas and the secular Fatah movements culminated in a de facto division of Palestinian government into a West Bank-based Fatah government governing the West Bank, and a Gaza based Hamas government governing Gaza. Alarmed by the rise to power of the Hamas in Gaza, on September 19th 2007 the Israeli government passed decision B/34, which declared the Gaza Strip a "Hostile Territory", and ordered the Israeli Defense Force to impose restrictions on the movement of goods and people into and out of the Gaza Strip, allowing for humanitarian considerations. The Egyptian authorities of the time, also alarmed by the rise of Hamas to power, cooperated, and closed the land crossing between the Gaza Strip and Egypt. The beginning of the blockade can therefore be dated to September 2007, and there is no reason to believe it was anticipated.

3.2. The economic background of the blockade on Gaza

From the beginning of the decade until 2005, the Gaza Strip was an open economy with effectively a fixed exchange rate, since it did not issue its own currency but used the New Israeli Shekel (NIS). Imports averaged 35.6% of total expenditure between 2000 and 2005, and exports were much smaller, equal on average to about 10% of imports during those years. The large trade deficits of the Gaza Strip were funded using three sources. First, unilateral transfers from the West Bank, from UN agencies, and from other donor countries; second, remittances from Palestinians working abroad; and third, foreign direct investment. No data regarding the exact size of each of these components exist. At the time the blockade was imposed, the GDP in Gaza was 1.43 billion USD, and the population was 1.35 million people.[2]

The details of the restrictions on movements of goods across the border were not always clear: Some are secret, and there are conflicting reports from various interested parties about others. At any rate, what is not disputed is that following decision B/34, exports from Gaza were essentially eliminated and non-energy imports were greatly reduced: According to Palestinian National Accounts, exports of goods from Gaza totaled USD 0.6 million in 2009, down from an average of USD 52.8 million per year in 2005–2006, and non-energy imports decreased by 75%, from an average of USD 482 million per year in 2005–2006 to USD 129 million in 2009.[3] Fig. 1 summarizes imports (excluding energy) and exports for Gaza for the years 2005–2011.

Since imports declined much more than exports, the closing of the Gaza Strip led to a direct loss of available resources of USD 480 million between 2006 and 2009. However, this decline was matched by a very similar decline of USD 491 million in gross investments during these years. Thus, the combined effect of the decline in the trade deficit and the decline in investment, if anything, left more resources available for consumption in 2009 relative to 2006, though the magnitude of this change is miniscule—less than USD 2 per household per month. We address intertemporal considerations later in the paper, but for now it is important to note that the loss of available resources due to a fall in the trade deficit can be accounted for by the decline of investment, and it did not have an effect of having a smaller amount of resources available for consumption.[4]

Between December 27, 2008 and January 18, 2009, following an escalation of rocket launching from Gaza into Israel, the Israeli army launched a series of strikes in the Gaza Strip that were later known as "Operation Cast Lead". These strikes led to some disruption in the economy, though reports about the extent of the damage vary widely by source. Fig. 2 gives two types of evidence showing that the macroeconomic effect of the strikes was short-lived. First, the strikes had no effect at all on prices in Gaza, especially relative to the very noticeable effect of the blockade. Second, while the effects of the three weeks of clashes do show in quarterly GDP data, there is no evidence of any lingering effect.

Other than operation "Cast Lead", the years of the blockade, and especially 2009, were relatively quiet. One measure—casualties data, compiled by the B'tselem NGO, show that the number of Palestinian casualties in Gaza in 2009 was 42, much lower than the 525 casualties in 2006.[5] These numbers provide further evidence that other than the events of operation Cast Lead, the level of conflict intensity in 2009 was lower than in 2006.

The blockade was first eased in February 2009, when Israeli authorities expanded the list of goods that were allowed to be imported into Gaza, while still banning all exports. A major change in policy came in June 2010, when, following a violent clash between Israeli commandos and political activists on a Gaza-bound Turkish flotilla, diplomatic pressure led Israel to ease restrictions very substantially, though certainly not completely, and thus effectively end the blockade. Again, there is no reason to believe that this was anticipated.[6]

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[1] Israel also maintained control of large parts of the area which was not inhabited, and over all of the area where Israeli settlements were located.

[2] According to data from the PCBS.

[3] The reason we do not include energy imports in this calculation is that a large share of total imports to the Gaza Strip consists of fuels and electricity, which by a decision of the Israeli Supreme Court from July 2007 in Albasioni Ahmad and Others v. The Prime Minister and Defense Minister of Israel (case number 9132/07), were either not restricted at all (electricity) or restricted very slightly (fuels).

[4] We also calculate the loss of return on lost investments. We use the average of interest rates to prime borrowers and of the interest rate in interbank lending in the West Bank and Gaza from the CIA World Fact Book (no data is available for Gaza by itself) as a proxy for annual return, even though they reflect the risk-adjusted marginal return on investment, not the average, risk-free return. This may lead to an overstatement of foregone returns. Aggregating the lost returns on all the lost investments in 2007–2008, yields a loss of USD 26 million for the year 2009. Subtracting that from the difference between the fall in the trade deficit and the fall in investment, the effect of the loss of resources on consumption per household is essentially zero.

[5] The data contain the names and circumstances of death of individuals who died as a direct result of the conflict.

[6] The most notable change was a switch from a "white list" of goods which were allowed in, to a "black list" of goods which are not. Exports were also allowed again, though they were still restricted.
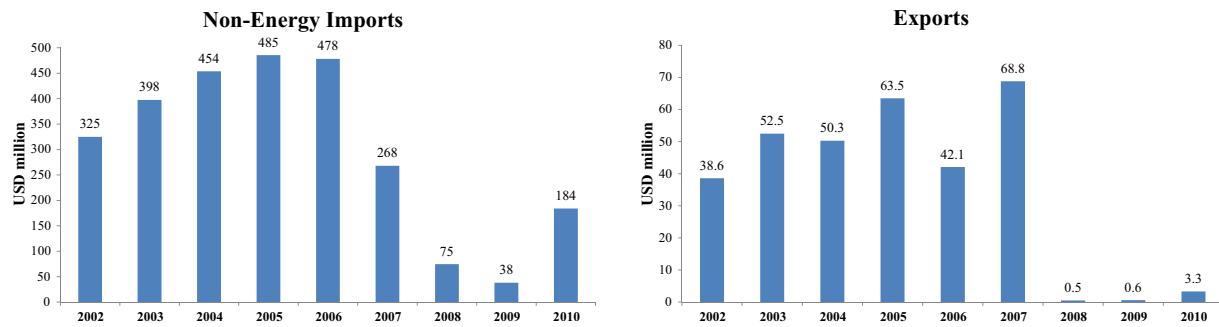
Fig. 1. Exports and Non-Energy Imports, Gaza 2002–2010. Notes: Data are in current USD million, taken from national accounts published by the PCBS. Data on energy imports is from trade data released by the PCBS, and includes electricity and fossil fuels. Since some food imports were still allowed, imports are still positive during the years of the blockade.
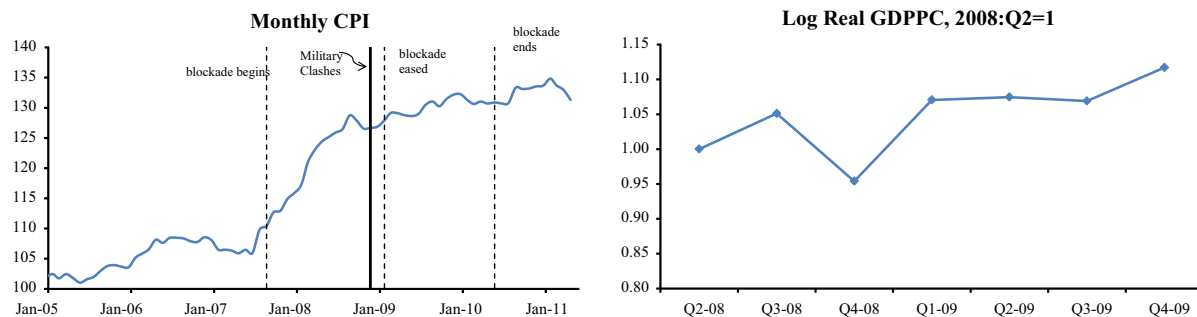

Fig. 2. Gaza CPI and Real GDPPC During Operation Cast Lead. Notes: Monthly price data is from the CPI files of the Palestinian Central Bureau of Statistics. Real GDP per Capita by quarter is from the Palestinian national accounts, and is normalized to 1 for the second quarter of 2008. All of the depicted period in the GDP graph takes place during the blockade.

As the fact that imports were not zero suggests, the blockade was never perfectly enforced. Food, medical supplies, and some humanitarian equipment (eg. blankets, diapers) were allowed into Gaza, though under at least some restrictions, throughout the whole period. On January 23, 2008 a part of the fence separating Gaza from Egypt was brought down, and for a few days there was substantial movement of people between the Gaza Strip and Egypt. Lastly, the smuggling industry was active, most notably through the use of underground tunnels between the city of Rafah in the south of the Gaza Strip and the Egyptian city by the same name across the border. No credible data about the magnitude of trade that went through the tunnels exists. A report by the U.S. Congressional Research Service (see Zanotti, 2010) mentions estimates of a 1000 tunnels at the end of 2010, but supplies no details of their size or the value of trade that goes through them. The Israeli newspaper "Haaretz" (see (Hess, 2008)) reported at the end of 2008, that there were 850 tunnels registered with the Palestinian police in Gaza, though only 400 of them were paying the fixed tunnel fee of USD 2500 per year, suggesting that a large fraction of tunnels were very small. However, the ability of even a few hundred tunnels, many of them with extremely limited capacity, to substitute for open borders is limited, considering that before the blockade an average of 10,000 trucks entered Gaza each month.[7] At any rate, if imports into the Gaza Strip were even higher than official data report, then estimates of the welfare costs of the decrease in trade volume are biased downward.[8]

Fig. 3 gives a bird's-eye view of the economic effects of the blockade, showing trends in real GDP per Capita in Gaza and in the West Bank from 2002 to 2010. As can be seen, the blockade was imposed on the Gaza Strip following 4 years of growth that began after the end of the most intensive days of the second intifada in 2002, and it was associated with a substantial economic downturn, while in the West Bank the

previous trend continued. Following the easing, and later the removing, of the blockade, GDP per Capita in Gaza began to grow again.

4. The welfare cost of the blockade

We turn now to perform three calculations for the welfare loss that resulted from the blockade on Gaza. The first uses Slutsky's compensating variation (CV) concept, the second uses the Hicks CV, and the last calculation uses the Slutsky equivalent variation (EV). These three calculations are, respectively, an upper bound, an approximation, and a lower bound for the welfare losses in Gaza. When expressed as a percentage of the base period value of consumption, these measures can be interpreted as the change in real consumption, which is the concept that is used as a welfare measure in the ACR formula, and in (Ossa, 2012), to which we will compare our results. The data we use are from the Household Expenditure Survey, which is a repeated cross-section documenting expenditure by households on several hundred items, and from the micro data of the Palestinian CPI. See the online data appendix for a complete description of the data and associated issues.

4.1. Using consumption as a measure for welfare

Using household consumption data as a measure for welfare allows us to make welfare calculations at the household level by creating a household-specific price index, and accounting for substitution effects. Since the weights in the CPI as published by the PCBS did not change during the blockade, this is an important advantage over using aggregate real consumption data from the national accounts. Moreover, the use of micro data reduces concerns about data credibility: While national accounts numbers, as published by the Palestinian Central Bureau of Statistics, can be easily manipulated for political gain, it is much harder to manipulate expenditure surveys, which include many hundreds of households (each consuming up to hundreds of items), and the price data, which include hundreds of individual item prices. However,

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[7] Reported by the Gisha NGO, based on data collected by UNSCO.
[8] The PCBS refused to answer whether the import data does or does not include imports through tunnels.
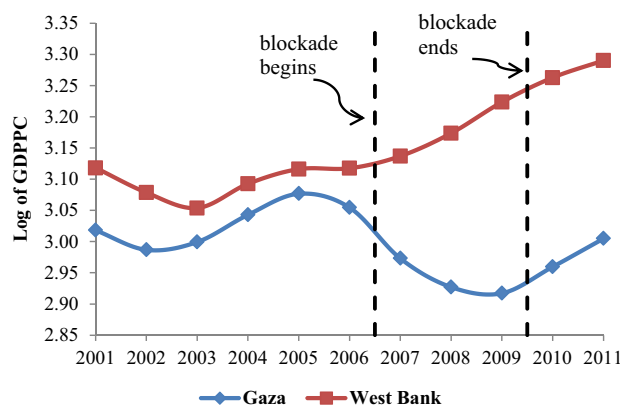
Fig. 3. Real GDPPC, Gaza and the West Bank. Notes: Each point in the graph is the natural log of real GDPPC, with 2004 as the base year for prices. The data are taken from the Palestinian national accounts and population statistics published by the PCBS.

these advantages come at a cost: Relying on consumption alone as a measure for welfare ignores changes in savings. If, for example, saving rates increased, the decline in the value of consumption will overstate the real welfare effects of the blockade. Yet, we do not believe that these considerations change our results in a quantitatively important way. First, we note that replicating the kind of calculations we perform, but based on real GNI per Capita data, we get an even larger welfare loss than the upper bound of our calculation which is based on household expenditures (see Table 2, and the discussion there). By this measure, if anything, our results may understate true welfare loss. Second, it is unlikely that savings rates for households in Gaza were higher in 2009 than in 2006—the two years we compare. In a standard dynamic optimization framework, an increase in the savings rate during the blockade can be a result of either an increase in the return on investment (or equivalently, a decline in the relative price of investment goods), or of expectations of further decline in incomes (or both). Neither one is realistic in 2009. Domestic investment in Gaza all but stopped during the blockade, declining by 80%, and investment opportunities outside of Gaza during 2009, if at all available for Gazans, with interest rates around the world at historic lows, were no better in 2009 than in 2006. Thus, there is no reason to believe that savings rates increased because of improved investment opportunities. As for expectations in 2009 for even worse future incomes—it is again unlikely. By 2009 the fall in GDPPC stopped, and modest growth began, especially following a limited easing of the blockade. It seems highly unlikely that facing improved conditions, Gazans' expectations deteriorated. We therefore conclude that changes to savings rates are not likely to have introduced an upward bias to our measure of changes to welfare.

4.2. Methodology: calculating welfare losses

We start with calculating the Slutsky Compensating Variation (CV). Formally, it is defined as

$$CV_i^s = e\left(p^a, c_i^t\right) - e(p^a, c_i^a)$$

where $j \, \epsilon \, \{a, t\}$ refers to autarkic[9] and trading conditions respectively, and $e(p^j, c_i^j)$ is the cost of consumption bundle c of household i when prices are given by the vector p^j.[10] In words: how much more money

a household in blockaded Gaza needs in order to be able to purchase the same bundle it did prior to the blockade. While this measure avoids the need to make assumptions about household preferences, it also ignores the ability of households to substitute for goods that became relatively more expensive. It thus tends to overstate welfare loss, and is best interpreted as an upper bound on it.

To account for possible substitutions in the consumption bundle, we also calculate the Hicks CV:

$$CV_i^h = e\left(p^a, u_i^t\right) - e(p^a, u_i^a)$$

where u_i^j is the utility of household i under trading conditions $j = \{a, t\}$. In words: how much more money a household in blockaded Gaza needs to be *as well off* as it was before the blockade. In theory, the Slutsky CV is always weakly larger than the Hicks CV. In practice, since relative prices changed, the Slutsky CV is strictly greater than the Hicks CV in all of our samples.

To calculate the Hicks CV, it is necessary to specify a utility function,[11] and we assume a Cobb–Douglas utility function for all households, $U_i = \prod_k \left(c_{k,i}\right)^{\alpha_{k,i}}$ with $\sum_k \alpha_{k,i} = 1$. The Cobb–Douglas utility function imposes a unit price elasticity which, given the level of disaggregation of the expenditure survey, is a reasonable assumption (Deaton and Meullbauer, 1980, p.79). However, Costinot and Rodriguez-Clare (2014) note that under extreme conditions, if some prices become arbitrarily large, a Cobb–Douglas utility will yield infinite gains from trade. Thus, we also report the results of using a calibrated CES utility $U_i = \left(\sum_k \beta_{k,i} c_{k,i}^{\frac{\sigma-1}{\sigma}}\right)^{\frac{\sigma}{\sigma-1}}$, with $\sigma > 1$ being the elasticity of substitution, and $\sum_k \beta_{k,i} = 1$.

Finally, we also calculate the Slutsky EV, defined as:

$$EV_i^s = e\left(p^t, c_i^t\right) - e\left(p^t, c_i^a\right)$$

In words: the amount of money that can be taken away from a household in pre-blockade Gaza so that it will only be able to afford the consumption bundle it had during the blockade. This, as explained below, is a lower bound on welfare loss.

4.3. Using a synthetic panel

Since the household data we have are not a panel, we do not observe the same household both before and during the blockade, and we cannot directly calculate any of the measures of welfare loss, as defined here, at the household level. In order to use as much data at the household level as possible, we do the following: First, it is instructive to rewrite the CV_i^s as the sum of two differences, by adding and subtracting $e(p^t, c_i^t)$:

$$CV_i^s = \left[e\left(p^a, c_i^t\right) - e\left(p^t, c_i^t\right)\right] + \left[e\left(p^t, c_i^t\right) - e\left(p^a, c_i^a\right)\right]$$

The first difference is the increase in the cost of the pre-blockade consumption bundle when moving from a trading to a blockaded economy. The second difference is the change in the total value of the household consumption bundle when moving from trade to autarky. Since we observe both p^t and p^a, the first difference is observed at the household level, and it uses all of the data about household consumption.[12] However, since we do not observe the same household under both trading and blockaded conditions, we do not observe the second difference.

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[9] For the rest of this section, for purposes of clarity and brevity, we will refer to the state of the Gaza Strip during the blockade as autarkic. As mentioned before, the Gaza Strip was not completely autarkic during this time.

[10] Note that this is not the standard definition of the expenditure function. The standard expenditure function is defined over prices and utility level, and it calculates the cost of obtaining a given level of utility under a given vector of prices. Under the definition used here, $e(p^j, c_i^j)$ is simply $p^j \times c_i^j$. We keep the function notation to make the comparison to the Hicks CV simpler.

[11] Technically, one only needs to specify an expenditure function. This distinction is not critical to the discussion here.

[12] This decomposition also clarifies why the Slutsky EV is a lower bound. The difference between the Slutsky EV and the Slutsky CV is only that the Slutsky EV calculates the change in the cost of the autarky consumption bundle, while the Slutsky CV calculates the change in the cost of the free trade consumption bundle. This is exactly the difference between the Paasche price index, which is a lower bound on the change to the cost of living, and the Laspeyres price index, which is an upper bound.
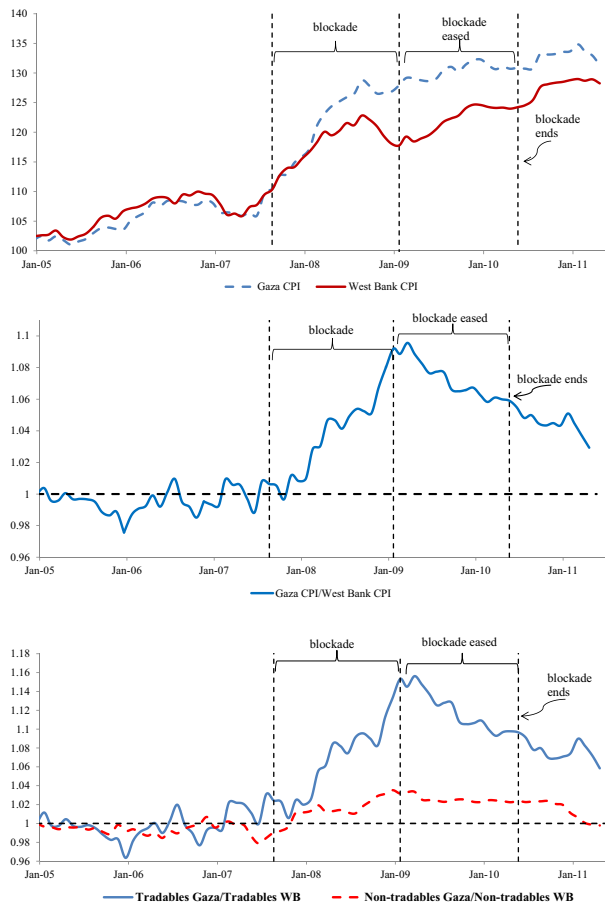
Fig. 4. CPI in Gaza and the West Bank, 2005–2011. Notes: Monthly price data is from the CPI files of the Palestinian Central Bureau of Statistics, using the Gaza weights for both Gaza and the West Bank.

Lack of panel data is a common problem with consumption surveys. See Dang and Lanjouw (2013) and the literature surveyed there. Following Deaton (1985), we construct a "synthetic panel", using households of different sizes as "cohorts". That is, we place all households from both the pre-blockade and the during-blockade sample into bins based on the number of people in the household, and then give each household, in each bin, the average change to the value of consumption in the bin to which it belongs. We use exactly the same procedure for the Hicks CV and for the Slutsky EV. It is important to note how the use of this synthetic panel affects the interpretation of the results. First, because changes in the total value of expenditure enter our calculations additively, the averages we report are not affected by the absence of panel data. However, the distributional results are affected. The correct interpretation for the welfare loss we calculate for household i is therefore the amount of money household i would need to be as well off as it was before the blockade, *assuming its total expenditure change was the average of such change for households of the same size*. Thus, our distributional results do capture the differential welfare effects of the blockade due to different consumption bundles, but only capture the distributional effects that are due to expenditure level changes to the extent that household size is a robust predictor of these changes.[13] Since household size was correlated both with the total size of expenditure before the blockade, and with expenditure per capita, it is likely that this process improves the precision of our distributional results relative to using only the population average change in total expenditure.

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[13] Unfortunately, the very reason we have to use a synthetic panel is the reason our data cannot tell us to what extent it is indeed the case.
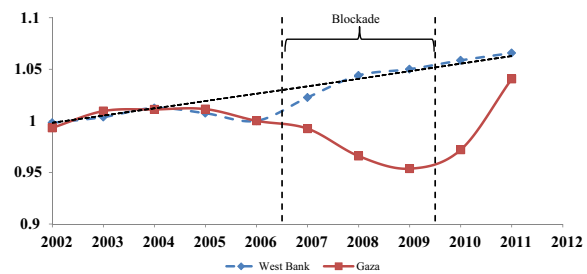

Fig. 5. Real consumption per capita in the West Bank and Gaza, 2002–2011 (2006 = 1). Notes: Data on consumption and population are from the Palestinian Central Bureau of Statistics, and are in logs of USD 1000. The levels of both the West Bank and Gaza are normalized to 1 in the year 2006, the last year before the blockade.

Lastly, as Helpman and Krugman (1985) clarify, the autarky-free trade framework is not about comparing an economy before and after it opened (or closed) to trade. Rather, it is a counterfactual exercise, comparing the state of the economy if it could trade, to its state if it could not. For our calculations, it means that c_i^t and u_i^t should not be the consumption bundle and utility level before the blockade, but rather what they would have been had the blockade never taken place. Specifically, in the case of the blockade on Gaza there is a concern that some of the changes in prices and expenditure that we observe are the result of unobserved trends that are unrelated to the blockade. Moreover, since some imports, especially food, were allowed into Gaza even during the blockade, some of the price changes we observe may reflect, at least in part, an increase in world prices and not the effects of the blockade. To address these concerns, we perform a "difference-in-difference" calculation, using the West Bank as a counterfactual economy for the Gaza Strip. To use the West Bank as a counterfactual, we calculate the Slutsky CV for each household as follows: To calculate the increase in the cost of the consumption bundle we use the extra increase in its cost in Gaza relative to the increase in the cost of an identical bundle in the West Bank, and for the change in the value of household expenditure we calculate the change for a household in Gaza relative to the change for a similar household in the West Bank. Formally, the Slutsky compensating variation is calculated as:

$$CV_i^s = \left\{ \left[e\left(p_g^a, c_i^t\right) - e\left(p_g^t, c_i^t\right) \right] - \left[e\left(p_{wb}^a, c_i^t\right) - e\left(p_{wb}^t, c_i^t\right) \right] \right\}$$
$$+ \left\{ \left[e\left(p_g^t, c_{i,g}^t\right) - e\left(p_g^a, c_{i,g}^a\right) \right] - \left[e\left(p_{wb}^t, c_{i,wb}^t\right) - e\left(p_{wb}^a, c_{i,wb}^a\right) \right] \right\}$$

where $p_i^a \{i = g, wb\}$ is the price vector for the West Bank and Gaza in 2009, $p_i^t \{i = g, wb\}$ is the price vector for the West Bank and Gaza in 2006, $c_{i,g}^t$ and $c_{i,g}^a$ are the consumption bundles of household i in Gaza in 2006 and 2009 respectively, and $c_{i,wb}^t$ and $c_{i,wb}^a$ are the consumption bundles that household i would have had had it been located in the West Bank during the years 2006 and 2009. Again, the exact same procedure is used for the Hicks CV and the Slutsky EV.

4.4. Using the West Bank as a counterfactual economy

Since the two key variables in our welfare calculations are prices and household consumption, similar trends in these variables are necessary conditions for the West Bank to be a good counterfactual economy for Gaza. Fig. 4 shows monthly data on the CPI[14] in Gaza and in the West Bank between 2005 and 2011. The top Panel shows the CPI for Gaza and for the West Bank, the middle panel shows the ratio of the CPI in Gaza to the CPI in the West Bank, and the bottom panel shows this ratio separately for the tradable and the non-tradable components of the CPI.

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[14] To avoid problems that may arise from the use of different weights in the calculation of the CPI in the West Bank and Gaza, we recalculate the West Bank CPI using the Gaza weights. In practice, it makes very little difference.

Four patterns in the data suggest that the West Bank is indeed a good counterfactual economy. First, before the blockade on Gaza, price indices in the West Bank and in Gaza followed a very similar path. Second, soon after the blockade began, a noticeable gap opened. Third, after the blockade was eased, the indices began to converge again. Fourth, the divergence in prices is much stronger for tradable goods than for non-tradables. Put together, the first three facts suggest that the West Bank price trends are a good counterfactual economy for Gaza price trends. The last two facts, showing that the divergence in prices happened almost only in tradable goods, and started reversing after the blockade was eased, strongly supports the claim that this divergence is indeed due to the blockade.

Fig. 5 shows annual data on real consumption per capita since 2002 in the Gaza Strip and in the West Bank, both normalized to one in 2006, the last year before the blockade.[15]

Here too, three patterns in the data, that were also present in the price data, suggest the West Bank is a good counterfactual economy: The trends were similar before the blockade, diverged sharply during the blockade, and after the blockade was eased the trends began converging again. This figure also underscores the importance of using the West Bank as a counterfactual economy: In 2006 there was a dip in consumption but it was essentially identical in the West Bank and in Gaza. While the West Bank recovered almost immediately, consumption in the Gaza Strip didn't start converging back to its historic trend until after the blockade was eased.

Not only total consumption was similar between the West Bank and the Gaza Strip, but also consumption patterns. Table 1 details average expenditure shares on each of 23 standard consumption categories in the Gaza Strip and the West Bank in 2006. The similarity is striking: The correlation between expenditure shares in the West Bank and Gaza was 0.95 in 2006, and 0.86 in 2009, and that in spite of the fact that nominal expenditure in Gaza was 22% lower than in the West Bank in 2006, and 28% lower in 2009.[16] Moreover, changes in expenditure shares over time were also limited: The correlation between expenditure shares in 2006 and 2009 is 0.92 in Gaza and 0.97 in the West Bank. These patterns are consistent with similar and homothetic preferences between the West Bank and Gaza. This is important since our calculation uses growth rates of consumption in the West Bank, and applies them to the levels of consumption in Gaza. This is only valid if we assume that growth rates have similar effects at different levels of consumption, which is the case under homothetic preferences.

A final concern with using the West Bank as a counterfactual economy is that it is possible that the West Bank was also affected by the blockade. Since Gaza was a trade partner of the West Bank, it is possible that loss of access to that market led to higher prices, and lower growth in the West Bank as a result of the blockade. Both of these effects will bias our estimates of the losses in Gaza downward. On the other hand, if the West Bank was in competition with Gaza in other markets, the West Bank may have benefited from the elimination of this competition, and this would have led to higher growth in the West Bank.

While it is possible that the blockade on Gaza had some effect on the West Bank, it is unlikely that it was substantial. Total exports From Gaza in 2005–2006 was less than 3% of total imports into the West Bank during these years. Even if the West Bank were the only destination for Gaza exports, which it clearly was not, losing Gaza as a source for imports would have had at most a minimal effect on the West Bank. It is even less likely that eliminating Gaza as a competitor had significant effects on demand for exports from the West Bank. The total value of goods exported from Gaza in 2005–2006 was about 11% of the value of the goods exported from the West Bank, and the overlap in the type of goods exported is limited, with Gaza's biggest export being

Table 1
Expenditure Shares in the West Bank and Gaza.

	2006		2009	
	West Bank	Gaza	West Bank	Gaza
Bread and cereals	5.7%	7.1%	6.8%	7.0%
Meat and poultry	9.8%	10.6%	9.8%	11.2%
Fish and sea products	0.6%	1.3%	0.8%	1.4%
Dairy products and eggs	3.6%	3.1%	3.4%	2.9%
Oils and fats	1.3%	2.3%	1.3%	1.8%
Fruits and nuts	2.9%	3.9%	2.9%	4.3%
Vegetables, legumes and tubers	4.8%	6.6%	4.7%	6.7%
Sugar and confectionery	2.1%	3.3%	2.0%	2.5%
Non-alcoholic beverages	1.8%	1.3%	1.9%	1.6%
Salt, spices and other foods	2.0%	2.6%	1.9%	2.4%
Take away food and meals in restaurants	2.4%	2.6%	2.7%	3.1%
Own produced food	1.7%	0.8%	2.2%	0.5%
Total food	38.7%	45.6%	40.4%	45.4%
Clothing and footwear	7.1%	7.4%	7.0%	9.8%
Housing expenditure	10.9%	8.9%	10.0%	8.9%
Furniture and utensils	5.1%	4.6%	4.3%	4.3%
Household operations	1.8%	1.8%	1.6%	2.1%
Medical care	5.8%	4.1%	4.4%	6.3%
Transportation	11.5%	10.1%	13.6%	7.8%
Communication	4.1%	4.0%	3.9%	4.3%
Education	4.6%	3.8%	2.3%	1.7%
Recreation	2.2%	2.1%	2.8%	3.4%
Personal care	2.6%	3.2%	5.4%	2.2%
Tobacco	5.5%	4.3%	3.9%	2.6%
Alcoholic beverages	0.0%	0.0%	0.0%	0.0%

Notes: Data on consumption are from the Household Expenditure Survey by the PCBS.

furniture, while the West Bank specialized mostly in textiles. Moreover, exports during these years were 15% of GDP in the West Bank. Even if demand for West Bank exports increased by the full amount of the decrease in exports from Gaza, which is highly unlikely, it would have had a very small effect on growth in the West Bank.

4.5. Results: total welfare losses and distributional effects

The results for the calculations described above are reported in Table 2. The welfare loss as a result of the blockade was indeed very large: Using the West Bank as a counterfactual economy, the average Slutsky CV for a household in Gaza is equal to 27% of the 2006 value of consumption, and the average Slutsky EV is 14%. The average Hicks CV depends, naturally, on the elasticity we use. Rows 2 and 3 in Table 2 report two benchmark values: for a constant unit elasticity, i.e. Cobb–Douglas utility, and for using a single tier CES utility, with an elasticity

Table 2
Compensating variation for Gaza 2006–2009, using the West Bank as a control group for both years.

	Mean	Median	Least affected 10%	Most affected 10%
Percent of 2006 expenditure				
Slutsky CV	27%	26%	16%	39%
Hicks CV–Cobb–Douglas	23%	22%	13%	34%
Hicks CV–CES ($\sigma = 1.35$)	21%	20%	11%	32%
Slutsky EV†	14%	14%	3%	24%
GNI PC	33%	*	*	*
Shekels (per month)				
Slutsky CV	511	392	156	922
Hicks CV	430	323	124	762
Hicks CV − CES ($\sigma = 1.35$)	393	294	105	717
Slutsky EV†	292	212	40	604
GNI PC	625	*	*	*

Notes: †The Slutsky EV calculation is based on a sample of households from 2009, unlike the Hicks and Slutsky CV which are based on the 2006 sample. Data on consumption is from the 2006 household expenditure survey, and data on prices is from the 2006 and 2009 Household Expenditure Surveys, and from the CPI data. All asterisks in this table mean that the column is irrelevant for that row. That is, for the row GNI PC there is no value to report for the columns Median, Least affected 10% and Most affected 10%.

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[15] Throughout the whole period, the West Bank had a higher level of consumption.
[16] To eliminate the concern that this correlation stems from a few categories that are very large, we also calculate the Spearman rank correlation, and get a correlation of 0.92 for 2006 and 0.89 for 2009.

Table 3
Compensating variation and total expenditure.

	Slutsky CV	Slutsky CV–Price Effect	Hicks CV	Hicks CV–Price Effect	Slutsky EV	Slutsky EV–Price Effect
Total expenditure	0.03***	0.03***	0.02***	0.03***	0.05***	0.05***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Food share	− 0.19***	− 0.20***	− 0.16***	− 0.18***	− 0.13***	− 0.11***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Number of persons in the HH	Yes	Yes	Yes	Yes	Yes	Yes
Type of locality dummy	Yes	Yes	Yes	Yes	Yes	Yes
N	419	419	419	419	1175	1175

Notes: Expenditure is in logs. Type of locality: urban, rural, or a refugee camp. Food share is the share of items in groups 1–10 in the expenditure survey, which is the definition used by the Palestinian Bureau of Statistics for food expenditure. Standard errors are in parenthesis. *** = P-value < 0.01

of $\sigma = 1.35$, which we estimated from the expenditure and price data (see Appendix B for a detailed explanation of the estimation). The results are a mean Hicks CV of 23% and 21% respectively.

Using real GNI per capita data yields a welfare loss equal to 33% of pre-blockade GNI per capita, larger than the Slutsky CV measure, which is an upper bound in our methodology. There are a few possible reasons for this, aside from data credibility. First, we note that since there was no change to the weights in the price index used for the calculation of the real GNI, the most relevant comparison is to our upper bound. Second, the numbers we report are a simple average over households, i.e. all households have the same weight in the average. Since, as we describe in detail in the next section, wealthy households lost disproportionally more, the aggregate data is expected to show a larger welfare loss than a non-weighted average of households.

The results also reveal a substantial variation in the size of welfare losses across households. The most affected 10% of households lost between 2.5 times (according to the Slutsky CV) and 8 times (Slutsky EV) as much as the least affected households. These patterns lend strong support to the fundamental idea in trade theory that trade creates "winners" and "losers", or at least bigger and smaller winners.

The relationship between international trade and inequality in developing economies is a topic of great interest.[17] Having data at the household level enables us to gain some insight about the distribution of the welfare losses that resulted from the blockade on Gaza. We test the correlation of welfare losses and the level of expenditure, by estimating the following regressions:

$$C_i^j = \alpha + \beta_1 \text{EXP}_i + \beta_2 Z_i + \epsilon_i$$

where $C_i^j \in \{CV_i^s, CV_i^h, EV_i^s\}$ is the Hicks CV, Slutsky CV, or the Slutsky EV for household i, expressed as a percentage of 2006 expenditure.[18] EXP_i is the natural log of total value of expenditure in 2006 by household i, and Z_i are household characteristics such as the number of persons in the household, and whether it is from a rural area, urban area, or a refugee camp. An important control variable for this regression is the share of total expenditure that is spent on food. Since Israeli authorities allowed some food items to enter the Gaza Strip even during the blockade, and poorer households spend a larger share of their income on food, this fact can lead to smaller welfare losses in poor households relative to rich households. The variation in the size of welfare loss can come from two sources: different changes to the household value of consumption, and different changes to the cost of the household consumption bundle (or the cost of its utility level). We therefore run another set of regressions, with the change to the cost of the household consumption bundle (or the change in the cost of its utility level) as the dependent variable, instead of the full welfare loss. Formally, for each household, we denote the difference in the cost of the pre-blockade

consumption bundle when the prices changed to autarky prices as $\Delta e_i^{cvs} \equiv e(p^a, c^t) - e(p^t, c^t)$, the change in the cost of the pre-blockade utility level as $\Delta e_i^{cvh} \equiv e(p^a, u^t) - e(p^t, u^t)$, and the change in the cost of the blockade consumption bundle as $\Delta e_i^{evs} \equiv e(p^a, c^a) - e(p^t, c^a)$. These three variables correspond to the Slutsky CV, Hicks CV, and Slutsky EV respectively, but using only the part of the welfare loss that is caused by the change in the cost of the consumption bundle. The regressions we estimate are:

$$\Delta e_i^j = \alpha + \beta_1 \text{EXP}_i + \beta_2 Z_i + \epsilon_i$$

for $j \in \{cvs; cvh; evs\}$ and all other variables are as before.

The results are reported in Table 3, and across all specifications we find a positive and significant (both statistically and economically) correlation between the value of a household's pre-blockade consumption, and its welfare loss calculated as a percentage of pre-blockade consumption. Each pair of columns reports the results of regressing total welfare loss, and of regressing only the part of the welfare loss that is caused by changes in prices. Columns 1 and 2 report the results for the Slutsky CV, columns 3 and 4 report the results for the Hicks CV, and columns 5 and 6 report the results for the Slutsky EV as the dependent variable. Quantitatively, the results suggest that a doubling of income is correlated with an increase of between 3 and 5 percentage points in welfare loss, which is substantial relative to the average welfare loss of between 14% and 27%.

4.6. Model-based prediction for welfare loss

Because the exporting industries in Gaza relied heavily on imported inputs, the distinction between export goods and import goods is blurry: Export goods from Gaza included a substantial imported component. Therefore, implementing a welfare calculation based on simple comparative advantage models to the case of Gaza, in a similar fashion to Bernhofen and Brown (2005) and Irwin (2005), is not straightforward.

To contrast our results with the predictions of a more general set of trade models, we use the formula from Arkolakis et al. (2012), and the one suggested by Ossa (2012). But before going to the data, it is important to ask if this is a valid comparison. The most important difference between the models and the historical episode of the Gaza blockade is the time horizon. While the Gaza blockade lasted only three years, at least in its strictest form, most quantitative trade models are static, and so refer to the long-run, after all adjustment had taken place. But how long the long-run is is a question of interpretation.[19] The point of this section is therefore not to empirically test the validity of quantitative trade models, but to help with their interpretation. The conclusion to be drawn from the large gap between the model-based predictions and our consumption-based results is not necessarily that the model is

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[17] See Goldberg and Pavcnik (2007) for a review of the empirical literature.

[18] All the results we report here for the Hicks CV use the Cobb–Douglas utility. Results are essentially unchanged when we use other elasticity of substitution.

[19] In calibrating his dynamic model to derive quantitative results, Yi (2003) assumes the rate of adjustment is such that a new steady state is reached after 125 years. Learning this, a colleague remarked, only half in jest, that perhaps current data should be used to evaluate the changes in Japan that resulted from the opening to trade in the 1850s.

"wrong", but that the model does poorly in describing a time horizon of a few years. Yet, we believe it is also misguided to completely dismiss the Gaza experience as too short to have any relevance to long-run predictions. Limiting the interpretation of the long-run in the quantitative models to refer only to the time horizon that will allow a complete reorganization of the entire economy makes their predictions extremely hard to test with any conceivable real world data, and of limited relevance for policy analysis.

We start with the version of the ACR formula that allows for the use of imported inputs. This formula gives the welfare prediction of an important class of trade models, including Armington (1969), Krugman (1980), Eaton and Kortum (2002), and some variations of Melitz (2003), based on three sufficient statistics: the change in the share of total expenditure that is spent on domestically produced goods, the elasticity of imports with respect to variable trade costs, and the share of production cost that is spent on non-tradable goods:

$$\widehat{W} = \widehat{\lambda}^{1/\beta\epsilon}$$

where $\widehat{W}$ is the change in welfare, $\widehat{\lambda}$ is the change in the share of domestic expenditure out of total expenditure, ϵ is the elasticity of imports with respect to variable trade costs, and β is the share of total costs (variable and fixed) that is spent on non-tradable inputs, such as labor. Here we replicate the calculation ACR perform for the US using data from Gaza. For the trade elasticity parameter, we use the range from −5 to −10, the same as ACR use, based on a survey by Anderson and van Wincoop (2004). For $\widehat{\lambda}$, we use the data from Gaza national accounts and trade statistics to get the share of non-energy expenditure that is spent on domestically produced goods. This number changed from 70.3% in 2006 to 93.5% in 2009. While we do not have the data to perfectly measure β, we use a value of $^2/_3$, which is a common assumption for labor share in the cost of production. The result is a predicted welfare loss of 4.4%–8.2%, which is significantly less than the 14%–27% we find based on household consumption data. Since import share in blockaded Gaza is not an optimal choice given trade costs, but rather it is constrained by what is and is not allowed into Gaza, it may be misguided to use the actual change in import share as a measure for $\widehat{\lambda}$. We therefore recalculate the welfare loss predicted by the ACR formula for the case of a complete elimination of imports into Gaza. However, even in that case, the predicted welfare loss is only 10.1%, which can be interpreted as an upper bound on what the ACR method can plausibly predict.[20]

In a recent paper, Ossa (2012) argues that the heterogeneity of the elasticity of trade between different sectors of the economy may lead to gains from trade that are higher than those predicted by the ACR formula. While, as he emphasizes in the paper, the argument is made using a very specific model, the point it makes is likely to be a general one: If the elasticity of trade in some sectors is particularly low, the gains from trade may be high, even if the average elasticity of trade is not very low. Thus, in the setting of Ossa (2012)—an Armington model with intermediate goods and a non-tradable sector—the gains from moving from autarky to observed levels of trade are given by the following formula:

$$\widehat{W} = \lambda^{\frac{\beta}{\gamma}\sum_i \alpha_i \frac{\ln\lambda_i}{\ln\lambda} \frac{1}{\sigma_i - 1}}$$

where $\widehat{W}$ is the change in welfare, λ is the aggregate share of domestic production out of total expenditure, λ_i is the share of domestic
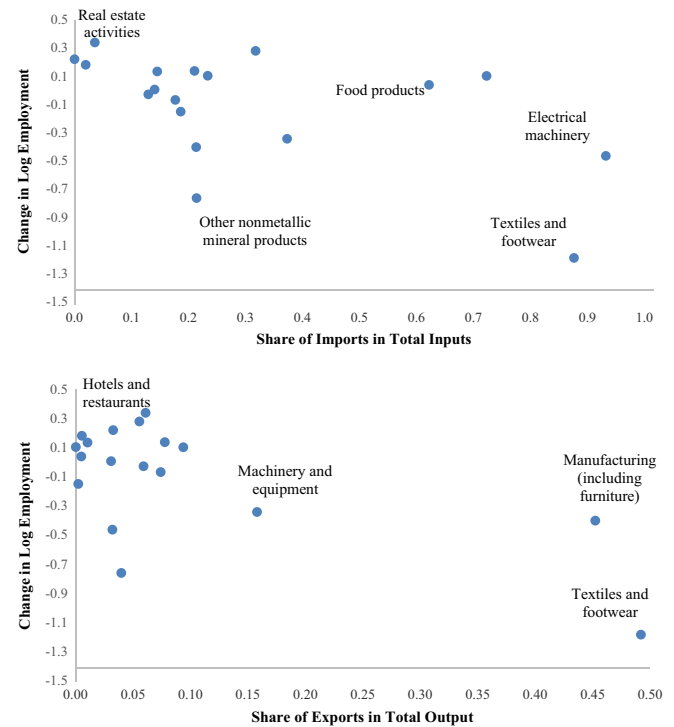

Fig. 6. Labor reallocation 2006–2009 and trade intensity. Notes: Import share of inputs and export share of output are calculated with 2006 data, and they take into account imports embedded in purchases from local upstream industries, and exports embedded in sales to local downstream industries. Employment data are based on the change between 2006 and 2009. The figures exclude industries which had less than 5 firms operating in 2006. Data is from the manufacturing and services surveys.

production out of expenditure in sector i, $\frac{1}{\sigma_i - 1}$ is the inverse of the industry level trade elasticity, α_i is industry expenditure shares out of total expenditure in the traded sector, and β and γ are, respectively, the share of the tradable sector value added in GDP and in gross production. In Data Appendix C we describe in detail the way we construct each variable in this formula. One important limitation of the available data is that data on industry level imports into Gaza in 2009 are incomplete.[21] We therefore calculate the predicted welfare loss of moving from 2006 trade levels to complete autarky, which may also be justified for the same reasons we gave before when using the ACR formula. According to this formula, the expected welfare loss of moving from 2006 trade levels to complete autarky is 24.3%. While this number is very similar to the results we get from the expenditure-based calculations, the same considerations that lead us to caution against interpreting our results as evidence against the ACR formula also apply here. However, the results from Gaza do support the general point Ossa (2012) is making: Heterogeneity of trade elasticities causes the large divergence between using Ossa's formula and the ACR formula for Gaza. If each of the goods imported into Gaza had the aggregate trade elasticity, the gains predicted by the Ossa formula shrink to only 12.8%—only marginally higher than the ACR prediction. However, heterogeneity matters: In the two industries with the highest trade elasticities (agriculture and textiles, with $\sigma_{agg} = 27.6$ and $\sigma_{tex} = 13.1$), Gaza had relatively high domestic production shares (71% and 64% respectively), and so the average trade elasticity understates the importance of trade.

5. Production

To gain a better understanding of the sources of welfare loss in Gaza, we turn now to look at changes to production there. The goal of this

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[20] In contrasting the results of the ACR formula with those calculated based on data from Gaza, it is important to note that the ACR formula is only applicable to models where trade is balanced, while the Gaza economy was far from balanced. However, all the models that the ACR formula deals with are static models, and the balanced trade assumption is not an assumption about intertemporal considerations or capital flows, which were certainly important in Gaza, but about the way trade costs are allocated between importing and exporting countries. That is, the assumption in ACR is not that trade is balanced every period, which is clearly not the case in Gaza, but that trade costs are borne in a specific way. There is no reason to assume this assumption is problematic for the case of Gaza any more that it is in general.

[21] Except for energy imports, which are also reported by Israel.

Table 4
Changes to employment by industry in Gaza, 2006–2009.

	Direct measures of trade			Inclusive measures of trade
	(1)	(2)	(3)	(4)
Share of inputs imported	−0.94**	−0.83**	−0.77**	−0.41*
	(0.00)	(0.02)	(0.04)	(0.09)
Share of output exported	−3.18***	−3.34***	−3.01***	−2.04***
	(0.00)	(0.00)	(0.00)	(0.01)
Capital intensity	−0.08	−0.06	−0.06	0.01
	(0.32)	(0.45)	(0.46)	(0.45)
Manufacturing dummy	No	Yes	Yes	No
Pre-blockade industry size controls	No	No	Yes	No
N	51	51	51	23

Notes: Data are from the industry and services surveys of 2006 and 2009. The dependent variable is the change in the natural log of number of workers in the industry between 2006 and 2009. Controls for pre-blockade size of the industry include number of workers, and total value of sales. The Direct Measure of Trade is the share of inputs that firms in the industry import, and the share of output they directly export. The inclusive measure takes into account imports and exports of upstream and downstream industries. * = P-value < 0.1, ** = P-value < 0.05, *** = P-value < 0.01.

section is to document two key facts about the adjustment of production: the reallocation of workers between sectors and between industries within each sector, and the decline in labor productivity. Put together, these facts can help explain the magnitude of the welfare losses during the blockade, and strengthen the argument that the changes experienced in the Gaza economy were related to the elimination of access to world markets. The data used in this section is taken from firms surveys, conducted by the PCBS. See the online data appendix for complete description of the data and associated issues.

5.1. Labor reallocation

Fig. 6 provides a first look at the data on the reallocation of workers. The top panel shows the correlation between changes to employment in an industry and the industry use of imported inputs, and the bottom panel shows the correlation between changes to employment and the industry's export intensity. In both cases a negative correlation is evident. Moreover, the figure also shows that while Gaza could not fully adjust to its new state, much reallocation did in fact occur.

Regression analysis further supports this correlation. Our basic specification is

$$\Delta L_i = \alpha + \beta_1 k_i + \beta_2 EX_i + \beta_3 IM_i + \beta_4 Z_i + \epsilon_i$$

where ΔL_i is the change to the log of number of workers in industry i, k_i is capital intensity of industry i, measured as the ratio of the value of non-building capital to the number of workers, EX_i is the share of total output in the industry that is exported, IM_i is the share of total value of inputs that is being imported, and Z_i is a vector of controls, including total sales, total number of workers, and a dummy that takes the value of 1 for industries in the manufacturing sector. All variables are measured at 2006 levels. To get a better measure of trade reliance, we also use Israeli input–output tables to construct a trade measure that takes into account imports of upstream industries, and exports of downstream ones. The textile industry in Gaza is a stark example for the importance of this calculation: While the garment industry imported in 2006 only 1.1% of its inputs, the weaving industry imported about 93% of its inputs.[22]

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[22] The way to construct this measure is described in Feenstra (2003, p.37). One important difference between the two measures of an industry's trade reliance is the fact that the input–output table we use is only available at a higher level of aggregation—26 industries in total, 23 with more than 5 active firms in Gaza, relative to 72 industries in total, 51 with more than 5 active firms in them in the firm-level data.

Table 5
Average worker productivity, and number of workers in Gaza by sector 2006–2009.

	ΔVA per worker	ΔLabor
Manufacturing	−36.6%	−33.0%
Services	−0.6%	+24%
Total	−20.0%	−2.5%

Notes: Data are from industry and services surveys for Gaza in 2006 and 2009. Real value added in an industry is total sales in the industry minus total purchases of inputs in the industry. Change to value added is calculated using the Divisia method. Because of concern for measurement errors, industries with extremely large changes to value added (5% of industries) are excluded from this calculation.

The results are presented in Table 4. Columns 1–3 report the results of using the direct imports and exports as a measure of trade reliance. Across all specifications, the coefficients show a reallocation away from industries that use imported inputs intensively and away from industries that exported intensively, even when controlling for the services-manufacturing difference. There is also some evidence that labor was reallocated away from capital intensive industries, though the significance of the result is weak. Column 4 reports the results of using the measures for trade reliance that take into account the imports and exports by upstream and downstream industries. The small number of observations for these regressions limits the number of covariates, but the key results do not change.

5.2. Real value added per Worker

As a measure of productivity we use real value added per worker, a commonly used measure (see for example Bernard et al., 2003). We start with calculating the 2006 level of value added per worker in each industry that operated in Gaza in both 2006 and 2009. That is, we divide total industry value added by the total number of workers to get the nominal value added per worker in the industry.[23] We then use the Divisia method, as in Basu and Fernald (1997), to calculate the change in value added:

$$\Delta VA_i \equiv \frac{\Delta Y_i - s_{m,i}\Delta m_i}{1 - s_{m,i}}$$

where ΔY_i is the change in real output, Δm_i is the change to real inputs, and $s_{m,i}$ is the share of total revenue that is spent on inputs—all measured at the industry level.[24] As Basu and Ferland explain, this measure can be thought of as a "partial Solow residual, subtracting materials growth from output growth, weighted by the share of intermediate inputs in revenue." To get the change in value added per worker, we then divide the result by the change in the number of workers in the industry.

Table 5 presents changes to average real value added per worker and the total number of workers at an aggregate level, and separately for manufacturing and services in Gaza. The total decline was 20%, while the total number of workers remained quite stable, declining by only 2.5%. However, the changes were very different between the manufacturing and the services sectors: The manufacturing sector experienced a decline of 36.6% in average real value added per worker, and a decline of 33% in total labor, while the services sector experienced only a 0.6% decline in average real value added per worker, and saw an increase of 24% in employment.

To verify that the measured decline in value added is the result of an actual decline in productivity, and not of reallocation to lower value

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[23] Since the data is a repeated cross-section, we cannot make our calculations at the firm level, but only at the industry level.

[24] Measuring changes to real inputs requires an industry specific input price index, which in turn requires on an input–output table, which does not exist for the Gaza Strip or the West Bank. For the results we report here, we use an input price index we created using an Israeli input–output table. For the share of inputs in total revenue, we use the average of 2006 and 2009, which is a discrete time approximation of the continuous time definition used here.

Table 6
Decline in value added per worker in Gaza, 2006–2009.

	Full sample	Excluding outliers
Total decline (% of 2006)	23%	20%
Productivity effect (% of total decline)	83%	81%
Reallocation effect (% of total decline)	17%	19%
# of activities	72	62

Notes: Real value added per worker in an industry is the sum of the industry sales minus its purchases of inputs, divided by total employment. Value added change is calculated using the Divisia method. Data is from industry and services surveys for Gaza 2006 and 2009. Column 1 uses the full sample, and column 2 excludes industries where the change in real value added per worker was greater than four-fold.

added sectors, we decompose the total decline as follows. We denote the average value added per worker in activity i, in year t by $a_{i,t}$, and the economy-wide value added per worker in year t by $a_t = \sum_i a_{i,t} \times w_{i,t}$ with $w_{i,t}$ being the share of workers employed in industry i in year t. The change in the economy-wide value added per worker is:

$$a_{2009} - a_{2006} = \sum_i w_{i,2009} \times \left(a_{i,2009} - a_{i,2006} \right) \\ + \sum_i a_{i,2006} \left(w_{i,2009} - w_{i,2006} \right) \qquad (1)$$

The first term is the intensive margin, or the *productivity change* effect, and the second term is the change along the extensive margin, or the *reallocation effect*. The results of this decomposition, applied at the finest level of disaggregation the data allows (11 services industries, and 61 manufacturing industries), are reported in Table 6. The key result is that the productivity effect was much larger than the reallocation effect. Out of a total decline of 23%, the productivity effect explains 83%.

Here too, a regression analysis establishes that the decline in productivity was correlated with trade reliance. Table 7 replicates the analysis of labor reallocation, but uses value added per worker as the dependent variable. Reliance on imported inputs is correlated with a decline in productivity across all of our specifications, and is both economically and statistically significant. However, unlike the case of labor reallocation, the share of output that used to be exported is not correlated with changes to an industry's productivity. This is expected, since unlike reliance on imported inputs, the inability to export a large share of your output does not directly affect productivity, though it is likely to lead to a decline in activity, and thus to a reallocation of labor away from the industry.

6. Conclusion

What can the blockade on the Gaza Strip teach us about the gains from trade?

The first lesson is that, for a small economy, the short-run costs of being removed from world markets may be very large, and in particular, substantially larger than what standard models of trade predict. The decline in welfare in Gaza, as calculated based on the household expenditure survey, was substantially larger than the predictions of such models. Moreover, the costs of the blockade on Gaza were not borne equally: Wealthier households suffered disproportionally more. A second lesson is that access to world markets not only allowed the Gaza economy to better allocate its factors of production, it also greatly increased their productivity, especially in manufacturing. When access to world markets was eliminated, 31 out of 40 manufacturing industries in Gaza[25] experienced a decline in labor productivity. Moreover, a large part of worker reallocation was from manufacturing and into the

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[25] As in the regressions, the sample is limited to those industries that had at least 5 operating firms in them.

Table 7
Changes to productivity by industry in Gaza, 2006–2009.

	Direct measure of trade			Inclusive measures of trade
	(1)	(2)	(3)	(4)
Share of inputs imported	−1.98***	−1.76***	−1.73**	−0.72
	(0.00)	(0.01)	(0.02)	(0.16)
Share of output exported	1.21	1.66	1.67	−0.85
	(0.56)	(0.43)	(0.44)	(0.15)
Capital intensity	−0.10	−0.07	−0.06	−0.01
	(0.23)	(0.41)	(0.46)	(0.89)
Manufacturing dummy	No	Yes	Yes	No
Pre-blockade industry size controls	No	No	Yes	No
N	47	47	47	23

Notes: Data are from the industry and services surveys of 2006 and 2009. The dependent variable is the change in the natural log of value added per worker in the industry between 2006 and 2009. Controls for pre-blockade size of the industry include number of workers and total value of sales. The direct measure of trade is the share of inputs that firms in the industry import, and the share of output they directly export. The inclusive measure takes into account imports and exports of upstream and downstream industries. ** = P-value - 0.05, *** = P-value < 0.01.

services sector, and not towards the import substitutes sectors, which is further evidence of the dependency of the Gaza manufacturing sector on imported inputs and export markets. Finally, by observing what happened when trade stopped, we learn that some predictions of trade theory—the decline in manufacturing productivity, and the differential effect on households with different incomes—are a good description of reality.

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