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Massimiliano Tani
University of New South Wales and IZA

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IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Skilled Migration Policy and the Labour Market Performance of Immigrants*

This paper studies whether migration policy, besides managing a country's population size, is a suitable tool to influence immigrants' labour market outcomes. To do so, it uses a migration policy change that occurred in Australia in the late 1990s and data collected by the Longitudinal Survey of Migrants to Australia. The statistical techniques employed in the empirical analysis consistently reveal that the policy change has no detectable impact on the employment rate, wages, over-education, occupational downgrading, and (self-reported) use of skills for male immigrants, who account for about 75% of the sample, while they have a modest short-term positive impact on female immigrants. These results support the view that migration policy is an ineffective policy tool to influence migrants' labour market outcomes. However, the economic relevance of making an effective use of migrants' skills provides scope for close coordination between immigration and employment policy to ensure that efforts in attracting foreign talent are not dissipated by labour market frictions and other inefficiencies.

JEL Classification: J15, J24

Keywords: skilled immigration, labour market, over-education, immigration policy

Corresponding author:
Massimiliano Tani
School of Business
University of New South Wales
Northcott Drive
Campbell, ACT 2612
Australia

E-mail: m.tani@adfa.edu.au

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1. Introduction

Australia carries out the world's largest skilled migration program. The annual inflow of skilled foreign workers is about 1% of Australia's population, and a higher proportion of its skilled labour force. Despite the potential competition with skilled natives, over the past three decades Australia has experienced wage growth and returns to higher education unparalleled in any other advanced economy.

Yet, the labour market outcomes for many foreign-educated migrants are substantially and stubbornly below those of comparably-educated natives. The incidence of skill mismatch (over-education) among university-educated foreign workers in Australia is as high as 40–50% versus 10–20% among comparable domestic workers (Green et al, 2007), placing Australia on equal footing to countries that do not implement selective immigration policies (Schuster, Desiderio, and Urso, 2013). This evidence is counterintuitive, as selective policies are designed to admit only the most productive migrants. It is also inefficient: migrants' economic under-achievement costs Australia potential income, spending for consumption and investment, and taxation revenue, and can potentially compromise its image of attracting and effectively using the best foreign talent available vis-à-vis other countries competing for the same individuals.

Is immigrants' education-occupation mismatch an issue that can be addressed by migration policy, if at all? This question is addressed by exploiting a migration policy change that occurred in Australia in the late 1990s and a detailed migrant survey that took place at the time, which is used to analyse its effect on immigrants' labour market outcomes using various quantitative techniques.

Due to a fortuitous coincidence, the policy change occurred between two cohorts of immigrants surveyed in detail by the Longitudinal Survey of Immigrants to Australia (LSIA). This circumstance makes this particular policy change quite unique as the

available data enable one to identify the change from time effects and other determinants of immigrants' labour market outcomes.

The paper focuses on five indicators of labour market performance: namely, the probability of finding employment, wages (derived from the ANU-4 scale of occupational prestige[1]), the probability of being over-educated for the job carried out according to the job analysis method[2], the subjective perception of skill usage, and occupational downgrading defined as a reduction in the occupational prestige (as measured by occupational codes at 4-digits) between the last job performed before migrating and the job carried out after settlement at the time of survey.

The rest of the paper is structured as follows: Section 2 present a short review of the economics literature focusing on migration policy and the use of selective admission criteria. Section 3 describes the policy change. Section 4 presents the data while Section 5 presents the results, and Section 6 concludes.

2. Migration as self-selection: income, skills and education

Economics-based studies tend to view migration as an individual decision resulting from a rational cost-benefit analysis, which arises from comparing the net expected benefits that can be gained by staying in the home country versus those obtained by moving abroad. Migration occurs if the latter are greater than the former (Sjaastad, 1962). As individuals differ in innate and demographic characteristics and circumstances, migrating does not occur at random in the population, but characterises a subgroup of self-selected individuals.

The literature has exploited the notion of self-selection into migration to identify the

[1] http://ipumsi.anu.edu.au/SiteTools/Status_Scales/scale4.php. Accessed 4th May 2017.

[2] The job analysis method measures over-education on the basis of occupational definitions developed by professional job analysts (in this case the Australian Bureau of Statistics – see ABS, 2006. A worker is considered to be over-educated if his or her actual education level is higher than the required education level specified in the occupational classification.

type of migrants attracted to different countries (Roy, 1951; Borjas 1987 and 1991). Of course, average differences in incomes between home and host countries play a critical role in determining who migrates where, as does the quality of the information set facing migrants. If information is complete and average incomes at home are below those of the host for each level of skill, then every home citizen will have an incentive to emigrate. However, if home and host countries place a similar value on skills, average incomes per capita will be similar, and the most skilled individuals will migrate to the country with the higher income inequality to increase the economic benefit they receive for their skills. Conversely, the least skilled will migrate to the country with a compressed income distribution to maximize the economic benefits for their skills. If the information is incomplete or imperfect then 'irrational' migration behaviours may be observed (e.g. Mbaye, 2014).

As discussed by Tani (2014) a selective immigration policy becomes relevant if the host country has a relatively high average income compared with the home country (most home citizens would want to emigrate), a compressed income distribution (low-skill home citizens want to emigrate), and possibly a comprehensive welfare system for its low-income earners. Keeping out low-skill immigrants in favor of skilled immigrants may not only "protect" the host country's welfare system and address its domestic employers' needs, but also offer an automatic mechanism to stabilize income inequality trends between skilled and unskilled native workers. This is because the earnings growth of skilled immigrants will be constrained (as there will be plenty of them), whereas unskilled (native) workers will be in shorter supply and therefore will command higher wages.

Clemens and Pritchett (2016) test the idea that restricting migration could be efficient because it prevents migrants from low-income countries from 'transmitting' low

productivity to high-income countries, but their result suggest that current restrictions to migration are still excessive for the 'low productivity contagion hypothesis' to be empirically supported based on current migrant flows.

In practice several countries limit the inflow of migrants using a point system[3]. This is typically based on educational achievement, language proficiency and age. However, the effects of such restrictions are debated[4], especially with reference to the determinants of selection on education (McKenzie and Rapoport, 2010; Beine et al., 2011) and the influence of immigration policies on the selection process from both a theoretical (Docquier et al., 2008; Bertoli and Brucker, 2011; Bertoli and Rapoport, 2015) and an empirical perspective (Antecol et al., 2003; Jasso and Rosenzweig, 2009; Aydemir, 2011; Belot and Hatton, 2012). In general, this literature suggests that imposing minimum educational requirements raises the educational profile of immigrants, but this does not guarantee better labour market outcomes. For example, Bertoli, Dequiedt and Zenou (2015) find that screening potential migrants on the basis of observable characteristics, especially education, may reduce admitted migrants' quality because education also influences migrants' self-selection on variables that are

[3] The selected criteria of a point system typically arise from the findings of applied research on migration and surveys of immigrants to determine the ingredients for successful economic assimilation. Thus, points are assessed based on short-term labor market criteria, such as having skills in high demand domestically, and desirable individual characteristics, such as youth, education, and language proficiency. Once applicants pass the point test, they must still meet additional minimum standards in such areas as health and good character. The economic principle underpinning the point system is to identify prospective immigrants' net benefit to the host country (their effect on gross domestic product or public finances), which has to be positive. As a result, points are awarded to younger immigrants, who can potentially contribute for longer to the public finances through income taxes and are less likely to need welfare assistance in the short term. Points are also given to applicants with high levels of formal education or vocational training, as their human capital can be employed without further training costs for the host country. These characteristics are also associated with high levels of adaptability and mobility, which help to minimize time out of the labor force. Furthermore, points are awarded for proficiency in the host country's language, as this reduces retraining costs and facilitates rapid economic and social integration. Canada, Australia, and New Zealand each award points to prospective immigrants in different ways, assigning different weights to desired characteristics that reflect the evolution of migration policy objectives.

[4] Examples are Borjas (1987), Antecol et al. (2003), Chiquiar and Hanson (2005), Jasso and Rosenzweig (2009), Moraga (2011), Ambrosini and Peri (2012), Dequiedt and Zenou (2013), and Kaestner and Malamud (2014).

not measured, like ability and motivation. An increase in selectivity based on education may lead to admitting less able and motivated migrants.

This paper adds to existing work that aims to draw general conclusions on the labour market effects of migration policy by studying the effect of a specific policy change in Australia. The change, which is well documented in the literature (e.g. Cobb-Clark, 2003; Richardson et at, 2004; Thapa et al, 2006; Mahuteau and Junankar, 2008) tightened entry conditions for applicants in the skilled independent and concessional family visa categories, but not in the preferential family, employer nomination, and humanitarian streams. This differential treatment across visa categories enables one to identify the effect of the policy change on those affected and not affected by it, and address the key question of the paper: whether or not migration policy is a suitable tool to influence not only a country's population size (its main objective) but also the labour market performance of its immigrants.

3. Australia's immigration policy change in the late 1990s

In 1996 a newly elected government introduced a number of significant changes to Australia's migration policy, affecting some visa categories in the skill and family reunification streams but not the remaining migration channels (Hawthorne, 2005). This new policy:

(1) abolished the social security benefit to new immigrants in the first two years after their arrival, as well as access to the Adult Migrant English Program, whose costs were to be met by the immigrant, and labour market programs, whose costs were to be repaid after securing work;

(2) allocated the highest points weighting to employability factors, namely skills, age, and English language ability. Age-related points for applicants over the age of 45 were abolished while bonus points were awarded to those with

relevant Australian or international professional work experience, a job offer, a spouse meeting the skill application criteria, an Australian sponsor who had to provide a guarantee, and carrying A$100,000 or more in capital;

(3) introduced additional points for occupations in demand in addition to degree-level specific as opposed to generic qualifications, and bonus points for qualifications obtained recently in Australia;

(4) pre-migration qualification screening was effectively outsourced to professional bodies, who could now disqualify Non-English Speaking Background (NESB) applicants from eligibility for skill migration.

The effect of this policy change has been examined in detail by previous studies, which compare the outcomes of the first cohort of the LSIA, who arrived in Australia prior to the policy change between 1993 and 1995 (LSIA1), with those of the second cohort, who arrived after the change between 1999 and 2000 (LSIA). These studies offer a mixed picture about the policy's effects on migrants' labour market outcomes: they concur that the second cohort had better education levels compared to the first cohort, especially with higher university degrees at the time of arrival, higher participation rates (Cobb-Clark, 2003; Chiswick and Miller, 2006), and lower durations to access their first job (Thapa and Goergens, 2006), as aimed by the policy change. However, the job quality of the second cohort is markedly inferior to that obtained by the first, with widespread occupational downgrade (Mahuteau and Junankar, 2008). This is partly attributed to the worse macro-economic conditions faced by migrants surveyed in the second cohort.

4. Data: Longitudinal Survey of Immigrants to Australia (LSIA)

The LSIA is an extensive longitudinal survey of migrants to Australia commissioned in the 1990s to collect better information on the settlement of new immigrants relative

to what was available through the national census, and is based on a representative sample of 5% of permanent migrants from two successive cohorts[5]. Despite being a very short panel, the LSIA captures valuable information about migrants' conditions prior to moving and during the initial stages of settlement[6]. An informative description of the LSIA is in Cobb-Clark (2001).

To improve the comparability between first and second cohort and focus on labour market outcomes the sample is restricted to primary applicants in working age (20-65). Additional restrictions include the removal of observations with missing occupational data and information on the education-occupation mismatch in the year before migration, which is used as a proxy for ability.

Figures 1-5 in the Appendix present some salient features of the five labour market outcomes used in the empirical analysis measured as of the first LSIA wave (about six months after settlement in Australia): employment rate, wage, incidence of over-education, self-assessed use of skills, and occupational downgrading. As found by previous research, the employment rate in the second cohort is higher, but the other labour market indicators do not show substantial improvements in the visa classes affected by the change, despite the marked shift towards better qualified and English proficient immigrants in these streams.

[5] LSIA1 surveys migrants who arrived between September 1993 and August 1995 and is composed of three waves collected between 4-6 months after settlement and up to 41 months afterwards. LSIA2 surveys immigrants arrived between September 1999 and August 2000 and contains two waves collected between 4-6 months after settlement and about a 15 months later. A third cohort, LSIA3, was collected using a substantially reduced version of the questionnaire. These data are not suitable for the analysis carried out in this paper and are hence not used. The LSIA oversamples some groups of individuals notably on visa categories. The humanitarian (refugee) category is over-represented but the weights to recover population statistics are available in the database.

[6] One of its strengths is information prior to migration, including the occupation at a 4-digit code in the last job prior to migrating and the job subsequently carried out in Australia. The LSIA has a number of limitations. It surveys a relatively small sample, so that categories within relevant explanatory variables often need to be aggregated. It covers neither native Australians nor New Zealanders, who face no work restrictions if settling in Australia, forcing comparisons only between different immigrant groups. It does not covers onshore applicants, like international students already in Australia, whose contribution to the skilled independent immigrant flow has been substantial since the early 2000s.

Table 1 reports the mean and standard deviation of key variables measured as of the first wave of the sample by cohort (before/after the policy change) and visa class (affected/not affected). Migrants in affected and not-affected visa categories are different in many respects, including labour market outcomes, demographic and educational characteristics, places of origin, and residential choices post-migration. These differences identify suitable control variables for the regression analysis.

Since the effect of time is measured by membership to the cohort (first/second) and that of immigration policy by the visa class, the effect of the change in immigration policy arises from their interaction: namely, the change in the difference between second and first cohort of affected and not affected groups ('difference in differences' or DiD).[7] This indicator is reported in the last column of Table 1, and its values across migrant characteristics may be interpreted as (unconditional) effects of the policy change.

[7] It can be defined as is defined as: $(C2_a - C1_a) – (C2_{na} – C1_{na})$ where *C1* and *C2* indicate the first and second cohort of the LSIA, respectively, and the subscripts *a* and *na* refer to whether or not the migrant was affected by the policy change.

Table 1 – Unconditional means in the working sample

	Cohort 1 (pre-change)		Cohort 2 (post-change)		
	Not-affected	Affected	Not-affected	Affected	DiD*
Employment rate	.605	.667	.799	.871	+0.010
	(.489)	(.471)	(.401)	(.336)	
Occupational downgrading (ASCO 2)	8.35	6.64	8.23	7.21	+0.113
	(25.7)	(24.1)	(23.9)	(22.0)	
Wage	3.696	3.780	3.683	3.880	-0.025
	(.788)	(.699)	(.635)	(.543)	
Over-education	.169	.268	.213	.287	+0.104
	(.375)	(.443)	(.410)	(.453)	
Use of skills	.587	.562	.603	.682	+0.690
	(.355)	(.337)	(.372)	(.349)	
Age	34.3	33.6	36.0	33.8	-1.530
	(10.67)	(6.63)	(10.64)	(6.50)	
Female	.488	.265	.491	.343	+0.075
	(.500)	(.441)	(.500)	(.475)	
Secondary schooling or less	.447	.052	.398	.068	+0.065
	(.497)	(.222)	(.490)	(.252)	
Vocational diploma	.258	.369	.285	.293	-0.103
	(.437)	(.483)	(.451)	(.456)	
Tertiary education or above	.296	.578	.317	.638	+0.039
	(.456)	(.494)	(.465)	(.481)	
Interview in English	.492	.804	.545	.950	+0.093
	(.500)	(.397)	(.498)	(.219)	
Born in Europe or N America	.146	.230	.184	.268	+0.000
	(.353)	(.421)	(.388)	(.443)	
Born in SE Europe or MENA	.348	.208	.338	.108	-0.090
	(.476)	(.406)	(.473)	(.311)	
Born in Asia	.347	.424	.345	.423	+0.001
	(.476)	(.494)	(.476)	(.494)	
Born elsewhere	.158	.138	.132	.201	+0.089
	(.365)	(.345)	(.339)	(.401)	
Living in NSW/VIC	.706	.700	.669	.703	+0.040
	(.455)	(.458)	(.471)	(.457)	
Living elsewhere	.294	.300	.331	.297	-0.034
	(.455)	(.458)	(.471)	(.457)	
Nr observations	3,229	1,492	2,096	556	7,373

Notes: Source 1st wave of LSIA1 and LSIA2.

* The difference in differences is obtained as $(C2_a - C1_a) – (C2_{na} – C1_{na})$

Some general features of affected and not affected groups are highlighted before discussing differences across cohorts and the unconditional effects of the policy change.

The affected visa categories cover immigrants selected via Australia's point system and include mostly highly educated migrants under the age of 45. It is hence not surprising that these affected in both cohorts are predominantly males, younger, have a higher incidence of tertiary education, and better English language skills than the not affected, who are predominantly migrants reunifying with family already in Australia. The affected groups also include a larger proportion of migrants originating from Asia and settling in the more urbanised states in Australia, where the largest cities and the national capital are located.

With references to changes over time between the first and second cohort, the affected groups are characterised by an increased proportion of female primary applicants (perhaps because married to a similarly educated but older male partner[8]), have higher rates of tertiary educated, and a better command of English. They also experience better labour market outcomes, especially in terms of employment rate, wages, and use of skills. However, they are also characterised by higher incidence of over-education and occupational downgrading.

The groups not affected of the second cohort tend to be older, better educated, and have better English language skills than those of the first cohort. They also experience better employment rates and skill usage though their probability of over-education is also higher.

With reference to the key indicator of the policy change, the last column of Table 1 shows that affected migrants in the second cohort are younger, have better knowledge of English, and a higher probability of holding a university degree (though only

[8] Under Australia's point system, the primary applicant is the household member with the highest score. As most points are given for young age using a scale reducing to zero for age of 45, it is likely that the primary applicant of a tertiary-educated and working couple where the male partner is beyond some intermediate age cut-offs is actually female.

secondary schooling is also more likely). Labour market outcomes are mixed: the employment rate is higher and so is the self-assessed use of skills but the other indicators point to worsened conditions as over-education and occupational downgrading are more prevalent, while wages are lower. Of course these results are obtained from unconditional means, and therefore cannot be relied upon as the potential influence of the other observed (but omitted) variables summarised in Table 1 is masking the underlying relationship of interest.

5. Methodology

Measuring the effect of the policy change faces the fundamental evaluation problem that only one of the outcomes of a treatment for an individual can be observed (Caliendo and Kopeinig, 2008; Roy, 1951; Rubin, 1974). This occurs as an individual can only be observed if either treated or not but not in both states at once[9]. As summarised by Athey and Imbens (2017), "estimates of causal effects are ultimately based on comparisons of different units with different levels of the treatment" (p.4).

To mitigate this issue, the literature has developed alternative strategies to generate suitable counterfactual groups by focusing on average treatment effects at the level of population, and especially on the 'average treatment effect on the treated' (ATT). This is defined as:

$$T_{ATT} = E(T \mid D = 1) = E[Y(1) \mid D = 1] - E[Y(0) \mid D = 1]$$

The ATT can be estimated directly if assignment to treatment is randomised so that the average effect on the untreated when treated $E[Y(0) \mid D = 1]$, which not observed, can be replaced by the average effect on the untreated $E[Y(0) \mid D = 0]$. However, as the decision to migrate is highly unlikely to be random in the population, the estimate

[9] Indicating with D a binary indicator for the treatment for individual *i* (1 if treated and 0 otherwise) and with Y_i (D_i) the outcome of interest, the individual treatment effect is $T_i = Y_i(1) - Y_i(0)$ but only $Y_i(1)$ if treated or $Y_i(0)$ if not treated can be measured.

of the ATT is possible only upon assuming some additional identifying assumptions. I follow four well-established alternative approaches (Lee and Kang, 2006; Imbens and Wooldridge, 2007).

The first is to focus on the pooled cross sections collected in the first wave of LSIA1 and LSIA2 using a 'before-after estimator'[10], whereby the before and after migration difference between affected and not affected is identified with the policy change in the statistical model:

$$Y_i = a + X_i b + cC_2 + dV_i + fC_2V_i + \varepsilon_i \qquad (1)$$

where X_i is a set of individual characteristics of migrant i that includes age, gender, education level, country of origin, and state of residence after settlement; C (= 0,1) indicates whether the migrant belongs to the second cohort, which migrates after the policy change; V_i (= *a, na*) indicates whether the visa use to migrate is affected by the policy change occurring between the two cohorts; the interaction term C_2V_i is equal to one for migrants affected by the policy change surveyed in the second cohort and zero otherwise; and ε_i is an idiosyncratic error term. The parameter of interest is f as it measures the effect of the policy change on the second cohort. This before/after estimator is obtained by applying Ordinary Least Squares (OLS) to equation (1), and is calculated as:

$$\hat{f} = \left(\bar{Y}_{Va,C1} - \bar{Y}_{Va,C0}\right) - \left(\bar{Y}_{Vna,C1} - \bar{Y}_{Vna,C0}\right)$$

where the subscripts refer to the affected ($_{Va}$) and not affected ($_{Vna}$) groups in the second ($_{C1}$) and first ($_{C0}$) LSIA cohort, respectively. This is exactly the equivalent of the difference-in-differences indicator reported in Table 1, but this time conditioned on other observed determinants of labour market outcome.

[10] In Imbens and Wooldridge (2007) this is referred to as a 'difference-in-differences estimator' (p.1 equation (1.2)).

One drawback of this approach is the assumption that the sampling error arising from measuring the means of each subgroup is the only type of uncertainty of the inference carried out. In reality it is possible that other sources of error (e.g. sampling selection, clustering) interfere with the quality of the sample used, and potentially be a source of bias for the estimates obtained.

An alternative approach is to make recourse to the conditional independence assumption (CIA), which states that for a given set of observed covariates that do not depend on the policy change, the outcome of interest is independent of whether or not one belongs to the group affected by the change. When this occurs it is possible to obtain a counterfactual group using a propensity score matching (PSM). This statistical matching technique attempts to reduce the potential bias originating from simply comparing outcomes among individuals that migrate under the visa classes affected by the policy change versus those who did not. In addition to the CIA, PSM requires an overlap of the characteristics observed among migrants affected and not affected by the policy change ('common support' - Caliendo and Kopeinig, 2008) to ensure that individuals with similar characteristics have a positive probability to migrate in either affected or not affected visa categories. The PSM estimator is the mean difference in outcomes over the common support weighted by the propensity score distribution of the individuals included. The PSM applied in the empirical analysis uses the nearest neighbor matching optimization algorithm and is obtained by Stata's user-written package *pscore* (Becker and Ichino, 2002).

A third approach involves exploiting the panel nature of the LSIA for both cohorts, and including the second wave of observations using the model:

$$Y_{it} = \alpha + \sigma t + X_{it}\beta + \gamma C_2 + \delta V_i + \mu C_2 V_i + u_i + \eta_{it} \qquad (2)$$

where *Y*, *X*, *i*, C_2, and V_i are described as for model (1), while *t* indicates time (survey wave) and $u_i + \eta_{it}$ is a composite error term that includes an individual-specific time-invariant unobserved component (u_i) and an idiosyncratic error term (η_{it}). Model (2) can be estimated by OLS but its main drawback is that the unobserved time-invariant individual heterogeneity is left entirely in the composite error term. This may not be problematic if individual heterogeneity is uncorrelated with the observed covariates. More serious is the possibility of serial correlation in the composite error term as OLS pools data across time. To partially eliminate the problem, I then estimate equation (2) using panel data techniques to control for time-invariant unobserved individual heterogeneity. As the covariate of interest (V_i) is itself time-invariant I apply random effects panel estimation[11].

To relax the assumption of orthogonality between u_i and the observed covariates the suggested (fourth) approach is to augment the random effects model with the time averaged values of the time-varying variables (Mundlak, 1978; Chamberlain, 1980; Wooldridge, 2010). This leads to the statistical model:

$$Y_{it}^{*} = \alpha_0 + \sigma_0 t + X_{it}\beta_0 + \bar{X}_{it}\beta_{10} + \gamma_0 C_2 + \delta_0 V_i + \mu_0 C_2 V_i + z_i + \eta_{it} \qquad (3)$$

where $\bar{X}_i$ is the time average of X_{it} and z_i is the individual effect. Adding $\bar{X}_i$ to the model as a control for unobserved heterogeneity allows one to estimate the effect of changing X_{it} whilst holding fixed the time average (Wooldridge, 2010).

[11] Panel estimation transforms the data by subtracting from each observation a portion θ of its time average, where θ depends on the variance of u_i and η_{it} and the number of period for which data are observed (Wooldridge, 2010). This quasi-demeaning of the data transforms equation (2) into:

$$Y_{it} - \theta\bar{Y}_i = \alpha(1-\theta) + \sigma(1-\theta)t + (X_{it} - \theta\bar{X}_i)\beta + \gamma(1-\theta)C_2 + \delta(1-\theta)V_i + \mu(1-\theta)C_2V_i + (\eta_{it} - \theta\bar{\eta}_{it})$$

Estimation of the above model yields consistent estimates under the assumption of orthogonality between u_i and the observed covariates as well as of $u_i \sim N(0, \sigma_u^2)$ and $\eta_{it} \sim N(0, \sigma_\eta^2)$. Although θ is not known in practice it can always be estimated (various methods are discussed in Wooldridge, 2009). An estimated θ close to zero results in random effect estimates being close to those obtained by pooled OLS, implying that time-invariant unobserved heterogeneity is relatively unimportant, as the variance of u_i is small relative to that of η_{it}. Conversely and more commonly, if the estimated θ is close to 1, then the variance of u_i is large relative to that of η_{it}, and the bias caused by unobserved time-invariant heterogeneity is large.

All regressions[12] are performed on the following broad specification:

$$Y_{it} = X_{it}\beta + \gamma C_t + \delta t + \mu i + \varepsilon_{it}$$

where the dependent variable is the labour market outcome of interest for an individual surveyed at a given time. The five outcomes considered are: the employment rate, occupational downgrading, the wage, whether the job carried out requires a lower level of education than the one held by the migrants (over-education), and a self-reported use of skills.

The independent variables include individual characteristics (educational level[13], age group[14]; English language proficiency based on whether the LSIA interview was conducted in English or in another language; main regions of origin[15]; and state of residence after settlement[16]). Other indicators include which cohort the migrant belongs to. This indicator also captures the economic conditions of the period when migrants settle in Australia. For analyses based on panel data, two additional indicators are included: a variable that controls for the wave in which data was collected; and a dummy variable for each individual surveyed, which controls for unobserved but time-invariant individual characteristics.

Table 2 reports the key outcome: the estimated link between a change in immigration policy and migrants' labour market outcomes controlling for all the observed

[12] The empirical analysis consists of six regressions. The first three are performed on cross-sectional data using the first wave of the LSIA for both cohorts. They are: (i) OLS based on model (1); (ii) PSM based on the nearest neighbour matching using the male's and females' common supports. These are depicted in Figure 6 and Figure 7 in the Appendix, respectively; and (iii) PSM using only the centre of the common support, after removing the observations in the propensity score's bottom and top 25%.
The next set of regressions is carried out on panel data using the first and second wave of the LSIA. They are: (iv) pooled OLS based on model (2); (v) the random effect estimator based on model (2); and (vi) the random effect estimator using Mundlak's correction based on model (3).

[13] Secondary and below, vocational, and tertiary.

[14] 4 categories: 25-35, 35-44, 45-50, and 50-65.

[15] 4 categories: North Europe/North America, South and East Europe/MENA, Asia, and rest of the world.

[16] 2 categories: NSW/Vic/ACT and Other.

covariates above. As a result, the table summarises what has been obtained by running 60 separate regressions - one for each labour market outcome, by gender[17], and across various techniques. Since discussing each underlying regressions is not practical for obvious reasons, only the key relationship of interest is analysed: namely if migration policy affects migrants' subsequent labour market outcomes.

Table 2 – Regression results

	Cross section (1st wave)			Panel (1st and 2nd wave)		
Model	**1**	**2**	**3**	**4**	**5**	**6**
Males			**(.2-.6)**			
Employment rate	.059*	-.020	.032	.037*	.039	.034
	(.031)	(.021)	(.022)	(.022)	(.026)	(.026)
Occupational downgrading	2.36	.422	1.17	2.47	n.m.	n.m.
	(1.89)	(1.038)	(1.24)	(1.94)		
Wage (ANU job score)	.012	-.046	-.051	.005	.010	.004
	(.054)	(.037)	(.046)	(.046)	(.048)	(.048)
Over-education	.018	.034	.028	.033	.036	.038
	(.036)	(.021)	(.028)	(.031)	(.030)	(.029)
Use of skills	.052	-.021	-.010	.055*	.052*	.050*
	(.035)	(.021)	(.025)	(.029)	(.029)	(.028)
Females			**(.2-.4)**			
Employment rate	-.006	.042	.114**	-.031	-.039	-.043
	(.048)	(.030)	(.048)	(.032)	(.038)	(.038)
Occupational downgrading	-3.58	-4.69**	-1.42	-3.23	n.m.	n.m.
	(2.76)	(1.90)	(2.56)	(2.80)		
Wage (ANU job score)	.157**	.242***	.232***	.091	.086	.087
	(.071)	(.053)	(.064)	(.056)	(.060)	(.057)
Over-education	-.105*	-.155***	-.200***	-.052	-.051	-.053
	(.056)	(.044)	(.057)	(.045)	(.043)	(.043)
Use of skills	.140***	.142***	.162***	.067*	.072*	.072*
	(.051)	(.034)	(.043)	(.041)	(.039)	(.039)

Notes: Source: LSIA1 and LSIA2, waves 1 and 2. n.m. = not meaningful. The numbering refers to the following models: (1) OLS; (2) propensity score matching (unrestricted scores); (3) propensity score matching (central scores only); (4) pooled OLS; (5) random effects; (6) random effects with Mundlak correction.

[17] Separate results are reported for males and females, as regressing pooled observations by gender do not satisfy the balancing property when the PSM technique is applied.

Few general observations linking the results to the findings of the previous literature are worth noting before discussing in some detail the overall picture that emerges from Table 2. First, the increase in employment rate documented by previous studies (Richardson et al, 2004; Cobb-Clark, 2003) is partly confirmed when policy-evaluation specific techniques are applied, but it arises only for males and in the first and fourth model applying a low significance level (10%). No detectable effect emerges when the effect of the policy change is estimated using estimators, which better control for individual heterogeneity (models 2, 3, 5, and 6). In the case of females, the only notable effect on employment arises using model 3. These outcomes imply a very modest effect, if any, of the policy change on the short-term employment prospects of the immigrants of the second cohort.

In contrast, the policy change seems to have had mixed effects on occupational downgrading (Junankar and Mahuteau, 2008), which can be read along the second row: there is no effect in the case of men, where the estimate is positive (implying that the occupation after migration is lower in the occupation scale than what was held before migrating) but statistically identical to zero.

It is negative and significant in the case of females but only in the second model. This result implies that upon migrating women take up jobs that are on a substantially lower level of the occupational scale relative to what they were performing in their countries of origin. However, part of this result may attributed to the characteristics of the migrants rather than the effect of the policy change due to a composition effect of the groups analysed by model 2. Furthermore, the effect of migration policy on occupational downgrading for women appears mixed across estimation techniques, suggesting additional caution in interpreting the result as conclusive that the policy change was principally responsible for the outcome observed.

With reference to wages, over-education, and use of skills the policy change has impacted immigrant men and women differently. In particular, the policy had no detectable statistical effect on the three indicators in the case of men in all models reported in Table 2. The small effect observed in the use of skills in models 5 and 6 indicates better matches between education and occupation, but it could arise from changing job or employer rather than being the effect of the policy change.

In contrast, the policy change seems to have significantly raised the wage and the use of skills, and lowered the degree of over-education among immigrant women, though only at the time of the first wave. This outcome likely reflects the higher proportion of unmarried women of working age in the second cohort (49% vs. 27% in the first cohort), which could be the result of concurrent circumstances characterising immigrant trends besides the policy change examined. None of these results holds in the panel data analysis, where the estimation controls for time-invariant unobserved individual characteristics. This suggests that results attributable to the effect of the policy change in models 1-3 may be caused by unobserved variables that affect migrants' quality rather than the policy change (e.g. higher volume of international students qualifying for permanent visa at the time of the second cohort).

Overall, the estimates shown in Table 2 are remarkably consistent in being statistically no different from zero, with only a handful of exceptions in the case of females. In the case of males, the policy change appears to have had no detectable effect on the five indicators of labour market performance regardless of whether using cross-sections or panel data techniques. The modest increase in the self-reported use of skills is confined to longitudinal models 5 and 6, which cover a longer period of time during which migrants had the opportunity to gain labour market experience and complete additional educational and professional qualifications in Australia. With

reference to the other indicators of performance, the policy change has no statistical effect on salaries, over-education (where the sign points to worsening conditions relative to those occurring before the policy change), and skills' usage.

In the case of females, the improvement across indicators of labour market performance only in the first wave of both cohorts may be caused by unobserved variables that affect migrants' quality but are unrelated to the policy change, such as an increase in the number of female international students completing their degrees in Australia and applying for permanent migration. Alternatively, this may reflect aspects of the policy change that are only corollary to immigration policy, such as the deferred access to welfare and/or heightened skill shortages in Australia, particularly in scientific and engineering jobs[18]. These changes may have prompted women to find a better education-occupation match in the labour market.

6. Labour market policy vs. immigration policy

The results shown in Table 2 leave the reader with the impression that the immigration policy change of the late 1990s had no real impact on the labour market performance of affected immigrants. Indeed this is the key message of the exercise carried out, notwithstanding that it is obtained from an imperfect database covering only the very short-term.

This conclusion raises the question of what other set of policy tools could be suitable to influence migrants' labour market outcomes. A natural answer is labour market policy, which broadly looks after the proper functioning of the labour market. In the case of migrants, employment departments guarantee the respect of legislation and established practices, ensuring, for example, that migrants' employment reflects the

[18] See reports prepared by the Australian Industry Group (*World Class Skills for World Class Industries*, 2004), the Department of Employment and Workplace Relations (DEWR, *Workforce Tomorrow*, 2005), and the Department of Education, Science and Technology (DEST, *Audit of Science, Engineering, and Technology Skills*, 2006).

laws of the country, and that they have access to the same rights and responsibilities enjoyed by natives. Employment departments are therefore the obvious regulatory reference point to oversee whether or not the skills brought by immigrants, especially if highly educated, are efficiently used, and if intervention is necessary.

Such responsibilities are markedly different from those of immigration departments, which broadly speaking cater for the orderly management of population inflows, but in countries that operate a selective immigration policy, like Australia, Canada and New Zealand, they also carry out the immigration selection mechanism, deciding whether applicants meet the stated selection criteria. They ultimately determine migrants' skill composition, which they regularly review using research covering economic and other aspects of integration. This has limited effect on outcomes once immigrants enter the host country's labour market, as shown by the results reported in Table 2.

This consideration raises another point: namely, to what extent it is desirable for immigration and employment policies to coordinate their aims and policy tools, at least with respect to migrants undergoing a selection process. The division of responsibility between immigration departments attracting foreign talent and employment departments ensuring its efficient usage in the labour market may generate discrepancies if carried out independently from each another. This presents a cost for the migrants, who may spend additional time working in jobs for which they over-qualify before their skills are properly utilised and rewarded, and for the host country's society due to the inefficient valuation of its immigrants' skills.

These results suggest that immigration and employment policy-making may benefit from working jointly to address issues of recruiting foreign talent and its subsequent utilisation in the labour market. Possible examples of collaborative work include the

development of accreditation programmes to ease the path into licensing for foreign-trained professionals, English language support, or access to finance for new start-ups. The coordination between immigration and employment policies is relevant as the costs of their 'going-it-alone' is not only borne by immigrants themselves and their families, but also by the host society at large, which does not benefit from the skills and talent that were selected (at some) cost and had been made available for use.

Unfortunately issues related to the efficiency of the labour market do not appear to be at the forefront of employment policies in several countries, including Australia, but at times of sluggish economic growth even a small forward step in improving efficiency in skill utilisation can make a substantial positive contribution to a country's economy.

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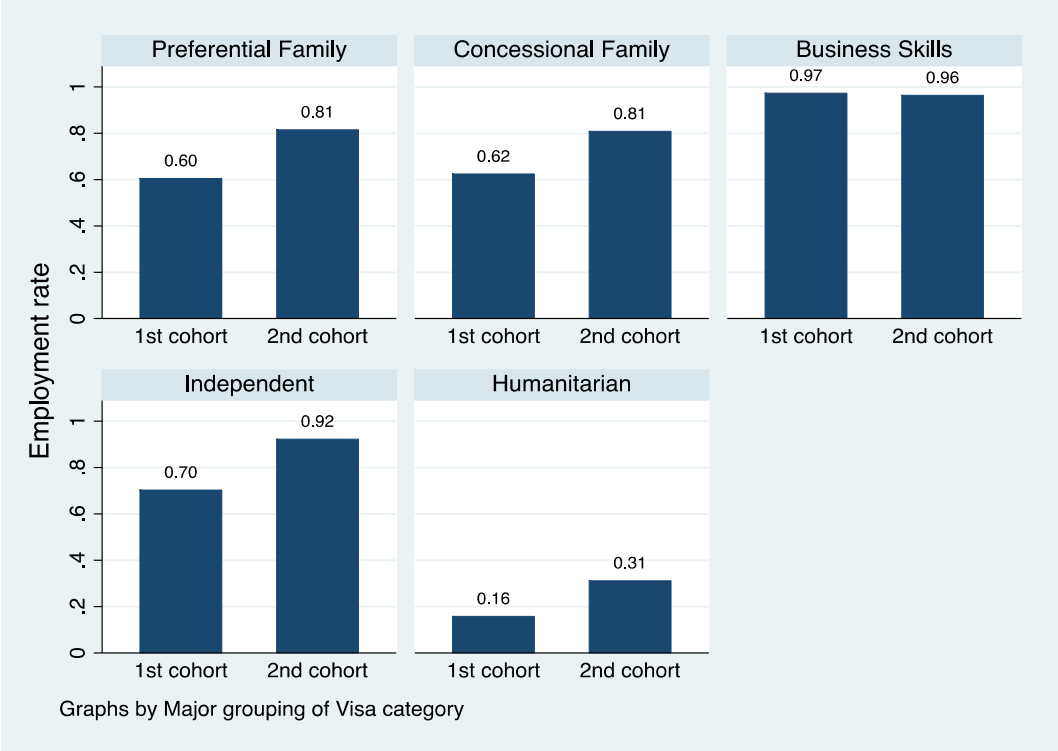
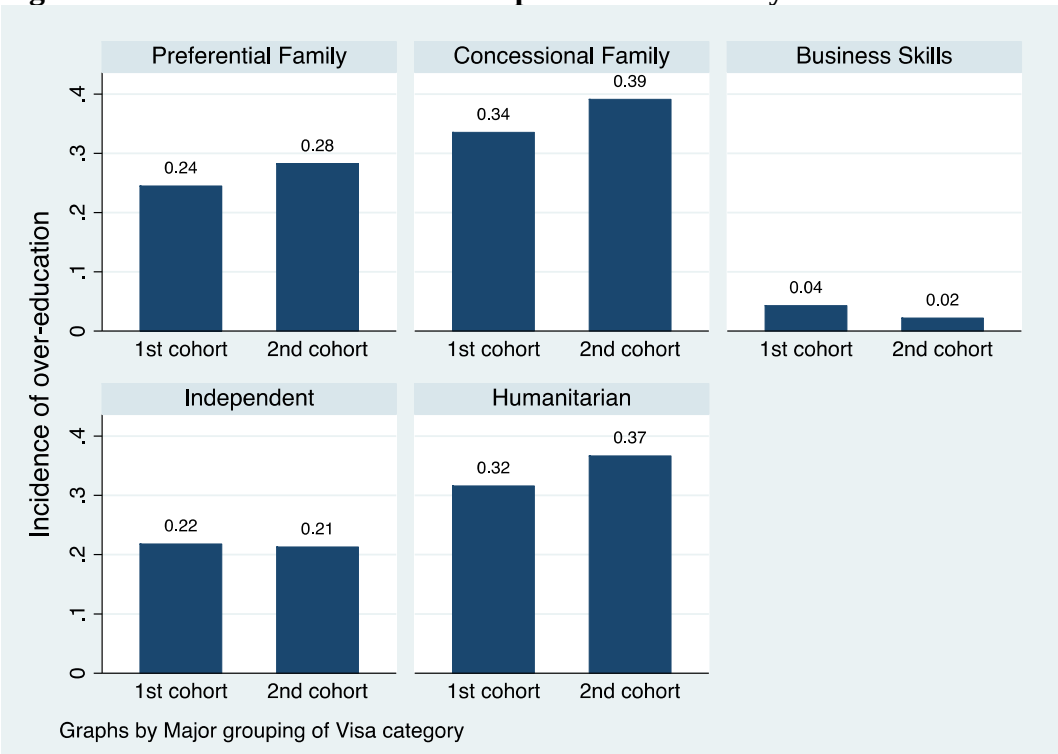
Appendix

Figure 1: Employment rate



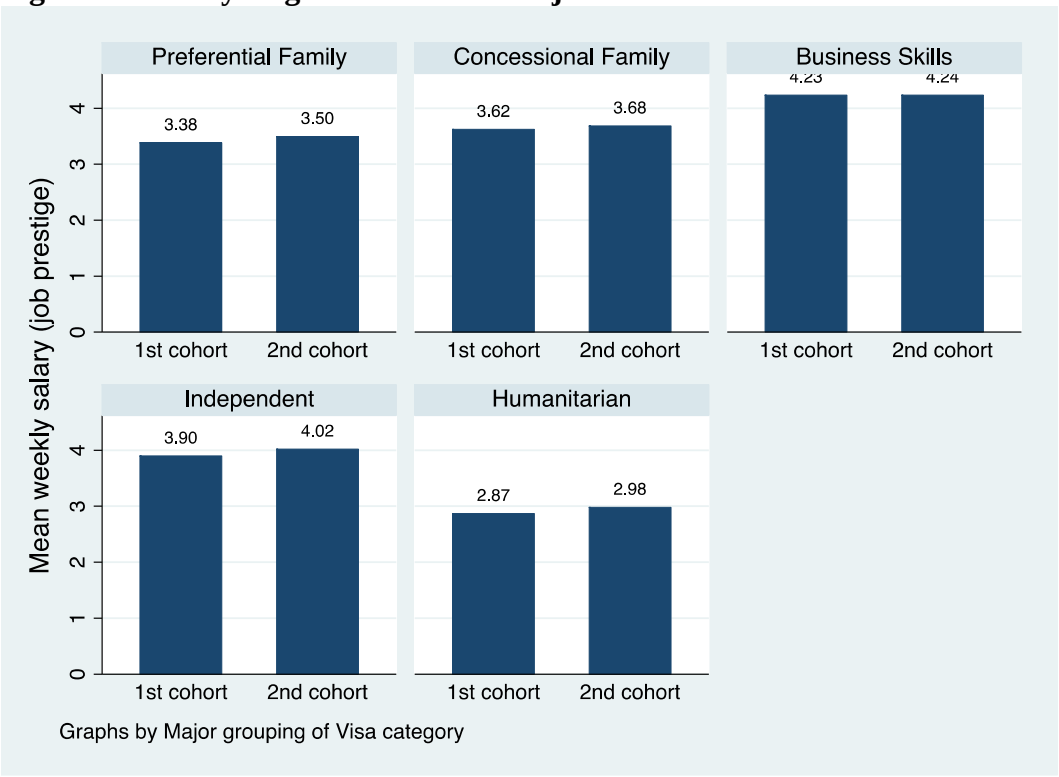
Source: wave 1, LSIA 1 and LSIA 2.

Figure 2: Incidence of education-occupation mismatch by visa class



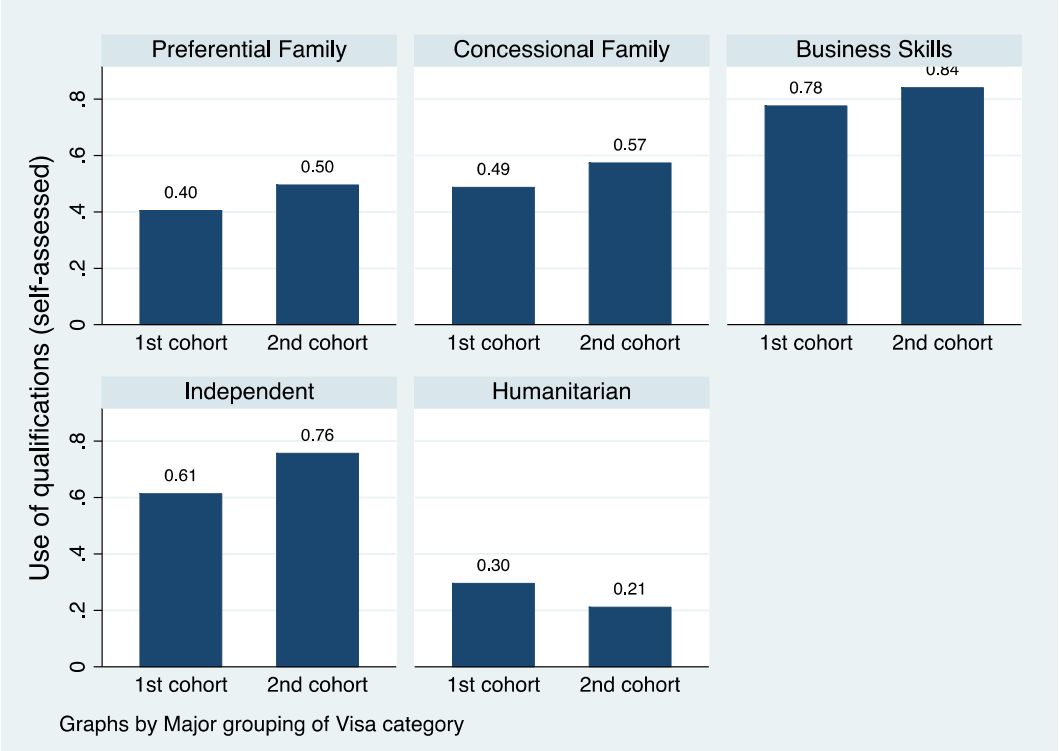
Source: wave 1, LSIA 1 and LSIA 2.

Figure 3: Weekly wage – based on ANU job score



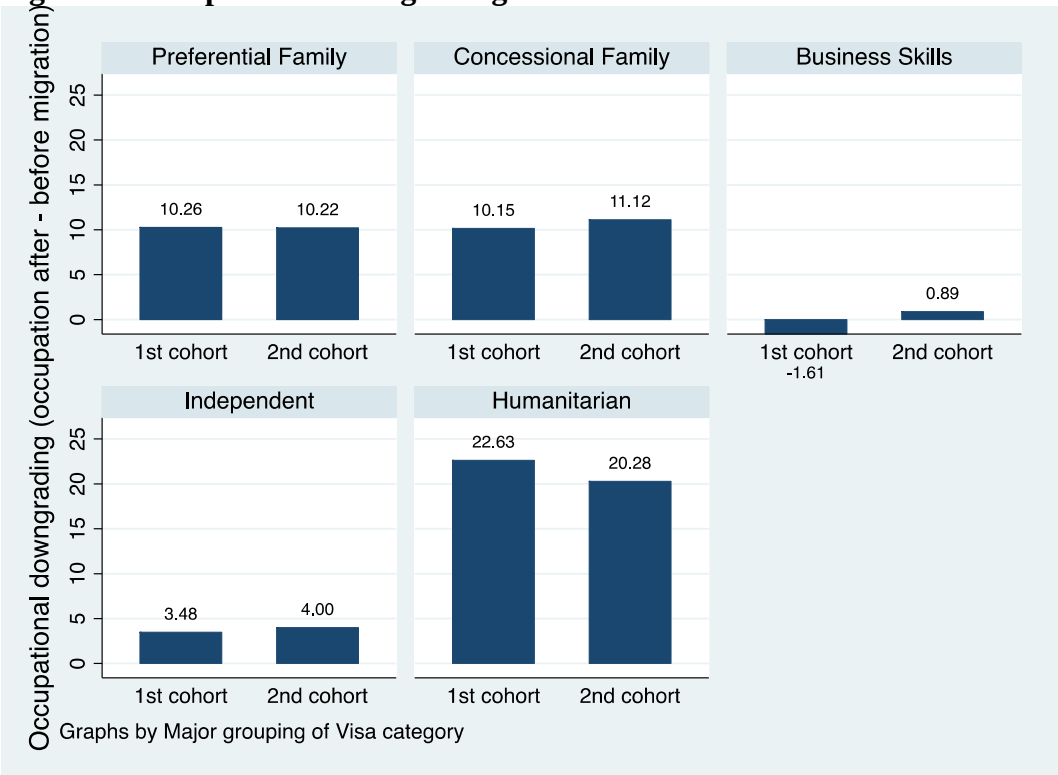
Source: wave 1, LSIA 1 and LSIA 2.

Figure 4: Use of qualifications (self-assessed)



Source: wave 1, LSIA 1 and LSIA 2.

Figure 5: Occupational downgrading



Source: wave 1, LSIA 1 and LSIA 2.

Figure 6: Common support - Males

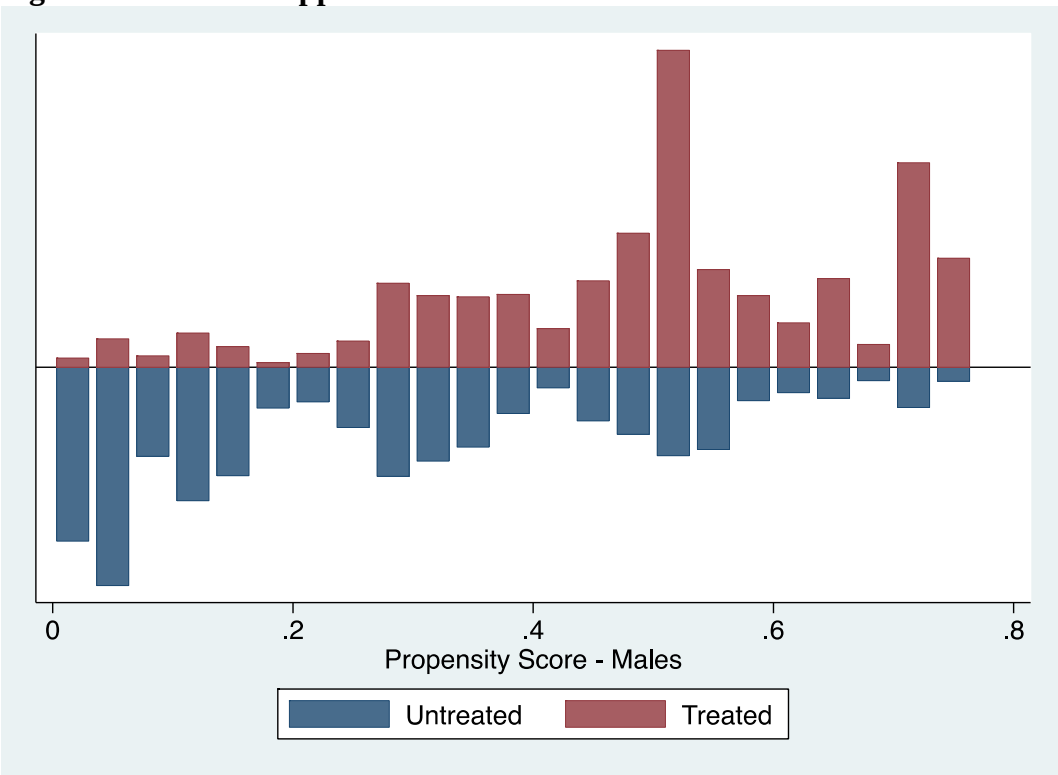


Figure 7: Common support - Females