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Who Gains from Trade Protection in Ghana: A Household-level Analysis?

by

Charles Ackah, Oliver Morrissey and Simon Appleton

Abstract

In this paper, we present one of the first direct microeconometric studies of the impact of trade protection on household income in Ghana. Tariff measures at the two-digit ISIC level are matched to Ghanaian household survey data for 1991/92 and 1998/99 to represent the tariff for the industry in which the household head is employed. We examine the possibility that the effect of protection on income might not be uniform across households characterized by different skill levels. Specifically, we allow the relationship between welfare and trade policy to differ for households with different levels of education. In the absence of suitable panel data, the analysis applies pseudo-panel econometric techniques to our repeated cross-section data. This method has rarely been used in poverty analysis. The results suggest that higher tariffs are associated with higher incomes for households employed in the sector, so tariff reductions may reduce incomes (and increase poverty), at least in the short run, but with differing effects across skill groups. We find that this positive effect of protection is disproportionately greater for low skilled labour households, suggesting an erosion of welfare of unskilled labour households would result from trade liberalization. We conclude that contemplating trade liberalization without recognizing the complementary role of human capital investment may be a sub-optimal policy for the poor, at least in the short-run.

Centre for Research in Economic Development and International Trade, University of Nottingham

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1. INTRODUCTION

The persistence of poverty in many developing countries, especially in Sub-Saharan Africa (SSA), in the face of increased globalisation and rapid trade liberalization during the past two decades has inspired considerable public debate on the impact of globalisation, in general, and trade liberalisation, in particular, on poverty. The standard arguments, based on the Stolper-Samuelson theorem of international trade theory, are that trade liberalisation would lead to a rise in the incomes of unskilled labour in developing countries. Thus, according to the associated Ricardian comparative advantage theory, the poor (unskilled labour) will be the largest beneficiary of trade liberalisation. In other words, since developing countries are more likely to have a comparative advantage in producing unskilled labour-intensive goods, one would expect trade reforms in these countries to be inherently pro-poor (see Krueger (1983); Bhagwati and Srinivasan (2002); Bhagwati (2004); Harrison (2005)). However, the experiences of many developing countries, particularly in SSA, have been disappointing and in many cases poverty has increased following trade liberalisation (see Easterly, 2001).[1] It is estimated that more than one billion people still live in extreme poverty (based on the US$1 per day poverty line), and half the world's population lives on less than US$2 a day. These statistics have stimulated a lot of concern about whether the poor gain from trade liberalisation, and under what circumstances it may by-pass or actually hurt them.

Not surprisingly, the impact of trade reforms on the welfare of the poor has become an important subject of interest to researchers and policy makers alike. However, there has been limited empirical research on how these reforms affect poverty at the household level (Winters, 2002; Winters *et al*., 2004). The main objective of this paper is to make a contribution to this small literature through an empirical investigation of the poverty effect of trade protection based on Ghanaian household data. This objective is motivated by the paucity of research in this area for Ghana. Very little evidence in Ghana concentrates on trade effects and few studies are based on household data. Despite the general concerns expressed in many quarters, relatively little is known about the actual impacts of trade policy reforms on the welfare of the poor. While there has been some work on poverty measurement and descriptive analysis of the characteristics of the poor, to our knowledge there is no accessible multivariate econometric analysis using policy variables, such as tariffs, to examine the impact of trade policy on household poverty (whether measured in terms of wages or income) in Ghana. The scarcity of studies on this important topic is primarily due to the lack of representative household panel data sets on the one hand, and the limited availability of trade policy data, coupled with the problem of identification of the poverty effects of trade policy at the household level, on the other hand.[2]

This paper takes a step towards filling this gap. Specifically, this is one of the first studies to use repeated cross-section data (RCS) from the Ghana Living Standards Survey (GLSS) data against the background of trade reforms of the 1990s to gauge some of the effects of trade policy on households. While the relationship between trade policy and incomes/poverty at the household level is by no means clear, and analysis is therefore complex, we demonstrate that, even with limited data, it is still possible to assess some of the effects of trade policy on households, and by inference on poverty, and therefore contribute to a more informed policy debate. Our analyses include static and dynamic, linear and non-linear, levels and first-difference models to indicate that a lower industry tariff tends to be associated with lower income being earned by households affiliated to the industry, controlling for household-specific characteristics, geographic variables and industry fixed-effects. We find that this positive effect of protection is disproportionately greater for low skilled labour households, suggesting an erosion of welfare of unskilled labour households would result from trade liberalization.

The remainder of the paper is organized as follows. The next section briefly reviews some relevant theoretical literature on international trade. Section 3 discusses the dataset and variable selection. Section 4 follows with a description of the empirical strategy. In

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[1] Compared to other regions, Africa, and especially SSA, has exhibited poor economic performance over at least the past two decades. While some countries have been exceptions to the trend and performed very well, the regional performance is cause for concern.

[2] Coloumbe and McKay (2003) cite the non-availability of panel data as one of the major limitations of using the GLSS in an analysis of the determinants of changes in poverty and inequality.

section 5 we summarize and assess the econometric results. Section 6 provides additional robustness checks while Section 7 concludes.

2. TRADE AND LABOUR INCOME: A THEORETICAL CONSIDERATION

This section provides a brief review of the main theories on the labour market impact of international trade. Specifically, we discuss what theory predicts about the impact of trade on labour income (or wages) in developing countries. The standard explanation in defence of trade liberalisation is based on the Stolper-Samuelson theorem, which suggests that international trade will lead to a rise in the relative returns of the abundant factor; unskilled labour in the case of developing countries. Thus, according to this theory, the poor (unskilled labour) will be the largest beneficiary of trade liberalisation. As developing countries are more likely to have a comparative advantage in producing goods that use unskilled labour relatively more intensively, we would expect trade reforms in these countries to be inherently pro-poor (see Krueger (1983); Srinivasan and Bhagwati (2002); Bhagwati (2004); Harrison (2005)).[3] These expected gains are conditional on a number of assumptions - including free mobility of labour, given technology and perfect competition[4]. However, the assumptions underpinning the theorem are inherently too restrictive to provide a practical interpretation of the complexity of the relationship between trade reform and poverty. Moreover, adjustment to trade may result in additional short and medium term costs and challenges for the poor (see Ackah and Morrissey, 2005:5-7 for a discussion of the benefits and costs of trade policy reforms).

Recently these sharp predictions of the Stolper-Samuelson theorem have been challenged. According to the new theories, trade liberalization could reduce the wages of unskilled labour even in a labour abundant country, thereby widening the gap between the rich and the poor. Many observers find the Stolper-Samuelson theorem quite restrictive, in that the theorem does not offer definitive conclusions if one or more assumptions are relaxed (see Davis, 1996). Davis and Mishra (2004 cited in Harrison, 2005), argue that the popular expectation that trade liberalisation should increase the incomes of the poor in low income

[3] For an empirical example, see Hertel *et al*. (2003) who estimate that global trade liberalization leads in the long run (i.e. when labour and capital are mobile across sectors) to a decline in poverty for all strata of the population largely because of increased demand for unskilled labour.

countries is based on a very narrow interpretation of the standard Heckscher-Ohlin model. Davis and Mishra show that in a world of many factors and many goods, a poor country might no longer have a comparative advantage in producing unskilled intensive goods. Similarly, if a poor country has large supplies of non-labour factors of production (like land or mineral resources), trade liberalization may not benefit the labour-intensive sectors.

The specific factor and the Ricardo-Viner models have become the natural alternative to the Heckscher–Ohlin model and the associated Stolper–Samuelson theorem. According to these models workers may gain from trade reforms depending on which sectors (import-competing or exporting) they are attached to. The models focus on the short- to medium-run and assume imperfect factor mobility with one factor mobile across sectors while the other is taken to be sector-specific. With these assumptions the models predict a positive association between protection and returns to factors of production (e.g. wages). Protection reduces imports and reduced imports increase labour demand, which in turn increases wages. When the price of a good falls following trade liberalisation the model predicts that the factor specific to the sector that experienced a price reduction loses while the other factor gains in real terms. In other words, if trade liberalisation occurred households affiliated to the industries that experience large tariff reductions would see a decline in their incomes relative to the economy-wide average income, while households attached to other (competitive) industries would gain in comparison.[5]

Given the apparent ambiguity in the theoretical literature discussed above the relationship between trade liberalization and poverty is ultimately an empirical matter. Empirically it is

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[4] This is an assumption that is unlikely to hold in developing countries like Ghana, especially in the short run, where labour markets are characterized by significant labour rigidities.

[5] Given the underdeveloped labour markets in most developing countries, this model appears a plausible starting point for thinking about the relationship between trade protection and income poverty in Ghana (see Attanasio *et al*., 2004). There are good reasons to believe that the assumption of perfect labour mobility across sectors is unlikely to hold, at least in the short run, in most developing countries including Ghana. Even the assumption of perfectly competitive markets can only be envisaged in the long run. While we do not propose, in this paper, to subject these theories to empirical testing, we hope that in the end we are able to find a theoretical basis for explaining the observed changes in household welfare (income) and inequality in Ghana *vis à vis* the trade reforms in the 1990s.

not simple to disentangle the effects on incomes of trade reform from other macroeconomic policies and technological changes occurring simultaneously. As mentioned in the introduction, the non-availability (or scarcity) of panel data sets in developing countries is one of the major obstacles hampering poverty analysis in these countries. The lack of suitable panel data, especially for many African countries, has led to the widespread utilization of OLS regression on cross-section datasets in order to estimate the effects of public policy on poverty at the household level. One potential problem is that the estimated coefficients are likely to be contaminated by unobserved household fixed effects (characteristics) leading to biases in the estimation results and incorrect inferences. Fortunately, there is by now a rapidly growing literature on pseudo panel data models constructed from repeated cross sections (see Appendix C for a review). This paper is in that tradition. We consider what can be learnt from analyzing repeated cross-sections as is predominant in studies interested in consumption and labour supply issues (e.g. Browning, Deaton and Irish (1985)). We extend these approaches for the analysis of trade policy and poverty in Ghana. In this way, this study circumvents the absence of 'true' panel data for Ghana, while still exploiting some of the attractive features of panel data analysis such as the ability to control for household-specific effects and unobserved heterogeneity (Deaton, 1985).

3. Data Description and Summary Statistics

In this subsection we describe the data and the main features of the variables that are relevant for the subsequent econometric analysis. Two sources of data for Ghana are used to assess the impact of trade policy on household welfare during the 1990s. The primary data source is the GLSS conducted in 1991/92 and 1998/99.[6] The second data source is the Most Favoured Nation (MFN) tariff data for years close to the two household surveys; tariff data, our preferred measure of trade policy, covers 1993 and 2000.[7] We construct a database of annual tariff data for 1993 and 2000 at the two-digit ISIC level to calculate

[6] The main advantage of using these two surveys is that they employed almost identical questionnaires which aids in analysing changes in poverty between the two survey years.
[7] Ideally, we would have required tariff data for 1998/99. However, for some reason this data is not readily available. This imposes a limitation on this study. Nonetheless, it is reasonable to assume that the tariff data captured in 2000 fairly represents tariffs prevailing in 1998/99. Evidence from Figure A1 in Appendix A

average industry-level tariffs. The result is a two-digit classification of 26 industries per year, of which 19 are in the traded-goods sector and seven in the non-traded sector.[8] Our sample is restricted to households with heads aged between 18-64 inclusive, employed in any sector (tradable or non-tradable). The sample is selected conditional on working so that the effects of protection conditional on being in the labour force are examined. Non-working households are excluded[9]. Each of the selected households is mapped on to one of the 26 sectors according to the sector of main employment of the household head. These exclusion restrictions leave us with a sample of 3350 and 4484 households from GLSS 3 and GLSS 4 respectively.

Among the household-level variables, we start by considering the following categories of variables: a set of demographic variables, variables relating to educational attainment, household size, linear and quadratic terms in the age of the head of the household are also included to capture possible life-cycle effects. We include agro-climatic zones in our model as dummy variables to control for the effects of agro-ecological zone characteristics on household welfare. Doing so allows us to gauge the effects of the other determinants on household welfare independent of the effect of agro-climatic conditions on the household. To ascertain whether there were any significant changes in household welfare between the two periods, we introduce a survey-year dummy, *GLSS4*. Furthermore, we allow for sectoral heterogeneity by including a dummy for households located in urban sectors, *Urban*. Using the information on the highest qualification obtained, we define five education indicators: No education, Basic education, Secondary education, Post-secondary education and Tertiary Education (university degree). For each cross section, Table 1 reports summary statistics of our key variables.

Ghana embarked on a massive expansion in the provision of education during the 1990s which has resulted in the increased educational attainments during the period. The

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suggests that tariffs remained stable during the latter part of the 1990s (from 1997) and we believe this pattern may have continued into 2000.

[8] Following Topalova (2005:16) all households employed in non-tradable industries are assigned a tariff of zero.

[9] This was necessitated by the fact that the survey questionnaire only solicited information about industry of employment for working individuals and since our tariff data is at the industry level.

proportion of households with illiterate heads (no education) fell from 32.3 percent to 28 percent. There were substantial increases in the proportions of households whose heads have completed more than primary school education. Proportion of heads with secondary education increased from 5.7 percent to 6.6 percent while those with post-secondary education increased from 3.5 percent to 6.6 percent. The share of heads with basic education has remained stable at around 57 percent. The percentage of heads with tertiary education, however, declined marginally - the share of those with university degrees fell from 0.8 to 0.6 percent.

Table 1: Summary Statistics

	1991/92		**1998/99**	
Variable	**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**
Welfare (consumption expenditure)	1,457,110	1,293,483	1,668,206	1,483,357
Log Welfare	13.927	0.710	14.056	0.729
Age of head	38.169	9.823	42.281	10.504
Age of head squared	1553	767	1898	921
Female-headed household	0.304	0.460	0.308	0.462
Household head has -				
No Education	0.323	0.468	0.280	0.449
Basic Education	0.574	0.495	0.578	0.494
Secondary Education	0.057	0.231	0.066	0.248
Post-secondary Education	0.035	0.183	0.066	0.248
Tertiary Education (University)	0.008	0.091	0.006	0.074
Log Value of Land	3.510	5.597	3.419	6.283
Economic Activity indicators				
Public Sector	0.159	0.366	0.114	0.318
Private Formal	0.053	0.224	0.060	0.237
Private Informal	0.040	0.197	0.035	0.185
Export Farmer	0.047	0.211	0.071	0.257
Food Crop Farmer	0.396	0.489	0.371	0.483
Non-farm Self-employment	0.304	0.460	0.347	0.476
Observations	**3350**		**4484**	

Source: Authors' calculation from GLSS 1991/92 and 1998/99
Note: The reported figures are weighted using survey weights. Values (welfare and land) are in constant prices of Accra in January 1999.

Over the period we observe a decrease (from 15.9 to 11.4 percent) in the share of households employed in the public sector, consistent with the public sector retrenchment which began in the mid 1990s under Structural Adjustment Programme (see Aryeetey, 2005). Even though food crop farming is the largest source of employment for a great majority of households, its share declined significantly from about 40% in 1991/92 to 37% in 1998/99. On the other hand, the share of export farming increased by a massive 51% between the two surveys, but only from 5% to 7%. The non-farm self-employment saw a 14% increase in its share to maintain its position as the second largest employer.

Table 2: Poverty by Economic Activity and Location, 1991/92 and 1998/99

Economic Activity	**1991/92**		**1998/99**	
	Poverty incidence	***Contribution to national poverty***	***Poverty incidence***	***Contribution to national poverty***
Public sector employment	0.35	9.1	0.23	6.2
Private formal employment	0.30	2.3	0.11	1.4
Private informal employment	0.39	2.3	0.25	1.9
Export farmers	0.64	7.8	0.39	6.9
Food crop farmers	0.68	57.3	0.59	58.1
Non-farm self employment	0.38	20.5	0.29	24.5
Non-working	0.19	0.7	0.20	1.1
Location				
Rural	*0.63*	*82.2*	*49.50*	*83.7*
Urban	*0.27*	*17.8*	*19.40*	*16.3*
All Ghana	**0.52**	**100.0**	**0.40**	**100.0**

Source: Authors' calculation from GLSS 1991/92 and 1998/99

Table 2 provides information on the incidence of poverty and contribution to national poverty by each occupation. In 1991/92 the incidence of poverty in food crop and export farming households were quite similar, 68% and 64% respectively. However, by 1998/99 poverty incidence decreased to 39% in export farming households, while for food crop farmers it only fell to 59%. In terms of poverty shares, food crop farmers actually saw a marginal increase in their share of national poverty from 57.3% to 58.1%. Similarly, the non-farm self-employed experienced an increase in their contribution to national poverty despite a drop in the incidence of poverty. Spatially, poverty in Ghana is almost entirely a rural phenomenon. With a population share of just about 64% the rural sector contributes disproportionately 82% to total poverty, while urban households account for only 18%. The story that emerges from Tables 1 and 2 suggests that those who appear to have benefited the most from the economic policies of the 1990s were the urban and export farming households.[10] The rural households and food crop farmers who form the bulk of the population appear to have benefited the least. What is clear is that policy reform has had a differential impact on different groups of households. Indeed, our conservative measure of inequality defined as the standard deviation of the log welfare, increased slightly over this period (from 0.71 to 0.73). This is broadly consistent with inequality as measured by the Gini coefficient which suggests a modest increase from 0.37 in 1991/92 to 0.39 in 1998/99 (Aryeetey and McKay, 2004).[11]

Table 3 considers the skill composition of these occupational groups while Table 4 does the same for the rural and urban sectors. Skilled (or semi-skilled) households are largely wage earners in either the public sector (39%) or the private formal sector (19%). Even though the unskilled dominate all socio-economic groups, almost all agriculture households (about 99% of food crop farmers and 98% of export farmers) are unskilled. Moreover, while the unskilled are predominantly rural (67%) the semi-skilled (73%) and skilled (55%) are largely located in urban centres. The foregoing descriptive evidence is instructive. The main message is that policy reforms in the 1990s were possibly not pro-poor if unskilled labour households benefited the least.[12] Of course the simple descriptive analysis adopted here is unable to attribute changes to any particular policy *per se*. A reasonable hypothesis is that trade policy is among the factors accountable for the observed evolution of poverty and inequality.

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[10] In principle, economic reforms (of which trade liberalisation is one aspect) are expected remove anti-export biases and shift incentives towards the production of tradables. To the extent that trade liberalisation leads to a rise in returns to exporting activities, it is not surprising that export farming households in Ghana recorded the highest reductions in poverty incidence during the 1990s. Aryeetey (2005) has argued, however, that one of the reasons why the export farming sector performed relatively better than their counterparts engaged in food crop farming is due to the fact that whilst agricultural subsidies were removed in the food sector as part of the liberalisation process, the export farmers have been benefiting from governmental support in terms of technical training and other export promotion packages.

[11] See also Teal (2001) who finds that inequality as measured by the standard deviation of log household expenditure per capita (in 1998 prices) increased from 0.76 to 0.77. This evidence is further corroborated by his Gin coefficient measure based on household expenditure per capita in 1998 prices, which indicates a rise from 0.42 in 1991/92 to 0.46 in 1998/99.

[12] Teal (2000a, b) presents further evidence that the 1990s witnessed a continuing fall in the real wages for unskilled labour in Ghana.

Table 3: Economic Activity Shares by Skill Levels, 1991/92

	Skill			
Economic Activity	***Unskilled***	***Semi-skilled***	***Skilled***	***All***
Public sector employment	0.61	0.19	0.20	1.00
Private formal employment	0.82	0.14	0.05	1.00
Private informal employment	0.89	0.10	0.01	1.00
Export farmers	0.98	0.01	0.01	1.00
Food crop farmers	0.99	0.01	0.00	1.00
Non-farm self employment	0.94	0.03	0.02	1.00

Source: Authors' calculation from GLSS 1991/92.
Note: Unskilled are households whose head has completed basic or no education, semiskilled for heads who have completed secondary or post-secondary and skilled for households with university graduate heads.

Table 4: Share of Skill Levels by Rural/Urban Location, 1991/92

	Location		
Skill	***Rural***	***Urban***	***All***
Unskilled	0.67	0.33	1.00
Semi-skilled	0.27	0.73	1.00
Skilled	0.45	0.55	1.00

Source: Authors' calculation from GLSS 1991/92.
Note: Same as for Table 3.

An alternative claim which seems to be gaining support is to say that trade is actually not to blame but rather skill-biased technological change is the problem. Görg and Strobl (2002), using firm-level data on manufacturing in Ghana, found that skill-biased technical change, arising from increased purchase of foreign machinery after the trade reforms, resulted in increased demand for skilled workers. However, to the extent that skill-biased technological change is an endogenous product of trade liberalisation, the relative non-performance of unskilled rural and food crop farming households could be attributed, at least partially or indirectly, to trade liberalisation. Moreover, Teal (1999, 2001), using firm-level and household data respectively, finds no evidence of any underlying technical progress in explaining the increased income inequality in the 1990s. In a related study, Teal (2000b) provides evidence which suggests that high rates of inflation and low investment are the two major factors responsible for the substantial falls in the real wages of the unskilled in manufacturing between 1992 and 1998. Unfortunately, Teal did not consider the role of trade policy in his analysis. In this paper we argue that trade policy is one of the factors contributing to the observed trends in poverty and income inequality.

Table A1 and Figures A2 and A3 in Appendix A show the average tariff levels and changes across all the 19 traded sectors between 1993 and 2000. It is worth pointing out that whereas the average unweighted scheduled tariff across *all* industries declined from 17% in 1992 to 8.5% in 1999 (see Figure A1 in Appendix A) the structure and pattern of tariff changes was not uniform across sectors. Hence, our data reveals that for a sizeable number of manufacturing industries (usually, sectors with relatively skilled labour) the average tariff actually increased during the 1990s. Most manufacturing sectors continued to enjoy high levels of protection with the average tariff for industry increasing by 12 percent. The agriculture and allied industries enjoyed especially high levels of protection to begin with but these are also the sectors where tariff reductions were greatest. This suggests that Ghana protected relatively unskilled, labour-intensive sectors during the era of import substitution industrialization which continued to persist into the early 1990s, notwithstanding the economic reforms of the 1980s. The rapid and substantive liberalization of trade in agriculture in the 1990s was not accompanied by similar reforms in manufacturing. What is unique about the 1990s was the sudden attempt to change the structure of protection from low-skilled agriculture and relatively low-skilled manufactures to relatively high skilled sectors. Indeed, Figure A3 suggests that sectors with relatively higher proportions of unskilled labour households witnessed the largest reductions in import tariffs whilst relatively skilled sectors experienced the largest increases in tariffs between 1993 and 2000.[13] The correlation between the unskilled labour share and the change in tariff, however, is weak (–0.08).

Since Ghana's trade reforms entailed larger tariff reductions (and hence largeer reductions in the price of their output) in relatively unskilled and relatively protected sectors, the logic of the Stolper-Samuelson theorem would imply that unskilled labour households will lose, relatively.[14] If labour is really perfectly mobile, i.e., if we assume away labour

[13] This is consistent with the experience in other developing countries in Latin America, especially Columbia and Mexico, where there were large increases in the skill premium following trade liberalization as noted by Attanasio *et al*. (2004).
[14] There is compelling evidence that the relative incomes of skilled labour in Ghana rose over the period under study (see Görg and Strobl (2002) and Teal (2000b)).

market rigidities (which is very unlikely for Ghana), as the theory assumes, we would expect an accompanying reallocation of labour across sectors. We would expect to see labour reallocation from the sectors with the largest tariff reductions (the contracting unskilled sectors) to the sectors with the smaller tariff reductions (the expanding skilled sectors). The theory further predicts that the share of unskilled labour in industry employment should rise as firms substitute away from skilled labour with the rising relative return to skilled labour. However, both predictions are not borne out by the evidence in Table A2 in Appendix A. First, we fail to observe any discernible shifts in employment between sectors (see right panel of Table A2). In fact, shares of industries in total employment remained relatively stable between 1991/92 and 1998/99.

4. EMPIRICAL METHODOLOGY

In this section, we discuss the econometric models estimated and some econometric issues encountered. Our main objective is to investigate the causal effect of trade policy on household welfare in Ghana during the 1990s. Of particular interest here is the potential contingency of the effect of trade policy on educational qualification or skill type of the household. We are also interested in systematically distinguishing the long-run impact of trade protection on household welfare from that of the short-run. In the end, we hope to provide answers to the following questions: (1) does trade protection affect every household equally independent of the skill type of the household? In other words, would the effect of trade liberalisation be felt equally across households (skilled and unskilled)? (2) Is the effect of trade protection constant or time-dependent? Put differently, is the long-run impact of protection similar or different from that of the short-run?

In order to investigate such questions, longitudinal data with multiple observations on the same households over time would be ideal. Unfortunately, such data are seldom available in developing countries, Ghana being no exception. The analysis in this paper therefore applies pseudo-panel econometric techniques to our repeated cross-sectional data. This method has rarely been used in poverty analysis. After matching each household with the relevant industry tariff information, we examine how the standard of living measure relates to trade protection. The approach is based on modelling the natural logarithm of per adult equivalent consumption expenditure of survey households, adjusted for variations in prices between localities and over time (Welfare, used here to proxy for income and by implication poverty).[15] One of the key features of the recent policy reforms in Ghana has been the significant changes in the levels of import protection. Undoubtedly, household incomes and consumption expenditures are likely to have been affected by the cross-sector pattern of tariffs.

We formalize the determinants of household welfare (or income) as follows:

$$\begin{aligned} \ln w_{it} &= \alpha + \beta_1 age_{it} + \beta_2 age_{it}^{\,2} + \beta_3 hsize_{it} + \beta_4 educ_{it} + \beta_5 urban_{it} \\ &\quad + \beta_6 ecoz_{it} + \beta_7 land_{it} + \delta_1 tariff_{jt} + f_i + \lambda_j + \gamma_t + \varepsilon_{it} \end{aligned} \tag{1}$$

where the dependent variable is as previously defined, age is the age of household head at the time of the survey, age^2 is squared age, $hsize$ is the size of the household, $educ$ is education of the household head, $urban$ is a 0/1 dummy which is 1 for households in urban localities, ez is agro-climatic zone, $land$ is the value of land owned by the household (instead of the actual land cultivated, in order to implicitly account for land quality), $tariff$ is the average tariff applied to imports of industry j's products in year t,

[15] The literature on how international trade affects incomes of the poor or poverty, more generally, is extremely scarce relative to the literature on wage inequality. Moreover, this already small literature tends to be concentrated the US and Latin America. Among the existing studies there has been a tendency towards modelling manufacturing wages as opposed to absolute measures of well-being, such as poverty (Goldberg and Pavcnik, 2003). In many developing countries, however, wage income is not the primary source of income for the poor. In Ghana, for example, the GSS (1995 and 2000a) reports note that wage employment, whether formal or informal, constitutes the main economic activity in only around one fifth of households. In fact, this already low proportion declined over the 1990s, albeit marginally, due mainly to the public sector retrenchment in the early 1990s. In contrast, 69 percent of households were involved in self-employment (39% in agriculture and 30% in non- agricultural activities). To the extent that trade liberalisation affects the returns to different economic activities, rents and remittances, an appropriate means of investigating the effect of trade policy on poverty is to look at incomes. Modelling household incomes is appealing due to the possibility of being able to consider, and also to compare, income from engaging in different activities (Aryeetey and McKay, 2004). However, on theoretical grounds (and in practice), most development economists prefer consumption expenditure over income (see Deaton and Grosh 2000; Appleton 2002; Teal 2006). This is due, in part, to the difficulty in measuring income, including those obtained from engaging in own account activities. McKay (2000, cited in Aryeetey and McKay (2004)), for example, finds that in the case of Ghana average household income in 1991/92 was underestimated by about 55% of average consumption expenditure in the same year. Hence, consumption expenditure is used as the standard of living measure in setting the poverty line in Ghana. In a later study seeking to understand the factors behind the changing patterns of poverty and inequality in Ghana, the authors adopted a consumption

f is the household fixed effects, λ is the fixed effects for the household's industry affiliation, γ is the year fixed effect and ε is the error term. Subscripts i and t index households and survey years respectively. Year fixed effects are included to absorb economy-wide shocks (such as technological change) that may affect welfare whilst industry dummies control for sector-specific effects.

Each of the explanatory variables is likely to explain some of the differences in household welfare. However, it must be recognized that other unmeasured or unobservable differences among households may also matter. Unmeasured or unobservable individual heterogeneity is a problem that faces all survey research. A pooled analysis of the data based on equation (1) will be seriously flawed, in part because such analysis cannot control for unobservables and in part because it assumes that repeated observations on each household are independent. The presence of f and λ in the model implies that we need panel data to consistently estimate the parameters in the model.[16] To address these issues, we employ the ideas espoused by Deaton (1985) by constructing a pseudo panel from our repeated cross-sectional data. Following the pseudo panel data literature, the first extension is to take cohort averages of all variables and estimate (1) based on the cohort means (see equation (C.2) in Appendix C).[17]

$$\ln \overline{w}_{ct} = \alpha + \beta_1 \overline{age}_{ct} + \beta_2 \overline{age}_{ct}^{\,2} + \beta_3 \overline{hsize}_{ct} + \beta_4 \overline{educ}_{ct} + \beta_5 \overline{urban}_{ct}$$
$$+\beta_6 \overline{ecoz}_{ct} + \beta_7 \overline{land} + \delta_1 \overline{tariff}_{ct} + \overline{f}_{ct} + \overline{\lambda}_{ct} + \overline{\gamma}_{ct} + \overline{\varepsilon}_{ct} \qquad (2)$$

Equation (2) can be estimated via random- or fixed-effects estimators. The random-effects estimator generates consistent parameter estimates if the individual effects are uncorrelated with the other explanatory variables. The fixed-effects estimator is also consistent under this assumption, but is less efficient. Under the alternative hypothesis that

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function approach by using the standard of living measure (equivalent adult consumption) as the dependent variable in their regressions (Coloumbe and McKay, 2003).

[16] Pooling individuals across years has obvious advantages but generates a number of estimation issues regarding individual heterogeneity. It is likely that observations over time for the same individual will be more similar than observations across different individuals. This might be due to persistence in or unmodeled characteristics of household living standards. This is particularly pertinent to our analysis because, there are good reasons to think that unobserved factors may affect household welfare. So we allow f to vary across households to capture unmeasured or unobserved heterogeneity.

[17] See Appendix C for a detailed review of developments in the pseudo-panel econometric literature.

the individual effects are correlated with other explanatory variables, only the fixed-effects estimator is consistent. We will use both methods to estimate (2), and report diagnostics to evaluate the estimators. To examine whether the trade policy *changes* can be directly linked to *changes* in living standards we will also estimate a differenced model based on (2) as an alternative econometric specification.

The consumption (welfare) models (1) and (2) both assume preferences to be time separable. However, some recent studies have drawn attention to a class of time non-separable preferences, exhibiting habit formation or persistence. The distinctive characteristic of these models is that current utility depends not only on current consumption, but also on a habit stock formed from past consumption (see Fuhrer, 2000; and Deaton, 1992)[18]. In effect, equation (2) may be misspecified (dynamically) if dynamics really matter. The best solution would obviously be to directly model the dynamics; unfortunately this is very difficult without panel data. But failing to deal with the dynamics can cause serious problems. To test this we employ an alternative dynamic econometric specification, introducing the lagged dependent variable as an additional regressor.[19] Here, we follow Moffit's (1993) guidance to estimate the model using the underlying micro data (see Appendix C for details).

$$\begin{aligned}\ln w_{it} &= \alpha + \beta_1 age_{it} + \beta_2 age_{it}^{\,2} + \beta_3 hsize_{it} + \beta_4 educ_{it} + \beta_5 urban_{it} \\ &\quad + \beta_6 ecoz_{it} + \beta_7 land_{it} + \beta_8 \ln w_{it-1} + \delta_1 tariff_{jt} + \lambda_j + \gamma_t + \varepsilon_{it}\end{aligned} \tag{3}$$

Equation (3) imposes a uniform and linear restriction on the parameter δ_1; the effect of tariff on welfare. The implicit assumption of such an approach is that the welfare effect of tariffs is uniform for all households. However, in light of the discussions in Section 2, such an approach will be misspecified. The above specification may suffer from an un-modelled contingency in the relationship between tariffs and welfare. In other words, the assumption that all households would derive the same benefits from trade liberalisation is

[18] A dynamic specification could be justified on several grounds. First, households are likely to incur short-term costs resulting from trade liberalisation due to rigidities. It may also take time to adjust to any policy shocks such as switching jobs from industries whose wages are declining to ones where wages are rising.

unlikely; and it is not supported by the discussion in Section 2 and the evidence in Section 3. Equation (4) is a variant of (3) except now the structure explicitly allows the effect of tariffs on households to differ. We hypothesize that differences can, at least partially, be attributed to skill differentials among households and returns effects on education. The resulting estimating equation is of the form:

$$\begin{aligned}\ln w_{it} &= \alpha + \beta_1 age_{it} + \beta_2 age_{it}^{\;2} + \beta_3 hsize_{it} + \beta_4 educ_{it} + \beta_5 urban_{it} + \beta_6 ecoz_{it} \\ &+ \beta_7 land_{it} + \beta_8 \ln w_{it-1} + \delta_1 tariff_{jt} + \delta_2 Tariff_{jt} * Skill_{it} + \lambda_j + \gamma_t + \varepsilon_{it} \end{aligned} \tag{4}$$

where $Skill$ are three mutually exclusive educational dummies (unskilled, semi-skilled and skilled) denoting the skill category of the household. *Unskilled* labour comprises households whose head has at least primary education; *semi-skilled* labour includes households with secondary education; and *skilled* labour is represented by households with graduate heads. This identification strategy assumes that the tariff reductions during the 1990s affected households differentially according to their skill type. We are thus able to assess whether trade protection is beneficial for households regardless of the level of skill.

4.1 Construction of the Pseudo Panel Data

Following the seminal work of Deaton (1985), we can construct a pseudo panel and track cohorts of households through our two cross-sections. Cohorts can be defined in terms of a single characteristic or multiple characteristics. In our case, since we have only two cross-sections, if the cohorts contain a large number of households, the number of cohort-groups will be small and hence the cross-sectional dimension of the panel will not be large. Thus, we construct our pseudo-panel by grouping households into cohorts based on some common multiple characteristics varying by generation (age category of head), gender of head and household's region of domicile. Since we are interested in a panel of households with heads between the ages of 18 to 64 and we have two cross-sections that are seven years apart then for the first cross-section (1991/92) the sample only includes households whose heads are aged 18 to 57, while the second cross-section (1998/99) only

[19] A significant coefficient on the lagged dependent variable is evidence that the previous models were mis

includes households with heads aged 25 to 64 so that all are in the normal working span in both surveys. Note that we add seven years to the age limits as we move to the next cross-section; this allows the households to "age" over time. We used 5-year bands in defining the generational cohorts resulting in eight birth cohorts constructed for each region in each survey year. For example, the first age cohort studied here was aged 18-22 in 1991/92 and 25-29 in 1998/99 (see Table C1 in Appendix C for details). Households whose heads are of these ages and found in the relevant cross-sections are pooled to form the pseudo cohorts. Although the actual households surveyed will differ in each survey year, they will be representative of the full cohort in the population.

5. ECONOMETRIC RESULTS

In this section we discuss the econometric results, focusing on estimates of equations (2) to (4). First, we estimated equations (1) - (3) without controlling for industry-specific effects. The results are reported in Tables B1 and B2 in Appendix B. The effects of tariffs on welfare are negative for all the specifications. It is possible that these results in Table B1 and B2 exaggerate the effect of tariffs on income; other factors, such as industry effects are potentially important. To examine if tariff effects can be accounted for by industry of employment, we re-estimate all the regressions but this time we include industry dummies; the effect of tariffs is reversed controlling for industry fixed effects.[20] This suggests that unobserved industry heterogeneity was responsible for the negative tariff effect in the previous regressions. Thus, the rest of the analysis and discussions in this paper refers to the regressions with controls for industry heterogeneity.[21]

We now turn to an in-depth discussion of the regression results. Our main findings are reported in Tables 5 and 6. For a start, Table 5 reports the simple impact of the degree of openness on welfare. The first column lists the results for the case where we apply conventional OLS, based on equation (1), to the pooled cross-sections. Columns 2 to 4, on

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(under) -specified.

[20] Other authors have found similar results. Attanasio *et al*. (2004), for example, estimates a positive tariff effect on industry wage premia only after controlling for unobserved sectoral heterogeneity. In their experimentation without industry dummies the tariff-wage effect turned negative.

[21] A Wald test of the hypothesis that the effects of the industry dummies are simultaneously equal to zero was rejected at the 0.1 level or better.

the other hand, are based on the pseudo panel equation (2). Columns 2 and 3 report random-effects and fixed-effects results respectively. Even though the key message is the same across these two models, we employed the Hausman specification test and report the diagnostic results in Table BA in Appendix B.[22] To examine whether the trade policy changes can be directly linked to changes in living standards we also estimate the first-difference model in column 4 based on (2). This specification could also mitigate the potential for any spurious correlation between tariffs and welfare.

Table 5: Trade Protection and Household Welfare: Evidence from Static Regressions

	Cross-Sectional	**Pseudo Panel**		**Pseudo Panel**
	Pooled OLS	*Random Effects*	*Fixed Effects*	*Differenced*
	(1)	**(2)**	**(3)**	**(4)**
Agehead	-0.022***	-0.038***	-	-
	(0.005)	(0.011)		
$Agehead^2$	0.001***	0.001***	-	-
	(0.001)	(0.001)		
Hsize	-0.109***	-0.085***	-0.096***	-0.096***
	(0.003)	(0.014)	(0.025)	(0.025)
Urban	0.268***	0.310***	0.332**	0.332**
	(0.016)	(0.077)	(0.146)	(0.140)
Basic	0.135***	0.103	0.126	0.126
	(0.016)	(0.087)	(0.165)	(0.193)
Secondary	0.360***	0.434	-0.787	-0.787
	(0.029)	(0.293)	(0.562)	(0.723)
Post-sec	0.344***	0.414	0.303	0.303
	(0.033)	(0.311)	(0.511)	(0.542)
Tertiary	0.768***	1.880**	1.956	1.956
	(0.085)	(0.892)	(1.391)	(1.845)
Land	0.006***	-0.009*	-0.013	-0.013
	(0.001)	(0.005)	(0.010)	(0.015)
Forest	0.017	0.110*	0.026	0.026
	(0.015)	(0.064)	(0.194)	(0.128)
Savannah	-0.187***	-0.227***	0.169	0.169
	(0.019)	(0.062)	(0.372)	(0.350)
Tariff	0.010**	0.056***	0.068**	0.068**
	(0.005)	(0.020)	(0.027)	(0.029)
GLSS 4	0.127***	0.154***	0.185***	-
	(0.015)	(0.047)	(0.058)	
Constant	14.798***	15.818***	14.948***	0.185***
	(0.135)	(0.897)	(1.498)	(0.050)
Industry dummies	Yes	Yes	Yes	Yes

[22] The test statistic equals 21.16 (probability of 0.98). This clearly fails to reject the null, at the 0.05 level of significance, that the unobserved heterogeneity is uncorrelated with the regressors, i.e. it finds that the random effects estimates are not significantly different from the fixed effects estimates. The more efficient random effects specification is therefore the preferred one.

N0. of Obs	7834	310	310	152
R-squared	0.42	0.74	0.35	0.32

Note: Robust standard errors in parentheses, * denotes significant at 10%; ** denotes significant at 5%, *** denotes significant at 1%.

Table 6: Trade Protection and Household Welfare: Evidence from Dynamic Regressions

	(1)	(2)
Lagged Welfare	0.386**	0.386**
	(0.156)	(0.156)
Agehead	0.036**	0.035**
	(0.015)	(0.015)
Agehead^2	-0.001**	-0.001**
	(0.001)	(0.001)
Hsize	-0.063***	-0.063***
	(0.018)	(0.018)
Urban	0.067	0.067
	(0.070)	(0.070)
Basic	0.066***	0.096***
	(0.023)	(0.028)
Secondary	0.186***	0.227***
	(0.065)	(0.069)
Post-sec	0.195***	0.237***
	(0.062)	(0.065)
Tertiary	0.391**	0.447***
	(0.156)	(0.158)
Land	0.004***	0.004***
	(0.001)	(0.001)
Forest	0.040*	0.039*
	(0.022)	(0.022)
Savannah	0.029	0.028
	(0.031)	(0.031)
Tariff	0.009*	0.012**
	(0.005)	(0.005)
Tariff x Skill		-0.002*
		(0.001)
GLSS 4	0.093***	0.093***
	(0.033)	(0.033)
Constant	8.057***	8.042***
	(2.473)	(2.473)
Industry dummies	Yes	Yes
No. of Observations	7834	7834
R-squared	0.45	0.45

Note: Robust standard errors in parentheses, * denotes significant at 10%; ** denotes significant at 5%, *** denotes significant at 1%. Regressions include controls for cohort group (dummies) suppressed here for brevity.

The effects of protection on welfare are positive and significant in all regressions in Table 5. In other words, holding other factors constant, the pseudo panel econometric evidence presented here suggests that welfare is higher (from which we infer that poverty is lower) in households (or cohorts) employed in protected sectors (sheltered from competition). The coefficient on *Tariff* implies that increasing protection in a particular sector raises consumption expenditures (or incomes) in that sector. The corollary that reducing tariffs in previously protected sectors lowers incomes (or welfare) in those sectors is equally supported by the first-difference model in column 4.

Although the regressions in Table 5 provide interesting results, we can be sceptical about their static nature and the linearity (homogeneity) restriction on the coefficient of *Tariff*. Thus, Table 6 presents results based on the dynamic models (3) and (4). The specifications as in column 1 of Table 6 and its variant as in column 2 are dynamically specified (with the lag of the dependent variable, log welfare, as a regressor) and estimated using 2SLS applied to RCS data as reviewed in Appendix C. Moreover, column 2 presents the estimates of the differential impact of the reforms on unskilled and skilled labour households. In column 2, based on equation (4), *Tariff* is interacted with the *Skill* dummy to show the differential effect of trade protection on households characterised by different levels of education.[23]

As discussed already, the main problem we face in estimating (4) is that the true value of the lagged dependent variable (lagged welfare), is unobserved because the same individuals are not tracked over time. Following Moffit (1993), however, the regressions in Table 6 are estimated by regressing the dependent variable (welfare) on the time-invariant explanatory variables using the observations in the first cross-section (1991/92). We then obtain the predicted dependent variable from the OLS estimation. In the second stage the predicted dependent variable is substituted in the original model (4) as the lagged dependent variable and estimated by OLS using all observations in both cross-sections; on the assumption that the (predicted) lagged dependent variable is asymptotically uncorrelated with the error term.[24]

[23] The assumption of homogeneity implies that the coefficient on the interactive term should equal zero. This restriction is obviously rejected as indicated by the significant coefficient on the interactive term. This suggests that the regressions in Table 5 may suffer from heterogeneity that is not modelled.
[24] We test for the sensitivity of our results to this assumption in the robustness checks (below).

Interestingly, we still find robust evidence regarding the effects of tariffs on poverty. In both regressions (Table 6) the average welfare responds positively to tariffs, so that tariff reductions would lead to a decline in welfare. In other words, welfare would be lower in households employed in protected sectors which were exposed to import competition. This finding supports the interpretation that incomes fell most in those industries where openness increased the most. Thus, we again find a positive and statistically significant correlation between trade protection and household welfare. Although the magnitude of the tariff coefficient changes, the positive and statistically significant relationship between tariffs and welfare is robust to different specifications. The estimated effect of protection on welfare drops however from an average of about 0.064 in columns 2 to 4 of Table 5, to 0.009 and 0.012 in columns 1 and 2 respectively of Table 6. These results suggest, in the case of Ghana, that trade policy reforms had a significant effect (albeit marginally) on household welfare. Households whose heads work in industries with the largest tariff reductions (mainly the agriculture and allied sectors) would tend to experience a decline of their welfare (income) relative to the economy-wide average.[25] The evidence seems to suggest that tariffs may protect incomes of households employed in relatively protected sectors. This implies that some of the economic rents are shared with labour, so that liberalisation could reduce incomes and potentially increase poverty (in protected sectors). Whether inequality increased depends on whether the sectors with the largest tariff reductions were the ones in which the poor are located, relatively intensively. Anecdotal evidence and the results contained in the descriptive analysis of this paper, however, point to the contrary. The poor in Ghana are predominantly rural, unskilled and employed relatively intensively in agriculture (mostly as landless peasant food crop farmers). It is for this reason that the results in Table 7 are especially important.

In Table 7 we show the three skill types of all households in our regressions, along with their actual welfare as reported in the data and the predicted welfare from the regression in column 1 of Table 6. In addition, we estimate how much of the variations in within-

[25] The only exceptions are households engaged in export farming (predominantly cocoa farmers). Aryeetey (2005) has argued, however, that one of the reasons why the export farming sector performed relatively better than food crop farmers is due to the fact that in the face of the severe agricultural import liberalization,

household welfare is explained by trade policy. Overall, the model explains reasonably well the experience of all households irrespective of the skill type. The unexplained welfare (residual) is negligible, ranging between 0.3% and 5.5% in absolute terms.

Table 7: Contribution of Trade Protection to Household Welfare

	1991/92			**1998/99**		
	Skill Type of Household			**Skill Type of Household**		
	Unskilled	*Semi-*	*Skilled*	*Unskilled*	*Semi-*	*Skilled*
Actual Welfare (log)	13.875	14.456	14.324	13.981	14.586	14.482
Predicted Welfare (log)	13.870	14.480	14.378	13.984	14.571	14.458
Residual	0.004	-0.024	-0.055	-0.003	0.016	0.025
Contribution of Tariffs to Welfare	0.200	0.184	0.182	0.176	0.168	0.168
Number of Observations	3016	190	144	3869	294	321

Note: Authors' calculations based on regression in column 1 of Table 6.
Figures are simple averages over all households in each skill type except tariff which is over households in traded sectors only.

The first main message from Table 7 is that for all the households in traded sectors the contribution of protection to welfare is positive. Second, the results corroborate the non-linear specification employed in column 2 of Table 6 (the model with the interactive term). We find that the contribution of tariffs to welfare is relatively higher (20%) for unskilled households. Without any special safety nets or complementary policies one can expect that trade liberalisation, alone, would have disproportionate negative consequences for households in this skill type, *ceteris paribus*. Finally, the results reveal, that over the period of seven years the contribution of tariffs to welfare has fallen for all skill types whilst average welfare for each skill type has increased slightly. This seems to suggest, perhaps unsurprisingly, that in the medium to long-run there appears to be a negative relationship between trade protection and welfare. If this were the case, it would be good news for free trade protagonists. The second and final messages from this table are the basis for the subsequent empirical analysis in this paper. First, we investigate further the apparent non-linear tariff-welfare relationship. Then, given the inherent dynamics in our model we estimate the long-run welfare responses to trade protection.

the export farmers have been benefiting from governmental support in terms of technical training and other export promotion packages.

5.1 Non-linearity

It appears reasonable to expect that trade protection, and trade liberalization, will impact differentially, either by direction or magnitude, on households with different levels of education. To examine how the effect of trade liberalization on households may vary by education, we have hypothesized a potential contingency in the relationship between protection (liberalization) and welfare. To attempt to capture this contingency, we introduced an interaction term between *Tariff* and *Skill* which is a categorical dummy variable constructed from the highest education completed dummies. The interaction term is meant to capture the non-linearity in the impact of trade policy on poverty, in order to ascertain whether the impact of greater openness is borne disproportionately by different skill groups.[26] Evidence of a contingent relationship is provided by a significant coefficient on the interaction term suggesting an un-modelled contingency bias in the results discussed previously.

The results reported in column 2 of Table 6 reveal a significant interaction effect under which the marginal impact of tariffs on welfare is decreasing in skill. We find that the positive tariff effect applies to all households but is more pronounced for less skilled households, suggesting that greater openness is likely to be associated with significantly lower returns to households with lower levels of education (the unskilled). This leads to the inference that unskilled households in highly protected industries enjoy relatively higher welfare than they otherwise would. Hence trade liberalization will worsen their welfare disproportionately, *ceteris paribus*. It is therefore reasonable to conjecture that only skilled households (because they are more educated and more mobile) would have benefited from trade liberalization in the 1990s. This evidence on the differential impact of trade protection on poverty is consistent with our earlier descriptive results concerning the finding that the rural, food crop farmers and non-farm self-employed, all of whom are relatively unskilled, benefited the least from the trade reforms in the 1990s. Trade liberalization in Ghana seems to accord with an increase in income inequality in favour of skilled households.

These results imply that the impact of trade protection on household welfare is a function both of the level of restriction and of the level of education (skill). To evaluate this conditional hypothesis, we use the three values for *Skill* (1 for unskilled; 2 for semiskilled; 3 for skilled) to compute the marginal effects of trade policy and report the results in the first row of Table 8.

Table 8: Marginal and Long-run effects of Trade Protection on Welfare

	Unskilled	**Semi-skilled**	**Skilled**
Marginal effects	0.01 (2.01)**	0.009 (1.92)*	0.006 (1.20)
Long-run effects	0.016 (1.45)	0.01 (1.45)	0.01 (1.11)

Note: Absolute t-ratios in parentheses, * denotes significant at 10%; ** denotes significant at 5%.
In order to test the hypothesis that the simple slope (marginal effect of tariffs) differs from zero, we approximate the standard error of the simple slope by the following equation: $s_b = sqrt\left[s_{11} + 2Zs_{12} + Z^2 s_{22}\right]$, where s_{11} is the variance of the tariff coefficient (i.e., the squared standard error of δ_1), s_{22} is the variance of the interaction coefficient (i.e., the squared standard error of δ_2) and s_{12} is the covariance of the two. These values are obtained from the asymptotic covariance matrix based on our regression model in Table 6 column 2.

From equation (4), the derivative of *welfare* with respect to *Tariff* is calculated as

$$\frac{\partial welfare}{\partial Tariff} = \delta_1 + \delta_2\left(Skill\right) \tag{5}$$

Evaluated at *Unskilled* and *Semi-skilled*, we find a positive and statistically significant tariff effect. However, evaluated at *Skilled* the marginal effect of *Tariff* becomes statistically insignificant (i.e. can be assumed to be zero). Thus, the regression indicates that the derivative of welfare with respect to tariffs is a decreasing and linear function of the level skill. We know from the fact that the coefficient on the interaction term is negative that the positive effect of trade protection declines as the level of skill increases. Consequently, the potential adjustment costs resulting from any given trade policy reforms will not be universal across different skill groups. Thus, for two households with similar characteristics, affiliated to the same sector (and thus facing similar tariffs) but belonging to different skill groups (unskilled and skilled), a tariff reduction in that sector will have different effects on their respective welfare. Skilled households stand to benefit more (or

[26] Alternatively, we could simply conduct separate regressions for households in different skill categories. However, this approach will impose too much restriction on the data and will also not permit us to explore how the marginal effect of trade policy varies for more-skilled and less-skilled households.

lose less) than unskilled households. Alternatively, unskilled households will benefit the least relative to skilled households.

5.2 Long-run Effects of Trade Protection

The analysis so far has been restricted to the short run impact of trade policy. While the short-run is definitely important and merits analysis, many economic policies have important long-run perspectives which equally deserve scrutiny. Most often, these long-run impacts are ignored by researchers and policy analysts. This is partly because of data constraints or because the electorates only care about the short-run costs and benefits of public policy. However, to the extent that it is possible, we need to investigate the long-run impacts as well. In our empirical application, we are interested in knowing whether the long-run effects of trade policy are the same as the short-run consequences already documented. Specifically, we want to see whether the positive impact of tariffs on welfare weakens over time. Fortunately, the dynamic specifications employed in Table 6 allow us to explore this. The estimated significant coefficient on the lagged dependent variable is 0.386 with a standard error of 0.156. This suggests that past shocks to household welfare do affect current levels of welfare, above and beyond the influence of household-specific characteristics. The estimated tariff coefficient is 0.012 with a standard error of 0.005. This estimate divided by one minus the coefficient estimate on the lagged dependent variable yields the *long-run* effect of trade protection on welfare. The last row of Table 8 reports this long-run impact for all three skill groups. There is an interesting twist. None of the long run tariff effects is statistically distinguishable from zero. In other words, conditional on controls for the persistence of household welfare the positive and significant tariff effect disappears. Hence, it seems reasonable to speculate that the arguments for protection are valid (especially for poor unskilled labour households) so long as the short-run is the period of interest. In the long-run, however, it is highly unlikely for any household, irrespective of the skill type and industry affiliation, to benefit from protectionism. Trade liberalization has therefore a potential role in enhancing welfare in the long run.

Results for the other control variables are also of interest. Household welfare correlates positively and significantly with land value. As expected, household size correlates negatively and significantly with welfare. The education variables show the expected pattern. All the estimated coefficients are positive and statistically significant, indicating that, other things being equal, all levels of education (relative to no education) of the household head improve welfare. It turns out that the returns to having progressively higher education are larger. The strong positive effect of education on welfare is increasing with the level of completed education of the household head. The incremental gain in welfare is smallest for households with heads with basic education and largest for graduate headed households. Note that the effects of post-basic education (i.e., secondary, post-secondary and tertiary) are quantitatively the largest of all included explanatory variables. Hence, education emerges as the fundamental household characteristic determining the probability that a household experiences a reduction (or improvement) in welfare, *ceteris paribus*.

6. FURTHER ROBUSTNESS CHECKS

To verify our main findings, we now turn to a number of robustness checks. Our first check was to take seriously the measurement error problem raised in the pseudo panel literature and reviewed in Appendix C. We are interested in finding out whether the results are sensitive to the construction of the pseudo panel. With an average cell size of 52 we can be worried that the measurement error problem can be an issue in the results in Table 5. However, since the main conclusions in this paper are based on Table 6 in which the regressions are based on the underlying micro data (not on cell means), we can safely ignore the measurement error problem. Nevertheless, we follow most researchers in this field (upon the advice by Verbeek and Nijman, 1993) and divide the sample into a smaller number of cohorts to ensure that observations per cell are reasonably large. To do this, we construct a new pseudo panel by taking 10-year generation bands while maintaining the regional (10) and gender (2) categories.[27] Cohorts are defined by the interaction of four age intervals (GLSS 1991/92: 18-27, 28-37, 38-47 and 48-57; GLSS 1998/99: 25-34, 35-

[27] The choice of 10-year intervals is essentially arbitrary, but meets the requirements for the cell sizes to be reasonably large (on average) so that the measurement error problem discussed previously is negligible.

44, 45-54 and 55-64), two gender categories (male and female) of head and ten geographic regions (see Table C2 in Appendix C for details). For example, now the first cohort here is aged 18-27 in 1991/92 and 25-34 in 1998/99. By so doing, the average number of observations per cell increases to 104 at the expense of a relatively small total number of observations (a potential of 160 but 148 realized). Tables B3 to B5 in Appendix B replicate all the regressions in Tables 5 and 6 using this new data. In all cases, we find that cohort selection issues are not driving the results. Our results remain largely unaltered. Both the signs and statistical significance of the coefficients are preserved in most cases. Thus the model parameters are robust in that they show little sensitivity to changes in the data construction. We still find convincing evidence of a positive and statistically significant correlation between tariffs and welfare which is contingent on skill (human capital). In fact, the orders of magnitude of the estimated tariff coefficient have actually become larger.

Next, we used the estimator proposed by Verbeek and Vella (2005) as a robustness check on using Moffit's version of estimating dynamic models from RCS. Our aim is to check if failure to instrument *Tariff* and the lag of the dependent variable as the authors suggest affected the estimated parameters. In effect, we relax the assumption that the (predicted) lagged dependent variable is uncorrelated with the prediction error. Essentially, we estimated (4) using standard IV methods with cohort dummies interacted with time dummies, serving as instruments for both lagged welfare and tariffs. The results are presented in Table B6 in Appendix B. We found no big difference in the estimated coefficients. In other words we did not have any major changes in significance or signs of the estimated coefficients in Table 6. In fact, the estimated coefficients on tariffs and the interaction term becomes stronger and both are significant at the 1% level. Hence, our results are not driven by model specification and the choice of estimator.

We also test for the joint significance of the industry fixed effects. The null hypothesis of the joint insignificance of the industry fixed effects (i.e., that each of the coefficients for the industry dummies is not significantly different from zero) is safely rejected for all relevant specifications. The specifications controlling for unobserved industry heterogeneity is thus retained as our preferred models. The fit of the models are good, with R^2 ranging from 0.32 to 0.74. With only a few exceptions, the signs on the parameters are as expected, and the relative magnitudes of the parameters are reasonable.

Finally, we performed diagnostic tests for influential observations to confirm that the parameter estimates are not unduly influenced by a small subset of observations. Our examination of the data for the presence of outliers, high-leverage points or influential observations using the DFFITS statistic (Besley *et al.*, 1980) flagged three observations as high-leverage and influential.[28] However, the omission of all three observations does not affect the fit and hence the estimated coefficients.

7. CONCLUSIONS

In this paper, we have presented one of the first direct microeconometric studies of the impact of trade protection on household income in Ghana. Tariff measures at the two-digit ISIC level were matched to household survey data for 1991/92 and 1998/99 to represent the tariff for the industry in which the household head is employed. We examined the possibility that the effect of protection on income might not be uniform across households characterized by different education (skill) levels. We have presented both descriptive and econometric evidence to show that trade policy reforms in Ghana during the 1990s could have resulted in increases in poverty among certain sections of the population, especially the rural unskilled labour households. Unskilled households, predominantly employed in Agriculture, would experience the largest increases in poverty. This is consistent with the

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[28] Suppose X denotes the matrix of explanatory variables in our model and x_i the ith element of X, containing observations on household i. Letting $P = X\left(X'X\right)^{-1}X'$ denote the associated *hat* matrix, the leverage statistic for observation i, which is the ith diagonal element of P, is $p_{ii} = x_i'\left(X'X\right)^{-1}x_i$, $i = 1, 2, ..., N$. This measures the distance of x_i from the centre of mass from the other rows of P. If r_i denotes the studentized residual of observation i, the $DFFITS_i$ statistic would be $DFFITS_i = r_i\sqrt{\dfrac{p_{ii}}{1-p_{ii}}}$. In our empirical application, we identified potential outliers as observations with associated $DFFITS$ statistic $-1 < p_{ii} > 1$.

observations made by Aryeetey and McKay (2004) that the poorest of the poor participated much less in the growth and poverty reduction over this period.

The econometric results confirm our previous descriptive findings and suggest that higher tariffs are associated with higher incomes for households employed in the sector, implying that some of the economic rents are shared with labour, so that liberalisation could reduce incomes and potentially increase poverty, at least in the short run, but with differing effects across skill groups. We find that the positive effect of protection is disproportionately greater for low skilled labour households, suggesting an erosion of welfare of unskilled labour households would result from trade liberalization. In the short-run, all households regardless of skill type would have lost out from trade liberalization, but the poor unskilled households (because they are sector-specific and less mobile) would lose disproportionately. The results suggest that within the same sector, a trade reform may lead to differing impacts on households with similar attributes but different skills. Moreover, education emerged as the fundamental household characteristic determining the probability that a household experiences poverty, *ceteris paribus*. From a policy standpoint, we conclude that contemplating trade liberalization without recognizing the complementary role of human capital investment may be a sub-optimal policy for the poor, at least in the short-run. Maximizing the potential long-term benefits and minimizing the short-run costs of trade liberalization would therefore require active interventions to *weather the storm* with the poor in mind. A *laissez-faire* approach can be disastrous.

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Appendix A: Data and Descriptive Statistics

Table A1: Inter-Industry Trade Protection (Liberalisation) during the 1990s

Industry Classification(26)	**Tariff (%)**		**Tariff Change (%)**
	1992/93	**1999/00**	
Traded Sectors (19)			
Agric, Forestry and Fishing (3) *of which*			
Agriculture crop & Livestock	23.2	19.44	-16.18
Forestry & Logging	24.77	20	-19.27
Fishing	20.34	13.97	-31.36
Average (unweighted)	*22.77*	*17.8*	*-22.27*
Manufacturing (14) *of which*			
Food	18.94	24.94	31.63
Beverages	20.45	21.43	4.76
Furniture	19.73	27.84	41.1
Electrical	12.63	10.86	-14.08
Metals	7.89	11.03	39.83
Chemicals	10.61	12.08	13.84
Plastics	14.39	17.17	19.34
Footwear	19	20	5.26
Textiles	21.35	23.04	7.93
Wood	18	16.89	-6.16
Apparel	24.44	22.22	-9.09
Printing	20	23.33	16.67
Rubber	10	10	0
Other manufacturing	11.21	13.76	22.75
Average (unweighted)	*16.33*	*18.19*	*12.41*
Mining & Quarrying	9.77	11.64	19.14
Utilities	12.14	10.71	-11.76

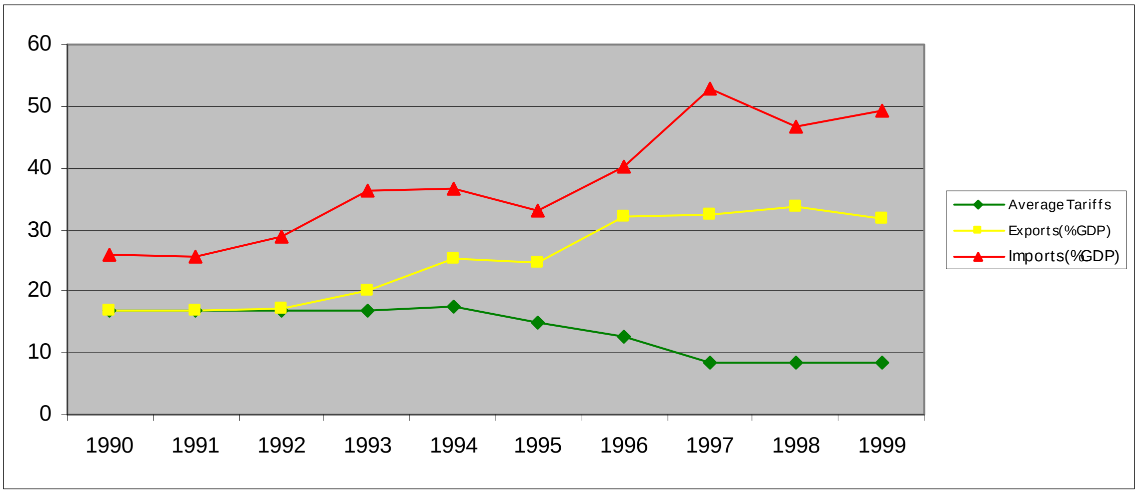
Source: Authors' calculations using SITC 2-digit level tariff data from the (UNCTAD) TRAINS Database.
Note: The other seven Non-traded sectors including Trading, Construction, Restaurant & hotel, Transport & communication, Financial services, Other services and Community & social care were all assigned a tariff of zero.

Table A2: Industry Employment Shares by Skill Levels

Industry Name	1991/92 Share of different skill levels in industry			1998/99 Share of different skill levels in industry			1991/92 Share of industry in total employment	1998/99 Share of industry in total employment
	Unskilled	*Semi-*	*Skilled*	*Unskilled*	*Semi-*	*Skilled*		
Agriculture crop & Livestock	0.982	0.014	0.004	0.969	0.022	0.010	0.481	0.486
Forestry & Logging	0.903	0.065	0.032	0.833	0.056	0.111	0.009	0.004
Fishing	0.960	0.000	0.040	0.988	0.000	0.012	0.015	0.018
Food	0.983	0.017	0.000	0.967	0.016	0.016	0.035	0.041
Beverages	0.957	0.000	0.043	0.903	0.065	0.032	0.007	0.007
Furniture	0.885	0.038	0.077	0.895	0.053	0.053	0.008	0.008
Electrical	0.667	0.000	0.333	0.000	1.000	0.000	0.001	0.000
Metals	0.889	0.111	0.000	0.810	0.143	0.048	0.005	0.005
Chemicals	1.000	0.000	0.000	0.500	0.500	0.000	0.001	0.000
Plastics	1.000	0.000	0.000	0.500	0.000	0.500	0.001	0.001
Footwear	1.000	0.000	0.000	0.818	0.182	0.000	0.001	0.002
Textiles	0.800	0.200	0.000	0.679	0.214	0.107	0.006	0.006
Wood	0.833	0.000	0.167	0.826	0.087	0.087	0.002	0.005
Apparel	0.944	0.037	0.019	0.882	0.082	0.035	0.016	0.019
Printing	0.571	0.429	0.000	0.545	0.273	0.182	0.002	0.002
Rubber	0.857	0.143	0.000	0.730	0.135	0.135	0.006	0.008
Other manufacturing	0.968	0.000	0.032	0.769	0.154	0.077	0.009	0.012
Mining & Quarrying	0.636	0.273	0.091	0.733	0.133	0.133	0.003	0.003
Utilities	1.000	0.000	0.000	0.500	0.500	0.000	0.001	0.000
Trading	0.931	0.063	0.006	0.877	0.085	0.038	0.142	0.147
Construction	0.931	0.056	0.014	0.793	0.103	0.103	0.021	0.026
Restaurants & Hotel	0.955	0.000	0.045	0.889	0.056	0.056	0.007	0.004
Transport & Communication	0.879	0.093	0.029	0.800	0.103	0.097	0.042	0.039
Financial Services	0.357	0.429	0.214	0.286	0.457	0.257	0.004	0.008
Other Services	0.796	0.122	0.082	0.632	0.211	0.158	0.015	0.013
Community & Social	0.632	0.163	0.206	0.540	0.138	0.322	0.160	0.134
Total	0.900	0.057	0.043	0.863	0.066	0.072	1.000	1.000

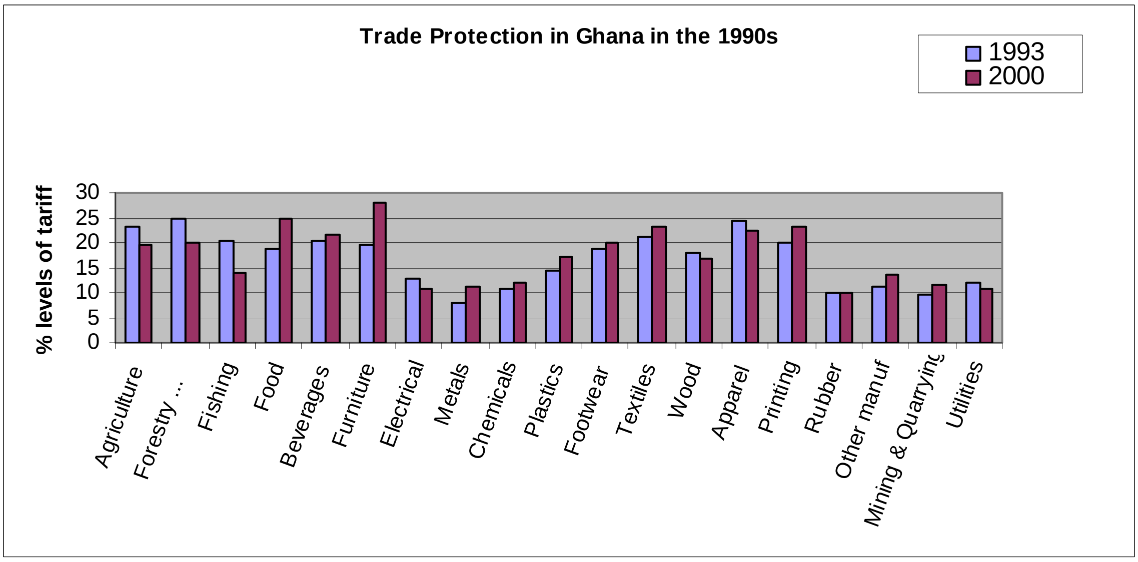
Source: Authors' calculations from GLSS surveys. These are the 26/68 sectors for which we successfully matched households by the main employment of head.

Figure A1: Trade Policy and Performance in the 1990s



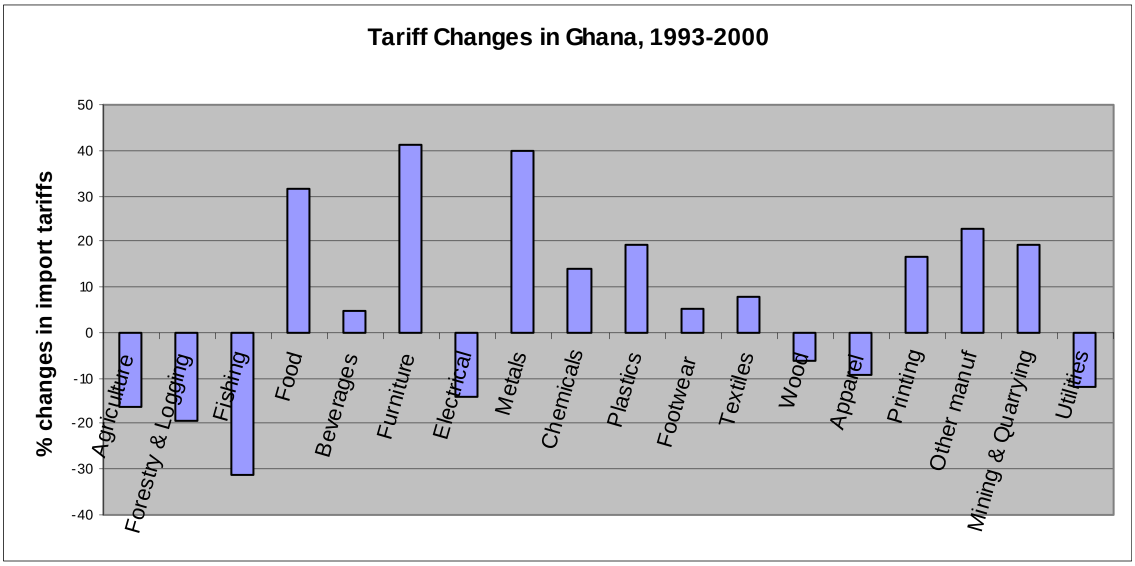
Source: Authors' with data from World Bank, *World Development Indicators* (WDI) 2002 CD-ROM.

Figure A2: The Pattern of Trade Protection in Ghana during the 1990s



Note: These are all the 19 tradable sectors in our data. There are seven non-traded sectors with tariffs coded as zero.

Figure A3: The Pattern of Trade Liberalization in Ghana during the 1990s



Note: These were the 18 tradable sectors where tariff changes occurred. The other tradable sector is rubber where the tariff change was zero.

Appendix B: Alternative Estimation and Results for Robustness Checks

Table BA: Hausman Specification Test

Explanatory variables	Coefficients		
	Fixed	***Random***	***Difference***
Household size	-0.096	-0.085	-0.011
Urban	0.332	0.310	0.022
Basic education	0.126	0.103	0.023
Secondary	-0.787	0.434	-1.220
Post-Secondary	0.303	0.414	-0.110
Tertiary education	1.956	1.880	0.076
Tariff	0.068	0.056	0.012
Forest	0.026	0.110	-0.085
Savannah	0.169	-0.227	0.396
Land	-0.013	-0.009	-0.004
GLSS 4	0.185	0.154	0.031

Note: The regressions included 25 industry dummies suppressed here.

Test: Ho: difference in coefficients not systematic

chi2 (36) = (b-B)' [S^ (-1)] (b-B), S = (S_fe - S_re)

= 21.16

Prob>chi2 = 0.9767

Regressions with no Controls for Industry Fixed Effects

Table B1: Trade Protection and Household Welfare: Linear (Static) Regressions

	Cross-Sectional	**Pseudo Panel**		**Pseudo Panel**
	Pooled OLS	*Random Effects*	*Fixed Effects*	*Differenced*
	(1)	**(2)**	**(3)**	**(4)**
Agehead	-0.020***	-0.026**	-	-0.065
	(0.005)	(0.012)		(0.046)
$Agehead^2$	0.001***	0.001**	-	-0.001
	(0.001)	(0.001)		(0.001)
Hsize	-0.109***	-0.092***	-0.083***	-0.110***
	(0.003)	(0.019)	(0.025)	(0.032)
Urban	0.315***	0.395***	0.387**	0.529***
	(0.016)	(0.102)	(0.157)	(0.133)
Basic	0.155***	0.121	0.092	0.342**
	(0.016)	(0.089)	(0.158)	(0.171)
Secondary	0.399***	0.278	-0.138	0.083
	(0.029)	(0.238)	(0.503)	(0.440)
Post-Sec	0.381***	0.651**	0.849*	0.462
	(0.032)	(0.286)	(0.497)	(0.444)
Tertiary	0.842***	1.968**	2.121*	3.605***
	(0.084)	(0.772)	(1.138)	(1.043)
Tariff	-0.010***	-0.013**	-0.005	-0.016**
	(0.001)	(0.006)	(0.009)	(0.008)
Forest	-0.007	0.147***	0.028	-0.131
	(0.015)	(0.053)	(0.161)	(0.182)
Savannah	-0.217***	-0.281***	0.095	0.293
	(0.019)	(0.059)	(0.290)	(0.252)
Land	0.004***	-0.012	-0.017	0.020
	(0.001)	(0.008)	(0.012)	(0.013)
GLSS 4	0.112***	0.069*	0.091**	(dropped)
	(0.014)	(0.038)	(0.040)	
Constant	14.772***	14.720***	13.649***	0.098
	(0.103)	(0.339)	(0.423)	(0.497)
Industry dummies	No	No	No	No
No. of Obs.	7834	310	310	152
R-squared	0.41	0.72	0.39	0.58

*Note: Robust standard errors in parentheses, * denotes significant at 10%; ** denotes significant at 5%, *** denotes significant at 1%. Regressions include controls for cohort group (dummies) suppressed here for brevity.*

Table B2: Trade Protection and Household Welfare: Linear (Dynamic) Regressions

	(1)	(2)
Lagged welfare	0.390**	0.390**
	(0.157)	(0.157)
Agehead	0.039***	0.039***
	(0.015)	(0.015)
Agehead^2	-0.001***	-0.001**
	(0.001)	(0.001)
Hsize	-0.063***	-0.063***
	(0.018)	(0.018)
Urban	0.110	0.110
	(0.071)	(0.074)
Basic	0.073***	0.073***
	(0.023)	(0.025)
Secondary	0.199***	0.199***
	(0.066)	(0.071)
Post-sec	0.217***	0.216***
	(0.062)	(0.076)
Tertiary	0.426***	0.426***
	(0.157)	(0.159)
Tariff	-0.009***	-0.009**
	(0.001)	(0.004)
Land	0.002	0.002
	(0.001)	(0.002)
Forest	0.024	0.024
	(0.022)	(0.023)
Savannah	0.017	0.017
	(0.031)	(0.032)
GLSS 4	0.067**	0.067**
	(0.032)	(0.033)
Constant	7.971***	8.316***
	(2.497)	(2.450)
Industry dummies	No	No
No. of Obs.	7834	7834
R-squared	0.44	0.44

*Note: Robust standard errors in parentheses, * denotes significant at 10%; ** denotes significant at 5%, *** denotes significant at 1%. Regressions include controls for cohort group (dummies) suppressed here for brevity.*

Sensitivity Analysis –Regression Results

Regressions from the 2nd Pseudo Panel with Average Cell Size of 105.

Table B3: Trade Protection and Household Welfare: Evidence from Static Regressions

	Cross-Sectional *Pooled OLS*	*Random Effects*	Pseudo Panel *Fixed Effects*	Pseudo Panel *Differenced*
	(1)	**(2)**	**(3)**	**(4)**
Agehead	-0.022***	-0.066***	-	-
	(0.005)	(0.016)		
Agehead^2	0.001***	0.001***	-	-
	(0.001)	(0.001)		
HH size	-0.109***	-0.034	-0.084**	-0.106**
	(0.003)	(0.022)	(0.037)	(0.040)
Urban	0.267***	0.385***	0.171	0.405
	(0.017)	(0.130)	(0.263)	(0.292)
Basic	0.134***	0.100	0.000	0.215
	(0.016)	(0.143)	(0.348)	(0.442)
Secondary	0.359***	0.500	-1.283	-1.445
	(0.029)	(0.533)	(1.138)	(1.208)
Post-Sec	0.342***	0.416	0.053	0.058
	(0.033)	(0.513)	(0.986)	(1.096)
Tertiary	0.766***	0.050	2.070	1.954
	(0.085)	(1.759)	(2.854)	(4.356)
Land	0.006***	0.001	-0.040**	-0.051**
	(0.001)	(0.009)	(0.018)	(0.020)
Forest	0.017	0.107	0.274	0.194
	(0.015)	(0.086)	(0.308)	(0.290)
Savannah	-0.185***	-0.230**	0.440	0.479
	(0.019)	(0.093)	(0.607)	(0.447)
Tariff	0.010**	0.079***	0.090**	0.106***
	(0.005)	(0.028)	(0.040)	(0.036)
GLSS 4	0.128***	0.202***	0.203**	-
	(0.015)	(0.063)	(0.087)	
Constant	14.804***	15.537***	12.376***	0.240***
	(0.135)	(1.596)	(2.862)	(0.075)
Industry dummies	Yes	Yes	Yes	Yes
No.of Obs.	7806	148	148	77
R-squared	0.42	0.84	0.58	0.62

*Note: Robust standard errors in parentheses, * denotes significant at 10%; ** denotes significant at 5%, *** denotes significant at 1%. Regressions include controls for cohort group (dummies) suppressed here for brevity.*

Table B4: Trade Protection and Household Income: Dynamic Regressions

	(1) Moffit (1993)	(2) Verbeek & Vella (2005)
Lagged Welfare	0.487*	0.473*
	(0.273)	(0.271)
Agehead	0.002	-0.003
	(0.008)	(0.008)
Agehead^2	-0.001	0.001
	(0.001)	(0.001)
Hsize	-0.052	-0.053*
	(0.032)	(0.031)
Urban	0.020	0.033
	(0.121)	(0.120)
Basic	0.057*	0.059*
	(0.031)	(0.031)
Secondary	0.146	0.147
	(0.105)	(0.104)
Post-Sec	0.158	0.137
	(0.099)	(0.099)
Tertiary	0.300	0.311
	(0.259)	(0.258)
Land	0.003**	0.003**
	(0.001)	(0.001)
Forest	0.047*	0.049*
	(0.026)	(0.026)
Savannah	0.029	0.033
	(0.030)	(0.031)
Tariff	0.009**	0.091***
	(0.005)	(0.012)
GLSS 4	0.120***	0.253***
	(0.021)	(0.028)
Constant	7.389*	7.557*
	(3.887)	(3.871)
Industry dummies	Yes	Yes
No. of Obs.	7806	7806
R-squared	0.45	0.42

*Note: Robust standard errors in parentheses, * denotes significant at 10%; ** denotes significant at 5%, *** denotes significant at 1%. Regressions include controls for cohort group (dummies) suppressed here for brevity.*

Table B5: Trade Protection and Household Income: Dynamic Regressions with Interaction

	(1) Moffit (1993)	(2) Verbeek & Vella (2005)
Lagged Welfare	0.492*	2.317***
	(0.272)	(0.433
Agehead	0.002	0.013
	(0.008)	(0.009)
Agehead^2	-0.001	-0.001
	(0.001)	(0.001)
Hsize	-0.051	0.161***
	(0.032)	(0.050)
Urban	0.018	-0.780***
	(0.121)	(0.192)
Basic	0.087**	0.606***
	(0.035)	(0.152)
Secondary	0.185*	0.443***
	(0.106)	(0.133)
Post-Sec	0.198**	0.507***
	(0.101)	(0.147)
Tertiary	0.352	-
	(0.260)	
Land	0.003**	-0.001
	(0.001)	(0.002)
Forest	0.048*	0.156***
	(0.026)	(0.034)
Savannah	0.028	0.013
	(0.030)	(0.035)
Tariff	0.013**	0.173***
	(0.005)	(0.025)
Tariff x Skill	-0.002*	-0.046***
	(0.001)	(0.012)
GLSS 4	0.120***	0.258***
	(0.021)	(0.031)
Constant	7.300*	-19.236***
	(3.885)	(6.298)
Industry dummies	Yes	Yes
No. of Obs.	7806	7806
R-squared	0.45	0.28

*Note: Robust standard errors in parentheses, * denotes significant at 10%; ** denotes significant at 5%, *** denotes significant at 1%. Skill is a categorical variable representing educational qualification (unskilled, semi-skilled & skilled). Regressions include controls for cohort group (dummies) suppressed here for brevity.*

Following Verbeek and Vella (2005) with Instruments for Tariff and Lagged Welfare

Table B6: Trade Protection and Household Welfare: Evidence from Dynamic Regressions *à la* Verbeek and Vella (2005)

	(1)	(2)
Lagged welfare	0.373**	0.366**
	(0.158)	(0.164)
Agehead	0.026*	0.024
	(0.015)	(0.016)
Agehead^2	-0.001*	-0.001
	(0.001)	(0.001)
Hsize	-0.065***	-0.065***
	(0.018)	(0.019)
Urban	0.079	0.080
	(0.071)	(0.074)
Basic	0.068***	0.454***
	(0.024)	(0.120)
Secondary	0.188***	0.707***
	(0.066)	(0.173)
Post-sec	0.177***	0.713***
	(0.063)	(0.176)
Tertiary	0.401**	1.120***
	(0.158)	(0.274)
Land	0.004***	0.005***
	(0.001)	(0.001)
Forest	0.041*	0.038
	(0.023)	(0.023)
Savannah	0.032	0.019
	(0.031)	(0.033)
Tariff	0.085***	0.129***
	(0.012)	(0.018)
Tariff x skill		-0.025***
		(0.008)
GLSS 4	0.238***	0.236***
	(0.039)	(0.041)
Constant	8.417***	8.244***
	(2.515)	(2.601)
Industry dummies	Yes	Yes
No. of Obs.	7834	7834
R-squared	0.43	0.40

*Note: Robust standard errors in parentheses, * denotes significant at 10%; ** denotes significant at 5%, *** denotes significant at 1%. Skill is a categorical variable representing educational qualification (unskilled, semi-skilled & skilled). Regressions include controls for cohort group (dummies) suppressed here for brevity.*

Appendix C: Details on Pseudo Panel Methods

C.1. Pseudo Panels from Repeated Cross Sections: A Theoretical Consideration

The use of 'pseudo-panel' data was introduced by Deaton (1985) for the analysis of consumer demand systems. In his seminal paper, Deaton (1985) suggests grouping individuals (cases, observational units) into cohorts on the basis of shared characteristics such as sex or age.[29] He then shows that averages within these cohorts could be treated as observations in a pseudo (synthetic) panel. The cohorts are then traced over time as "they" appear in successive surveys, forming a panel, from which standard panel data models can be identified and consistently estimated.

Assuming we have a time series of T independent cross-sections with N observations in each, we can write the linear model with individual effects as following:

$$w_{it} = \mathbf{x}_{it}'\beta + f_i + \varepsilon_{it} \qquad i = 1,...,NT, \quad t = 1,...,T.\cdot \tag{C.1}$$

where w_{it} is equivalent adult consumption in period t of household i, $\mathbf{x_{it}}$ is a set of characteristics (socio-economic or demographic), β is a vector of parameters to be estimated, f_i is the household fixed effect and ε_{it} represents an error term. Since, in general, f_i, will be correlated with the other explanatory variables, such an equation can only be consistently estimated from panel data. However, assume the case where i is a member of well-defined cohort group c, whom we can follow via its (randomly chosen) representatives through repeated cross sections. Deaton's suggestion is to take simple means of equation (C.1) over all households that happen to be observed in period t belonging to cohort c to obtain

$$\overline{w}_{ct} = \overline{\mathbf{x}}_{\mathbf{ct}}'\beta + \overline{f}_{ct} + \overline{\varepsilon}_{ct} \qquad c = 1,...,C. \tag{C.2}$$

The problem with estimating equation (C.2) derives from the fact that the cohort fixed effect $\overline{f}_{ct}$ can be correlated with $\overline{\mathbf{x}}_{\mathbf{ct}}$ (if f_i is correlated with $\mathbf{x_{it}}$), is unobserved and not constant over time due to the changing membership of the cohorts as new surveys are

[29] Note that while the use of "cohorts" has become synonymous with the grouping of individuals by year-of-birth, whenever the term "cohorts" is used in this paper we refer to groups of units (individuals, households, etc.) sharing some common characteristics (not exclusive to year-of-birth). A broader term used, often to mean the same thing, is "cell".

conducted. Likewise, all the other *observed* cohort mean variables ($\overline{w}_{ct}$ and $\overline{\mathbf{x}}_{\mathbf{ct}}$) are merely error ridden estimates acting as proxies for the true cohort means. In this case, the standard within estimator based on the pseudo panel will be inconsistent.

However, if it were at all possible to observe the true cohort *population* means, the true relationship would be as presented in equation (C.3).

$$w_{ct}^{*} = \mathbf{x}_{ct}^{*}\beta + f_{c}^{*} + \varepsilon_{ct}^{*} \tag{C.3}$$

where asterisks denote population (i.e., cohort population) means. In this case, the cohort fixed effects f_c could be directly estimated using cohort dummy variables. Deaton (1985) argues that unless the sample is large the use of cohort-means (equation C.2) as estimates of the unobservable population means (equation C.3) without appropriate correction will potentially lead to inconsistent estimates since the model is in fact one of errors in variables with all variables (except dummies) subject to error. Thus, he proposes an errors-in-variables technique to account for the measurement error.

Imagine the measurement errors are distributed with zero mean, independent of the true values, i.e.:

$$\begin{pmatrix} \overline{w}_{ct} \\ \overline{\mathbf{x}}_{\mathbf{ct}} \end{pmatrix} \approx N\left(\begin{pmatrix} w_{ct}^{*} \\ \mathbf{x}_{\mathbf{ct}}^{*} \end{pmatrix}; \begin{pmatrix} \sigma_{00} & \sigma' \\ \sigma & \Sigma \end{pmatrix}\right). \tag{C.4}$$

Where σ_{00} is the sampling variance of $\overline{w}_{ct}$, σ is the sampling covariance vector between $\overline{w}_{ct}$ and $\overline{\mathbf{x}}_{\mathbf{ct}}$, and Σ is the sampling variance-covariance matrix of $\overline{\mathbf{x}}_{\mathbf{ct}}$[30]. Deaton's estimator for β is given by

$$\hat{\beta}_{Deaton} = \left(M_{xx} - \Sigma\right)^{-1}\left(M_{xy} - \sigma\right), \tag{C.5}$$

where,

$$M_{xx} = \frac{1}{CT}\sum_{c=1}^{C}\sum_{t=1}^{T}\left(\overline{\mathbf{x}}_{\mathbf{ct}} - \overline{\mathbf{x}}_{\mathbf{c}}\right)\left(\overline{\mathbf{x}}_{\mathbf{ct}} - \overline{\mathbf{x}}_{\mathbf{c}}\right)' \tag{C.6}$$

[30] The observed micro-data used to construct the cohort means could be used to derive estimates of the sampling variances and covariances which can be used to obtain consistent estimators of the regression parameters using errors in variables procedures (See Deaton, 1985:117-121).

$$M_{xy} = \frac{1}{CT}\sum_{c=1}^{C}\sum_{t=1}^{T}\left(\overline{\mathbf{x}}_{\mathbf{ct}} - \overline{\mathbf{x}}_{\mathbf{c}}\right)\left(\overline{w}_{\mathbf{ct}} - \overline{w}_{\mathbf{c}}\right)' \tag{C.7}$$

However, Verbeek and Nijman (1993), have shown that consistency of Deaton's errors-in-variables estimator (hereafter, EVE) requires that the number of available cross-sections tends to infinity. The authors also note that Deaton's estimator increases variance at the same time that it reduces bias, giving rise to a mean-squared error trade-off. They have suggested several modifications of EVE which do not suffer from an inconsistency due to a small number of time periods. In principle, starting from EVE and the standard within estimator they derive a consistent estimator (for fixed T, i.e. small number of cross sections) by taking deviations from the pseudo panel cohort means and adjusting the moments matrices of the least squares estimator to remove a fraction $\tau = \frac{T-1}{T}$ of the (estimated) error variance (not all of it). Their proposal results in a slight adjustment to EVE such that we can write the estimator of β as,[31]

$$\hat{\beta}_{corrected} = \left(M_{xx} - \tau\,\Sigma\right)^{-1}\left(M_{xy} - \tau\sigma\right), \tag{C.8}$$

In particular, Verbeek and Nijman have suggested that when the cohort size is fairly large (at least 100 members), and the time variation in the cohort means is sufficiently large, the bias in the standard within estimator will be small enough that the measurement error problem can be safely ignored.[32] Hence, to avoid the measurement error problem, most researchers would usually divide the sample into a smaller number of cohorts, C, (between 10 and 20) to ensure that observations per cell, n_c, is reasonably large (see, for example, Browning *et al.* (1985), Attanasio and Weber (1995), and Blundell *et al.* (1993, 1998)).[33] Unfortunately, there is no general rule as to how large is 'large enough' to

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[31] As shown by Verbeek and Nijman equation (C.8) is the same as EVE (equation C.5) as Deaton assumes $\tau = 1$. Similarly, if we assume $\tau = 0$ equation (C.8) is equivalent to the fixed-effects estimator on the pseudo panel.

[32] Often, the time series dimension of the data set is large so that even with a small number of groups the total number of observations in the panel is fairly large.

[33] When cell sizes are large, most applied researchers tend to treat pseudo-panel data as though they were genuine panels thereby employing standard econometric methods for panel data, such as the fixed-effects estimator. Collado (1998), however, notes that this approach is only valid if one wants to estimate linear models. He argued that in the case of discrete choice models this approach is unnecessary as the estimators do not rely on asymptotics in the cross-sectional-time-series dimension of the data (CxT). For such models, he shows that a reasonably large number of cohorts are needed to guarantee efficiency.

attenuate the bias in the within-estimator. For example, some authors including Devereux (2003 cited in Verbeek and Vella, 2005) have more recently argued that there can still be substantial bias in the standard within estimator even if cohort sizes are ‘reasonably’ large. He recommends that cell sizes should be larger, at least 2000, possibly. In practice, however, it is almost impossible to construct cohorts with cell sizes that large. Note that many observations per cohort imply a small number of cohort observations C, in the pseudo panel, resulting in inefficient estimators (Verbeek and Nijman, 1993:4).

So far, we have we only looked at the case of estimating the linear fixed-effects model on the cohort means and how to correct for the measurement errors arising from using the observed but error-filled cohort means to proxy for the unobserved cohort population means. An important microeconomic study that uses RCS methods is Browning, Deaton and Irish (1985), who use British household survey data to study consumption and labour supply issues. The variables used in their models are constructed by computing means over cohort-year groups (as in equation C.2). The Browning, Deaton and Irish study fostered other work on the econometric properties of RCS estimation, most notably by Moffitt (1993). Moffitt’s study shows that estimation of RCS models can proceed using the individual level data, and he provides insight on the identification issues with RCS methods. Unlike Deaton (1985), Moffit (1993) analyzes pseudo-panel data in which the number of individuals per group is large relative to the number of groups and time periods.[34] Furthermore, he stresses the importance of constructing cohorts by time-invariant characteristics and shows that RCS estimation can be viewed as instrumental variable estimation. Moffitt (1993:105) argues strongly that grouping individuals into cohorts and estimating the model on the cell means is “unnecessary for identification and point estimation”. He suggests rather that the underlying individual data be employed to achieve efficiency.[35]

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[34] Deaton (1985) assumes that the number of cohorts C tends to infinity which is equivalent as saying that the number of individuals N tends to infinity as cohort sizes remain constant. On the other hand, Moffit’s (1993) asymptotic properties relies on the assumption that C is constant while N tends to infinity.

[35] Since the procedure he suggests here is a corollary of his proposal for identification and estimation of dynamic fixed effects linear models, we do not discuss the static case further. See Moffit (1993) and Ridder and Moffit (2006) for exhaustive discussion.

Another strand that can be discerned in the literature, and which we believe to be important in shaping public policy discourse, is whether one can estimate parameters of a dynamic relationship (models with lags) from RCS data. Up to this point we have only considered the case of the static pseudo-panel linear models with individual effect. However, in many applications estimating a dynamic linear model may be of interest, in its own respect, or required by economic theory.[36] In the absence of genuine panel data, the dynamic equation cannot be estimated directly on individual level data. However, some indirect estimation is possible by considering successive observations of individuals in the same cohort, even though those individuals are not the same across surveys.[37]

In an excellent and instructive study, Moffitt (1993) breaks new grounds in this area by providing an interesting discussion of estimating dynamic models from RCS data. He proposes a two-stage least squares estimator to address this issue. Let us consider the simple first-order autoregressive model given by

$$w_{i(t),t} = \alpha w_{i(t),t-1} + \mathbf{X}'_{i(t),t}\beta + \varepsilon_{i(t),t}, \quad i = 1,...,N; \quad t = 2,...,T; \quad i(t) = 1,...,N_t.^{38} \qquad \text{(C.9)}$$

where all variables are as previously defined in equation (C.1) with the vector $\mathbf{X}_{i(t),t}$ defined to include both time-varying and time-invariant covariates. The lagged dependent variable, $w_{i(t),t-1}$ refers to the value of w at time $t-1$ (say GLSS 3) for individual i observed in cross-section t (say GLSS 4). The main problem facing the researcher using RCS data is that the true value of the lagged dependent variable, $w_{i(t),t-1}$, is unobserved because the same individuals are not tracked over time. Following Moffit (1993), however, equation (C.9) can still be estimated if an instrument for $w_{i(t),t-1}$ can be constructed by using information on the w-values of other individuals observed at $t-1$. If

[36] See Collado (1998) for a flavour of the use of pseudo panel techniques in the case of binary choice models.
[37] However, here the units for which the group mean of lagged dependent variable is computed are different from those for which the group mean of the dependent variable is computed.
[38] It is conventional in the literature to index individuals (or variables) by a double subscript to indicate the non-panel nature of the data.

we let $z_{i(t)}$ denote the set of time-invariant variables in $\mathbf{X}_{i(t),t}$, then one could consider a linear orthogonal projection of $w_{i(t)}$ upon $z_{i(t)}$ using the observations at $t-1$:[39]

$$w_{i(t-1),t-1} = m'_{i(t-1),t-1}\delta_2 + z'_{i(t-1)}\delta_3 + u_{i(t-1),t-1}, \tag{C.10}$$

where $m_{i(t-1),t-1}$ is a set of time-varying covariates contained in the vector $\mathbf{X}_{i(t),t}$. $w_{i(t-1),t-1}$ here refers to the value of w at time $t-1$ for individual i observed in cross-section $t-1$. Once the predicted lagged dependent variable, $w_{i(t),t-1}$ has been obtained from OLS estimation of (C.10) it is now possible to obtain consistent estimates of the parameters from the original model (C.9), substituting $\hat{w}_{i(t),t-1}$ in place $w_{i(t),t-1}$ such that,

$$w_{i(t),t} = \alpha\hat{w}_{i(t),t-1} + \mathbf{X}'_{i(t),t}\beta + \varepsilon_{i(t),t}, \tag{C.11}$$

Moffit recognizes, however, that consistency hinges upon the assumption that $\hat{w}_{i(t),t-1}$ is asymptotically uncorrelated with $\varepsilon_{i(t),t}$.

Recently, Verbeek and Vella, (2005) have taken an issue with Moffitt's (1993) estimator arguing that some of the underlying assumptions may be indefensible and too restrictive for empirical analyses. Their argument is that regardless of how $\hat{w}_{i(t),t-1}$ is estimated, its inclusion in the original model (C.9) implies that at least one of the regressors is error-ridden. The authors show that once the predicted lagged dependent variable, $\hat{w}_{i(t),t-1}$, is inserted into the original model, equation (C.9) is no longer valid. Rather, one would expect

$$w_{i(t),t} = \alpha\hat{w}_{i(t),t-1} + \mathbf{X}'_{i(t),t}\beta + \varepsilon^{*}{}_{i(t),t}, \tag{C.12}$$

where,

$$\varepsilon^{*}{}_{i(t),t} = \varepsilon_{i(t),t} + \alpha\left(w_{i(t),t-1} - \hat{w}_{i(t),t-1}\right). \tag{C.13}$$

Their main disagreement has to do with the "inappropriateness" (in their view) of the key assumption that $\mathbf{X}_{i(t),t}$ is uncorrelated with the prediction error. This assumption is

[39] In most applications $z_{i(t)}$ represents a set of cohort dummies (Collado, 1998, Girma, 2000). In this case it becomes apparent that Moffit's estimator is a special kind of grouping consistent with taking cohort means of the samples (Verbeek and Vella, 2005).

implausible and will result in inconsistency when time-varying exogenous regressors are used (Verbeek and Vella, 2005).

As a solution, Verbeek and Vella (2005), propose an *augmented instrumental variables estimator* using time-invariant instruments. Essentially, one needs to instrument $\mathbf{X}_{i(t),t}$ even though its members are assumed exogenous in the original model (C.9). If, for simplicity, we assume a set of potential instruments, $I_{i(t),t} = z_{i(t)}$, and $z_{i(t)}$ are assumed (not necessarily) to be cohort dummies, we can allow for "cohort effects" by including $z_{i(t)}$ explicitly as regressors in (C.11) as,

$$w_{i(t),t} = \alpha \hat{w}_{i(t),t-1} + \mathbf{X}'_{i(t),t}\beta + z'_{i(t)}\lambda + \eta_{i(t),t}, \tag{C.14}$$

where

$$E\left(\eta_{i(t),t} z_{i(t)}\right) = 0. \tag{C.15}$$

In sum, (C.14) would be the estimating equation using standard IV methods with $z_{i(t)}$ interacted with time dummies, serving as instruments.

Table C1: Cohort Definition and Cell Sizes (5-year Age Bands)

Cohort ID	Region of domicile	Age in 1991/92	Age in 1998/99	Sex of head	Mean cell size
1	Western	18-22	25-29	male	65
2	Western	23-27	30-34	male	98
3	Western	28-32	35-39	male	127
4	Western	33-37	40-44	male	105
5	Western	38-42	45-49	male	96
6	Western	43-47	50-54	male	78
7	Western	48-52	55-59	male	72
8	Western	53-57	60-64	male	41
9	Western	18-22	25-29	female	15
10	Western	23-27	30-34	female	33
11	Western	28-32	35-39	female	42
12	Western	33-37	40-44	female	37
13	Western	38-42	45-49	female	23
14	Western	43-47	50-54	female	27
15	Western	48-52	55-59	female	24
16	Western	53-57	60-64	female	29
17	Central	18-22	25-29	male	30
18	Central	23-27	30-34	male	78
19	Central	28-32	35-39	male	105
20	Central	33-37	40-44	male	80
21	Central	38-42	45-49	male	88
22	Central	43-47	50-54	male	56
23	Central	48-52	55-59	male	46
24	Central	53-57	60-64	male	52
25	Central	18-22	25-29	female	32
26	Central	23-27	30-34	female	33
27	Central	28-32	35-39	female	51
28	Central	33-37	40-44	female	50
29	Central	38-42	45-49	female	41
30	Central	43-47	50-54	female	55
31	Central	48-52	55-59	female	44
32	Central	53-57	60-64	female	49
33	Greater Accra	18-22	25-29	male	77
34	Greater Accra	23-27	30-34	male	104
35	Greater Accra	28-32	35-39	male	136
36	Greater Accra	33-37	40-44	male	117
37	Greater Accra	38-42	45-49	male	121
38	Greater Accra	43-47	50-54	male	97
39	Greater Accra	48-52	55-59	male	69
40	Greater Accra	53-57	60-64	male	38
41	Greater Accra	18-22	25-29	female	47
42	Greater Accra	23-27	30-34	female	59
43	Greater Accra	28-32	35-39	female	62
44	Greater Accra	33-37	40-44	female	75
45	Greater Accra	38-42	45-49	female	40
46	Greater Accra	43-47	50-54	female	44
47	Greater Accra	48-52	55-59	female	44
48	Greater Accra	53-57	60-64	female	35
49	Eastern	18-22	25-29	male	43
50	Eastern	23-27	30-34	male	81
51	Eastern	28-32	35-39	male	109
52	Eastern	33-37	40-44	male	96
53	Eastern	38-42	45-49	male	107
54	Eastern	43-47	50-54	male	86

55	Eastern	48-52	55-59	male	66
56	Eastern	53-57	60-64	male	59
57	Eastern	18-22	25-29	female	16
58	Eastern	23-27	30-34	female	30
59	Eastern	28-32	35-39	female	41
60	Eastern	33-37	40-44	female	47
61	Eastern	38-42	45-49	female	43
62	Eastern	43-47	50-54	female	33
63	Eastern	48-52	55-59	female	49
64	Eastern	53-57	60-64	female	41
65	Volta	18-22	25-29	male	43
66	Volta	23-27	30-34	male	87
67	Volta	28-32	35-39	male	117
68	Volta	33-37	40-44	male	89
69	Volta	38-42	45-49	male	87
70	Volta	43-47	50-54	male	77
71	Volta	48-52	55-59	male	61
72	Volta	53-57	60-64	male	58
73	Volta	18-22	25-29	female	23
74	Volta	23-27	30-34	female	26
75	Volta	28-32	35-39	female	34
76	Volta	33-37	40-44	female	38
77	Volta	38-42	45-49	female	29
78	Volta	43-47	50-54	female	43
79	Volta	48-52	55-59	female	38
80	Volta	53-57	60-64	female	34
81	Ashanti	18-22	25-29	male	91
82	Ashanti	23-27	30-34	male	137
83	Ashanti	28-32	35-39	male	140
84	Ashanti	33-37	40-44	male	122
85	Ashanti	38-42	45-49	male	99
86	Ashanti	43-47	50-54	male	99
87	Ashanti	48-52	55-59	male	57
88	Ashanti	53-57	60-64	male	47
89	Ashanti	18-22	25-29	female	52
90	Ashanti	23-27	30-34	female	83
91	Ashanti	28-32	35-39	female	72
92	Ashanti	33-37	40-44	female	75
93	Ashanti	38-42	45-49	female	60
94	Ashanti	43-47	50-54	female	77
95	Ashanti	48-52	55-59	female	46
96	Ashanti	53-57	60-64	female	45
97	Brong Ahafo	18-22	25-29	male	48
98	Brong Ahafo	23-27	30-34	male	71
99	Brong Ahafo	28-32	35-39	male	91
100	Brong Ahafo	33-37	40-44	male	75
101	Brong Ahafo	38-42	45-49	male	77
102	Brong Ahafo	43-47	50-54	male	59
103	Brong Ahafo	48-52	55-59	male	40
104	Brong Ahafo	53-57	60-64	male	31
105	Brong Ahafo	18-22	25-29	female	28
106	Brong Ahafo	23-27	30-34	female	39
107	Brong Ahafo	28-32	35-39	female	38
108	Brong Ahafo	33-37	40-44	female	34
109	Brong Ahafo	38-42	45-49	female	26
110	Brong Ahafo	43-47	50-54	female	27
111	Brong Ahafo	48-52	55-59	female	22

112	Brong Ahafo	53-57	60-64	female	22
113	Northern	18-22	25-29	male	36
114	Northern	23-27	30-34	male	64
115	Northern	28-32	35-39	male	91
116	Northern	33-37	40-44	male	77
117	Northern	38-42	45-49	male	82
118	Northern	43-47	50-54	male	58
119	Northern	48-52	55-59	male	54
120	Northern	53-57	60-64	male	28
122	Northern	23-27	30-34	female	4
123	Northern	28-32	35-39	female	6
124	Northern	33-37	40-44	female	6
125	Northern	38-42	45-49	female	7
126	Northern	43-47	50-54	female	5
127	Northern	48-52	55-59	female	9
128	Northern	53-57	60-64	female	6
129	Upper West	18-22	25-29	male	4
130	Upper West	23-27	30-34	male	18
131	Upper West	28-32	35-39	male	21
132	Upper West	33-37	40-44	male	21
133	Upper West	38-42	45-49	male	18
134	Upper West	43-47	50-54	male	13
135	Upper West	48-52	55-59	male	16
136	Upper West	53-57	60-64	male	14
140	Upper West	33-37	40-44	female	3
141	Upper West	38-42	45-49	female	5
142	Upper West	43-47	50-54	female	5
145	Upper East	18-22	25-29	male	19
146	Upper East	23-27	30-34	male	35
147	Upper East	28-32	35-39	male	34
148	Upper East	33-37	40-44	male	53
149	Upper East	38-42	45-49	male	49
150	Upper East	43-47	50-54	male	52
151	Upper East	48-52	55-59	male	40
152	Upper East	53-57	60-64	male	27
155	Upper East	28-32	35-39	female	5
156	Upper East	33-37	40-44	female	5
157	Upper East	38-42	45-49	female	10
158	Upper East	43-47	50-54	female	5
160	Upper East	53-57	60-64	female	7
	Average cell size				***52***

Note: Cohorts are defined by interacting 5-year generation bands with regional (10) and gender (2) categories.

Table C2: Cohort Definition and Cell Sizes (10-year Age Bands)

Cohort ID	Region of domicile	Age in 1991/92	Age in 1998/99	Sex of head	Mean cell size
1	Western	18-27	25-34	male	163
2	Western	28-37	35-44	male	232
3	Western	38-47	45-54	male	174
4	Western	48-57	55-64	male	113
5	Western	18-27	25-34	female	48
6	Western	28-37	35-44	female	79
7	Western	38-47	45-54	female	50
8	Western	48-57	55-64	female	53
9	Central	18-27	25-34	male	108
10	Central	28-37	35-44	male	185

11	Central	38-47	45-54	male	144
12	Central	48-57	55-64	male	98
13	Central	18-27	25-34	female	65
14	Central	28-37	35-44	female	101
15	Central	38-47	45-54	female	96
16	Central	48-57	55-64	female	93
17	Greater Accra	18-27	25-34	male	181
18	Greater Accra	28-37	35-44	male	253
19	Greater Accra	38-47	45-54	male	218
20	Greater Accra	48-57	55-64	male	107
21	Greater Accra	18-27	25-34	female	106
22	Greater Accra	28-37	35-44	female	137
23	Greater Accra	38-47	45-54	female	84
24	Greater Accra	48-57	55-64	female	79
25	Eastern	18-27	25-34	male	124
26	Eastern	28-37	35-44	male	205
27	Eastern	38-47	45-54	male	193
28	Eastern	48-57	55-64	male	125
29	Eastern	18-27	25-34	female	46
30	Eastern	28-37	35-44	female	88
31	Eastern	38-47	45-54	female	76
32	Eastern	48-57	55-64	female	90
33	Volta	18-27	25-34	male	130
34	Volta	28-37	35-44	male	206
35	Volta	38-47	45-54	male	164
36	Volta	48-57	55-64	male	119
37	Volta	18-27	25-34	female	49
38	Volta	28-37	35-44	female	72
39	Volta	38-47	45-54	female	72
40	Volta	48-57	55-64	female	72
41	Ashanti	18-27	25-34	male	228
42	Ashanti	28-37	35-44	male	262
43	Ashanti	38-47	45-54	male	198
44	Ashanti	48-57	55-64	male	104
45	Ashanti	18-27	25-34	female	135
46	Ashanti	28-37	35-44	female	147
47	Ashanti	38-47	45-54	female	137
48	Ashanti	48-57	55-64	female	91
49	Brong Ahafo	18-27	25-34	male	119
50	Brong Ahafo	28-37	35-44	male	166
51	Brong Ahafo	38-47	45-54	male	136
52	Brong Ahafo	48-57	55-64	male	71
53	Brong Ahafo	18-27	25-34	female	67
54	Brong Ahafo	28-37	35-44	female	72
55	Brong Ahafo	38-47	45-54	female	53
56	Brong Ahafo	48-57	55-64	female	44
57	Northern	18-27	25-34	male	100
58	Northern	28-37	35-44	male	168
59	Northern	38-47	45-54	male	140
60	Northern	48-57	55-64	male	82

62	Northern	28-37	35-44	female	12
63	Northern	38-47	45-54	female	12
64	Northern	48-57	55-64	female	15
65	Upper West	18-27	25-34	male	22
66	Upper West	28-37	35-44	male	42
67	Upper West	38-47	45-54	male	31
68	Upper West	48-57	55-64	male	30
71	Upper West	38-47	45-54	female	10
73	Upper East	18-27	25-34	male	54
74	Upper East	28-37	35-44	male	87
75	Upper East	38-47	45-54	male	101
76	Upper East	48-57	55-64	male	67
78	Upper East	28-37	35-44	female	10
79	Upper East	38-47	45-54	female	15
80	Upper East	48-57	55-64	female	9
	Average cell size				***104***

Note: Cohorts are defined by interacting 10-year generation bands with regional (10) and gender (2) categories.