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Article

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IZA Journal of Migration

Provided in Cooperation with:

IZA – Institute of Labor Economics

Suggested Citation: Kóczán, Zsóka (2016) : (Why) are immigrants unhappy?, IZA Journal of Migration, ISSN 2193-9039, Springer, Heidelberg, Vol. 5, Iss. 3, pp. 1-25,
<https://doi.org/10.1186/s40176-016-0052-4>

This Version is available at:

<https://hdl.handle.net/10419/149450>

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ORIGINAL ARTICLE

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(Why) are immigrants unhappy?



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Abstract

Recent studies suggest that migrants may be less satisfied with their 'new' lives than members of the host population and worry that this may be driven by cultural factors, such as feelings of not belonging. Motivated by this concern, this paper analyses the life satisfaction of immigrants once settled in the host country. We rely on the German Socio-Economic Panel's immigrant sample for the years 1984–2010 and find that while immigrants are less satisfied than natives, this difference can be explained by factors related to economic integration, such as the details of their employment conditions, rather than cultural factors such as feelings of not belonging, which often loom large in the public mind.

JEL codes: J15, K37, O15

Keywords: Integration, Subjective well-being, Segregation, Citizenship law

1 Introduction

In the popular mind, migration is often associated with an increase in well-being, as embodied in narratives of the 'promised land' and a move 'in hope of a better future'. In recent years, there has been increasing interest in measuring subjective well-being in economics; it has also featured prominently in public discourse and debates (e.g. the Stiglitz-Sen-Fitoussi Report on social progress and well-being, commissioned by Nicholas Sarkozy; the British National Well-Being Project embraced by David Cameron). Recent studies have also increasingly looked at the impact of migration and integration on life satisfaction and have raised concerns that migrants may be less satisfied than the native population and that this could be driven by cultural factors, such as feelings of not belonging. We thus aim to contribute to the literature by examining immigrants' outcomes from a subjective perspective, looking in particular at whether cultural or economic factors matter more for their life satisfaction. We analyse the immigrant sample of the German Socio-Economic Panel, for the years 1984–2010 (for a detailed discussion of the dataset please see Section 4).

We will examine two questions: (1) we will first look at whether (and if so, why) immigrants are less satisfied than natives (as suggested by some recent papers) and (2) we will then analyse the determinants of the subjective well-being of immigrants, in a sense examining 'who does better and who does worse' among the group of migrants.¹ Card et al. (2012) recently highlighted that public attitudes towards immigration policy are more influenced by cultural and social concerns than economic ones. Georgiadis and Manning (2013) note the widespread belief that societies will function better if they manage to establish a common sense of identity among the population and

contemporary fears in many countries that this common identity is threatened. We will look at various socio-economic and cultural measures that could be associated with integration, trying to shed light on the question which elements matter for subjective well-being.

As Graham (2008) put it: 'happiness surveys may shed light on how the *direction* and *nature* of progress affects well-being' (p. 85, italics in the original). Such potential policy implications may be interesting in the context of immigration as public discourse often debates the 'alternatives' of integration, assimilation or multiculturalism. Research on the subjective well-being of immigrants could thus have broader implications for social cohesion and hence policy.

The paper is structured as follows: Section 2 reviews the empirical literature on the life satisfaction of immigrants, Section 3 discusses our empirical approach and Section 4 introduces the dataset used. Section 5 presents the results including robustness tests and Section 6 concludes.

2 Literature review

In recent years, there has been increasing interest in the migration literature in subjective well-being, primarily linked to the question whether migration increases life satisfaction. Some studies have explicitly compared life satisfaction before and after migration. Stillman et al. (2012) provided a key contribution to this field, relying on a natural experiment to compare successful and unsuccessful applicants to a migration lottery to experimentally estimate the impact of migration on objective and subjective well-being. A recent paper by Melzer and Muffels (2012) used the German reunification as a natural experiment to examine whether migration from East to West increased life satisfaction, focusing on income effects as well as the role of social comparisons. Amit and Riss (2014) compared subjective well-being pre- and post-immigration among North Americans who arrived in Israel during the last two decades and found that social networks, a religious motive for migration and work satisfaction played important roles. Hwang et al. (2011) found negative effects of involuntary migration on migrants' well-being, despite relative gains in housing quality. Bartram's (2011) study circumvented the data constraint by contrasting the 'marginal increase in satisfaction from income' among natives and migrants and found that the association between income and subjective well-being is indeed stronger for immigrants than for natives—but even for immigrants that association is still relatively weak. Looking at economic migration and life satisfaction around the world, Olgiati et al. (2013) found a distinctive immigrant advantage in translating income into higher life satisfaction in some countries but also found a number of 'frustrated achievers' (immigrants who report a negative association between income and life satisfaction) in some other countries.

Graham and Markowitz (2011) took one step back and looked at the selection effect, examining whether migrants are different from the rest of the population in terms of subjective measures of well-being. They found that people who intend to migrate are generally less satisfied than those who do not (as could be expected); however, more broadly, they found that migrants are 'frustrated achievers' (as also noted by Olgiati et al. 2013), people who have relatively high levels of objective well-being, such as income, but who are nonetheless dissatisfied with their

situations and seek to improve them via migration. Cai et al. (2014) found that individuals with higher subjective well-being have lower international migration desires and that, at the individual level, the subjective well-being-migration relationship appears to be more robust than the income-migration relationship. Knight et al. (2007) examined the question why rural-urban migrant households settled in urban China have an average life satisfaction score lower than that of rural households. Three basic hypotheses were considered: migrants had false expectations about their future urban conditions, about their future urban aspirations or about their future selves. The disparity appears to be driven both by certain features of migrant conditions (in line with the classical literature on the psychology of migration and acculturative stress²) and by high aspirations (in line with Graham and Markowitz's (2011) notion of 'frustrated achievers'). In a related vein, Navarro-Azorín and Artal-Tur (2015) analysed the differences in well-being between Spanish municipalities reflected by people's migratory decisions, and Chindarkar (2014) looked at a sample of Latin American countries and found that life satisfaction is a significant driver of intention to migrate abroad. Lovo (2014) examined the preferences over migration destinations of those revealing a desire to permanently leave their country and found that people's preferences are better explained by average levels of life satisfaction in the destination country. Bartlett et al. (2010) highlighted the impact of out-migration on life satisfaction in rural areas. Betz and Simpson (2013), Akay et al. (2014) and Longhi (2014) looked at the impact of immigration on the well-being of natives.

A number of recent papers have looked at the well-being of immigrants once settled in the host country and have highlighted the relative dissatisfaction of migrants compared to the native population (e.g. Werkuyten and Nekuee 1999; Baltatescu 2007; Constant et al. 2012; Amit 2010; Bartram 2011; Safi 2010; Gokdemir and Dumludag 2012; Obućina 2013; Hendriks et al. 2014 referred to it as the 'migrant-local happiness-gap'). Two explanations were often brought forward for this: discrimination and feelings of not belonging or loyalties towards immigrants' countries of origin. The few papers that explicitly linked immigrants' ethnic/national identities to their life satisfaction usually found positive effects of strong majority identities and negative effects of strong minority identities on subjective well-being (Boski 1989; Phinney et al. 2001; Bartram 2011).

We hope to contribute to the existing literature on subjective well-being and migration by focusing on the life satisfaction of immigrants once settled in the host country. In particular, we aim to provide value added by looking at whether it is economic or the much-discussed cultural concerns which matter more for immigrants' life satisfaction. First, we examine whether immigrants are indeed less satisfied than the natives (as suggested by the existing literature). Second, we study the determinants of the life satisfaction of immigrants in greater detail, analysing the role of socio-economic as well as cultural factors. We attempt to deal with estimation difficulties resulting from the endogeneity of current characteristics (such as identity,³ discrimination or social contacts) and omitted variable bias (primarily due to unobserved personality traits) by relying on panel data. We use a fixed effects estimator to account for unobserved individual heterogeneity, examine changes over time and use lags to deal with reverse causality. We also examine restricted subsamples to assess robustness.

3 Estimation

Psychologists argue that life satisfaction is both a ‘trait’ and a ‘state’, i.e. it has a stable, ‘genetic’ component, linked to personality traits such as extraversion, conscientiousness and emotional stability, but it is also affected by circumstances, and these two factors can of course interact in influencing life satisfaction.⁴ In our empirical approach, we will thus explicitly acknowledge that subjective well-being depends on the interaction of a ‘genetic’ component, linked to personality traits, and circumstances, some of which may be more persistent than others. Most papers have looked at the determinants of life satisfaction relying on simple ordinary least squares (OLS); however, since the ‘trait’ and the ‘state’ components may interact in affecting life satisfaction (i.e. unobserved personality traits may influence subjective well-being, while also being correlated with the characteristics on the right-hand side), not taking into account this unobserved individual heterogeneity leads to biased estimates of the parameters of interest. We thus rely on panel data to compare a pooled OLS estimator with an estimator including individual fixed effects.⁵ Whereas the pooled OLS estimator allows us to look at the effects of stable as well as changing characteristics or circumstances, but may be confounded by personality traits, the fixed effects estimator uses repeated observations on an individual over time to parse out the ‘trait’ component to give cleaner measures of the ‘state’ component. Although this does not provide value added for the estimation of the effect of the immigrant dummy variable itself (which here by definition is time invariant), and we of course realize that for some variables such as education there is little time variation in our sample, we believe that comparing pooled OLS and fixed effects estimators can give us useful additional insights into whether, for instance, the roles of citizenship or details of employment conditions are driven by a selection effect or changes over time.

As noted before, we are interested in two questions: (1) whether immigrants are less satisfied than natives (as suggested by the existing literature) and (2) what factors influence the life satisfaction of immigrants. We thus start our analysis by looking at the full sample, including both natives and immigrants to examine the first question, and will then restrict the sample to the immigrants only to examine the second.

The existing literature stresses the importance of individual characteristics such as age, gender, marital status and years of education, the harm done by unemployment or by competitive struggles among individuals and the negative effects of serious illness. For a comprehensive early review on the determinants of subjective well-being see, e.g. Diener (1984). While we follow the literature in including controls for these characteristics, we extend this set to include variables which are of special interest in a migration context—please see Table 1 below. For a detailed discussion of each of these variables and available empirical evidence on them, please see Table 7 in the Appendix.

Table 1 Control variables

Personal and parental characteristics	Age, gender, marital status, children, education, employment, home ownership, parental education, rural/urban childhood, health
Migration-related variables	Years since migration, identity, German language skills, ethnic composition of neighbourhood, citizenship

4 Data⁶

We use data from the German Socio-Economic Panel (GSOEP), a large representative longitudinal survey of private households in Germany. Immigrants are oversampled in the GSOEP: our focus here is on the 'B Foreigner West sample', defined as those households where the head of household is Turkish, Italian, Spanish, Greek or from the former Yugoslavia. During the latter half of the 1950s, the German government started actively recruiting guest workers in response to a labour shortage prompted by high economic growth rates. In 1973, the government stopped the recruitment of further guest workers as Germany entered a period of economic recession. In the subsequent years, the inflow of immigrants from the former guest worker countries consisted mainly of family members of those guest workers who remained in Germany (family reunification).

The GSOEP was first administered in 1984; we use data until (and including) 2010. In the GSOEP, all household members are interviewed individually once they reach the age of 16. In principle, all persons who took part in the first wave of the survey as well as their children whenever born are surveyed in the following years. 'New' persons thus become part of the GSOEP by being born into a surveyed household/reaching the minimum respondent's age or by moving into a surveyed household (either from somewhere else in Germany or from abroad). Individuals who move out of a surveyed household or split off into new households are still followed (within Germany) but under a new household identifier. Individuals moving into existing GSOEP households are followed even if they subsequently leave that household. People exit the sample by death, moving abroad or refusal to interview. Individuals or households which could not be successfully interviewed in a given year are followed until there are two consecutive temporary dropouts of all household members or a final refusal. As noted above, compared to the German population, immigrants are oversampled: the 'B Foreigner West sample' started with 1393 households, and the sampling probability was about 0.0008. We have not reweighted the data, so our pooled regressions of natives and immigrants should not be seen as representing the German population. We restrict our analysis to first-generation immigrants as we do not wish to pool first- and second-generation immigrants together and the sample of second-generation immigrants who were asked about our variables of interest is unfortunately too small for separate analysis. However, we do not explicitly restrict the age range. Our sample is also not restricted to adults present at the start of the panel. Individuals can re-enter our sample under the same identifier if they drop out only temporarily. Children present in 'native' GSOEP households are allowed to enter our sample when they reach the minimum respondent's age. Our results are robust to explicitly enforcing common support on age (as discussed in Section 5.5) and to excluding them to match the exclusion of second-generation immigrants. We include all adults in the household who answered our questions of interest—our results are robust to clustering standard errors at the household level. Our findings also hold up when controlling for the number of years an individual is present in the panel (the median number of years in our sample is 9 for natives and 11 for immigrants). We also rely on the 'Microm indicators', a unique dataset providing information on the ethnic composition of neighbourhoods at various levels of aggregation, which can be merged with the GSOEP.⁷ These indicators are discussed in detail in Section 5.3.

We use the standard life satisfaction survey question as our dependent variable:

How satisfied are you with your life, all things considered?

with answer categories ranging from 'completely dissatisfied' (0) to 'completely satisfied' (10). This Satisfaction With Life Scale (SWLS) was developed to assess satisfaction with the respondent's life as a whole. The scale does not assess satisfaction with life domains such as health or finances separately but allows subjects to integrate and weigh these domains in whatever way they choose. It therefore assesses an individual's conscious evaluative judgment of his or her life by using the person's own criteria (Pavot and Diener 1993). In the following, we briefly discuss the reliability of this scale as a measure of life satisfaction.

The SWLS has been shown to be psychometrically sound and well-validated (see, e.g. Diener 1984 or Pavot and Diener 2008 for a review): it shows good convergent validity with other scales and with other types of assessments of subjective well-being. Its scores have been shown to correlate with measures of mental health and to be predictive of future behaviours such as suicide attempts (Diener et al. 1985).⁸ New evidence from neuroscience also supports the reliability of life satisfaction questions: Davidson (2002, 2004) identified areas in the prefrontal cortex where the level of electrical activity is highly correlated with self-reported happiness, both across people and 'within' people over time. Larsen et al. (1983) also found that such single life satisfaction measures did not seem to be highly contaminated by social desirability.

Given our comparison between natives and immigrants, an important concern is culture-based anchoring bias, i.e. cultural differences in response styles, so that people in different cultures might report different answers to the same question, even if in other respects their life quality is the same (see, e.g. Oishi (2002) for a discussion of concepts like 'happiness' or 'well-being' in different languages and cultures or Senik (2011) on the 'French unhappiness puzzle'). One basic check is to see to what extent the answers drawn from different countries and cultures appear to be influenced by the same factors: Rose and Özcan (2007) found that the determinants of life satisfaction in Turkey are very similar to those observed in the EU15, increasing our confidence in these measures. Also, overall means are roughly in line according to both the Eurobarometer and World Values Survey results.

There is evidence that momentary mood influences subjects' responses to subjective well-being questions (Schwarz and Clore 1983). This finding is consistent with memory research (e.g. Natale and Hantas 1982), which shows that people tend to recall past events that are consonant with their current affect. However, despite the influence that current mood can have on subjective well-being measures, Kammann and Flett (1983) and Kammann et al. (1979) presented evidence indicating that this does not substantially distort scores, with both current mood and long-term affect being reflected in life satisfaction measures. Diener and Larsen (1984) found substantial amounts of cross-situational consistency in mean levels of person affect. Life satisfaction as assessed by the SWLS shows a degree of temporal stability (e.g. 0.54 for 4 years), with part of this being explained by personality and part by the stability of conditions in the respondents' lives. Yet, it has sufficient sensitivity to be able to detect changes in life satisfaction (Pavot and Diener 1993).

Overall, subjective well-being measures seem to contain a substantial amount of valid variance; however, of course, the limitations of using such self-reported data need to be kept in mind in the following analysis.

4.1 Descriptive statistics

Table 2 reports descriptive statistics for the natives and first-generation immigrants in 2010 and looks at economic as well as cultural characteristics. In terms of simple averages, the natives seem slightly more satisfied than the first generation (7.1 versus 6.7).

The immigrants are somewhat less educated, have less secure jobs, are less likely to work in the occupation they were trained for and are less likely to own the house/apartment they live in (in line with the findings of Constant et al. 2012). Almost 60 % of immigrants in our sample have a strong minority identity, while 20 % have a strong majority identity (in terms of overlaps, 10 % have a dominant majority identity, 43 % have a dominant minority identity, 39 % have two strong identities and 8 % have two weak identities). Around a quarter of the first generation in our sample has German citizenship by 2010.

In terms of life satisfaction, there is almost as much variation ‘within’ individuals over time as there is between them (standard deviations of 1.30 and 1.49, respectively) and there is substantial movement between categories over time; the correlation of this year’s life satisfaction with last year’s is 0.59, for 5 years ago, it is 0.44 (this is in line with the existing literature). We do not observe systematic differences between immigrants and natives in this respect.

Table 2 Descriptive statistics, 2010

	Mean	
	Natives	Immigrants
Life satisfaction	7.112	6.678
Age	44.203	51.392
Male	0.485	0.515
Married	0.583	0.781
Years of education	12.465	9.996
Employed	0.51	0.447
Works in occupation trained for	0.68	0.642
Owns house/apartment	0.589	0.362
Has insecure job	1.636	1.879
Serious illness	0.407	0.365
Years since migration		33.934
Minority identity		0.58
Majority identity		0.202
German citizenship		0.245
Speaks German		0.837
Writes German		0.593

Note: All differences between groups are significant at the 1 % level

5 Results

5.1 Are immigrants less satisfied?

Table 3 below reports fixed effects results as well as pooled OLS for comparison, for a combined sample of natives and first-generation immigrants. As noted before, while most of the literature on subjective well-being relied on simple OLS, we also examine a fixed effects specification to parse out the effects of the ‘trait’ component, in particular unobserved personality traits, to obtain cleaner measures of the effects of circumstances (the ‘state’ component). Again, while we realize that unfortunately this provides little value added for some variables which have no or little time variation in our sample (such as the immigrant dummy variable or education), we hope to gain insights by comparing the pooled OLS and fixed effects estimators, for instance, when looking at the impact of the details of employment conditions or certain cultural factors.

The first two columns show a simple specification: in line with much of the earlier literature, we find a negative effect of age and a negative effect of being divorced or

Table 3 Are immigrants less satisfied?

	POLS (1)	FE (2)	POLS (3)	FE (4)	POLS (5)	FE (6)
Age	0 (0.002)	-0.027*** (0.003)	-0.011*** (0.002)	-0.044*** (0.003)	-0.005 (0.003)	-0.029*** (0.006)
Male	-0.039 (0.043)	(omitted)	0.125** (0.045)	(omitted)	0.103 (0.057)	(omitted)
Married	0.105 (0.059)	0.046 (0.091)	0.037 (0.064)	0.184** (0.065)	0.081 (0.088)	0.085 (0.139)
Separated/divorced	-0.572*** (0.103)	0.004 (0.157)	-0.492*** (0.1)	-0.006 (0.103)	-0.510*** (0.138)	-0.03 (0.219)
Widowed	-0.248 (0.151)	-0.752* (0.317)	-0.545** (0.191)	-0.091 (0.214)	-0.848** (0.296)	-1.898** (0.669)
Years of education	0.033*** (0.008)	0.01 (0.029)	0.008 (0.01)	0.034* (0.017)	-0.001 (0.012)	0.026 (0.057)
Employed (lagged)	0.224*** (0.041)	0 (0.049)	0.098* (0.043)	0.05 (0.035)	0.182* (0.078)	0.087 (0.093)
Immigrant	-0.276*** (0.055)	(omitted)	0.087 (0.061)	(omitted)	0.072 (0.09)	(omitted)
Works in occup.			0.131*** (0.037)	0.086** (0.029)	0.012 (0.051)	0.006 (0.067)
Owner			0.238*** (0.044)	0.099* (0.048)	0.178** (0.062)	0.1 (0.12)
Children in household			-0.005 (0.039)	0.001 (0.038)	0.001 (0.057)	-0.055 (0.082)
Insecure job					-0.430*** (0.032)	-0.236*** (0.035)
Number of obs.	71,779	71,779	29,835	29,835	28,369	28,369
Number of ind.	8256	8256	3929	3929	3888	3888

Note: Standard errors in parentheses (clustered at the individual level). * denotes significance at 5 %, ** at 1 % and *** at 0.1 %. Columns 3–6 also control for parental education, urban/rural childhood and health. ‘POLS’ refers to a pooled ordinary least squares specification, ‘FE’ to fixed effects

widowed. Years of education has a positive effect, though this is no longer significant with fixed effects—unsurprising, since, given our sample of adults, education does not change much over time ‘within’ individuals. As expected, employment has a positive effect (it is lagged here to avoid reverse causality, similar positive effects are found for contemporaneous values or longer lags). In line with the earlier literature, which used similar specifications, we find that the first generation still seems less satisfied than the natives, even controlling for characteristics such as education and employment. In fact, the coefficient on the immigrant dummy (-0.276) roughly corresponds to the difference observed in raw means (6.7 for the migrants versus 7.1 for the natives).⁹

Columns 3 and 4 examine whether this relative dissatisfaction effect holds up when we introduce further control variables. Note that the large fall in the number of observations between the two specifications is driven by the fact that the health question was not asked each year and that information on parental education and rural/urban childhood is not available for all respondents. Repeating the simple specification of columns 1 and 2 on this smaller sample yields very similar results (please see Table 8 in the Appendix). Our main finding is that the first generation is no longer less satisfied than the natives; the effect is mopped up by the details of employment conditions, in particular whether the respondent works in the occupation they were trained for.¹⁰ This question is not usually asked explicitly in other surveys and may capture employment effects that are not picked up in other analyses. The GSOEP first asks respondents about their current position/occupation (‘What is your current position/occupation? Please give the exact title.’) and then asks, ‘Is this position the same as the profession for which you were educated or trained?’ with answer categories ‘yes’, ‘no’, ‘currently in education/ training’ and ‘I have not been trained or educated for a particular profession’ (the last two categories are omitted here; the analysis focuses on the ‘yes’-‘no’ binary distinction). Younger respondents are more likely to work in the occupation they were trained for; this is the case both for immigrants and for natives (64 % of immigrants in the 20–30 age group work in the occupation they were trained for, as opposed to 58 % of the 31–50 year olds and 61 % of 51–65 year olds; the corresponding percentages for natives are 76, 64 and 64 %), though the immigrant-native gap is also largest for this group. Lower educated immigrants and natives are also less likely to work in the occupation they were trained for (58 % of immigrants with up to 12 years of education work in the occupation they were trained for, as opposed to 67 % of those with more than 12 years of education; the corresponding shares are 67 and 76 % for natives). Immigrants are less likely to work in the occupation they were trained for in the first few years after migration (only 54 % of those who migrated up to 5 years ago work in the occupation they were trained for, or 57 % of those who arrived up to 10 years ago, as opposed to 60 % for those who migrated more than 10 years ago) and if they arrived at an older age (64 % of those who arrived under the age of 20, as opposed to 57 % of those who arrived between the ages of 21 and 40, work in their original occupation; ratios are similar splitting the sample at age 14). Immigrants with and without German citizenship do not appear to be systematically different along this dimension; German language skills however matter: 55 (56) percent of those who speak (write) German ‘poorly’ or ‘not at all’ work in the occupation they were trained for, as opposed to 62 (66) percent of those who speak (write) German ‘well’ or ‘very well’—the latter are close to the shares for natives.

This specification looked at the effects of variables, which were as predetermined as possible with respect to current life satisfaction (occupational choice occurred in the past, before migration). Although we would also be interested in the effects of current characteristics, such as the degree of job insecurity, here we are more worried about reverse causality: that people who are less satisfied with their lives as a whole select into more insecure jobs, possibly creating a downward spiral. As it is difficult to find valid instruments for job insecurity, which would not affect subjective well-being, we did not include this measure in our preferred model. Columns 5 and 6 extend the specification of columns 3 and 4 by also including a measure of the insecurity of the respondent's last job.

Job insecurity is a subjective measure here, assessed on a one-to-three scale,¹¹ and shows a highly significant negative effect. Note that this is likely to be correlated with whether the respondent works in the occupation they were trained for, explaining why this variable is no longer significant. To try to deal with the likely endogeneity of job insecurity (and possible bias due to its subjective nature), we examine robustness to using a predicted measure of job insecurity instead. Relying on a first-stage regression of job insecurity on years with the firm, years of employment and its square (to allow for non-linear effects) and occupational categories, and using these predicted values in the second stage (correcting standard errors using bootstrapping), we find that job insecurity still has a highly significant negative effect, whereas the first-generation immigrant dummy variable is no longer significant. Our results are in line with the findings of Clark et al. (2010) who looked at the impact of job insecurity more generally and found that (at least for men) it significantly reduces well-being and is more important than the simple employment-unemployment distinction. Origo and Pagani (2009) noted that what matters for job satisfaction is not just the type of contract but mainly the perceived job security, which may be independent of the type of contract.

Note that job insecurity (as well as working in the occupation trained for in columns 3 and 4) had significant effects in both the pooled OLS and the fixed effects specifications, thus even once we look at the cleaner measure, removing the effects of unobserved individual heterogeneity. The Hausman test favours the fixed effects specification for all models, pointing to the role of unobserved individual heterogeneity, in particular personality traits.

Overall, our results are in contrast with the conclusions of Bartram (2011), Safi (2010) and Baltatescu (2007), who argued that even after controlling for a number of personal characteristics immigrants are less satisfied than natives; however, they only included measures of education and employment, but did not control for the nature of the job and may thus have been affected by omitted variable bias.

As most of the existing literature found that immigrants are less satisfied than natives, but our results so far do not provide evidence for this, we also examine whether immigrants may be hurt more by adverse shocks such as losing a job or facing decreasing job security. Looking at the effects of job loss (constructed as 1 for those who were unemployed at time t but employed at time $t - 1$, 0 for those employed at time t and at time $t - 1$) and increasing job insecurity (job insecurity at t higher than at $t - 1$), these have highly significant effects of the expected signs. Their interactions with the first-generation dummy variable are, however, not significant, suggesting that while becoming unemployed decreases life satisfaction significantly for natives as well as migrants, immigrants are not hurt more (please see Table 9 in the Appendix). While this may seem surprising at first sight, it can be explained by the fact that guest workers in

Germany have full access to the German welfare system and having resided in Germany for over 30 years have most probably also built up their informal security nets.

Overall, answering our first question, although in terms of raw means immigrants indeed seem less satisfied than natives, the difference can be explained in terms of observables, in particular details of employment conditions. In fact, similar economic characteristics seem to affect the life satisfaction of immigrants as for the native population.

5.2 What determines the life satisfaction of immigrants?

Turning to the second question, what determines the life satisfaction of immigrants, Table 4 restricts the sample to first-generation immigrants to examine the effects of variables related to ‘integration’. Again we contrast the pooled OLS results as the baseline with a fixed effects estimator, which provides cleaner estimates by removing the effect of personality traits. Note that unfortunately the sample size here is much smaller (both in terms of individuals and in terms of person-years) as some of these ‘cultural’ questions (for instance, on feelings of belonging) were not asked each year and were in part only asked of a random subset of immigrants.

Looking first at columns 1 and 2, neither majority nor minority identity has a significant effect in either of the specifications—such loyalties/feelings of belonging thus do not seem to play a role *per se*. This finding is in contrast with the early results of Boski (1989) or more recently Bartram (2011). We believe that this difference can be explained by the fact that we used a lagged measure of identity to avoid reverse causality and accounted for unobserved individual heterogeneity using fixed effects. We also examined alternative identity measures such as ‘feelings of not belonging in Germany/feeling stateless’ or ‘not feeling at home in the country of origin either’—neither of these were significant once we controlled for individual fixed effects. These results are also robust to allowing for multiple, overlapping identities.¹²

Table 4 What determines the life satisfaction of immigrants?

	POLS (1)	FE (2)	POLS (3)	FE (4)
Years since migration	-0.014 (0.01)	(omitted)	-0.004 (0.009)	-0.045 (0.027)
Min. identity (lagged)	0.003 (0.063)	0.032 (0.08)	0.026 (0.058)	0.036 (0.065)
Maj. identity (lagged)	0.24 (0.14)	0.007 (0.157)	0.245* (0.119)	0.051 (0.126)
German citizenship	3.986*** (0.513)	(omitted)	2.229** (0.754)	0.899 (1.031)
Speaks German	0.221* (0.092)	0.173 (0.101)		
Writes German	-0.012 (0.114)	0.056 (0.137)		
Number of obs.	2837	2837	3741	3741
Number of ind.	1170	1170	1231	1231

Note: Standard errors in parentheses (clustered at the individual level). * denotes significance at 5 %, ** at 1 % and *** at 0.1 %. Other control variables are as in columns 3 and 4 of Table 3. 'POLS' refers to a pooled ordinary least squares specification, 'FE' to fixed effects

Better German language skills have the expected positive effect—they bring benefits in the labour market as well as facilitating social contact with Germans. They are no longer significant once we include fixed effects, since, as for education, given our sample of first-generation immigrants who arrived in Germany over 30 years ago, there is little variation in German language skills ‘within’ individuals over time. Results on other control variables are very similar to those reported in Table 3, in particular working in the occupation trained for is still significant (if included job insecurity has a similar significant negative effect as before).

Note the large and highly significant positive effect of German citizenship in the pooled OLS specification—the coefficient would imply an increase in life satisfaction from 6 to almost 10 on the 0–10 scale. In this specification, it drops out when using fixed effects due to collinearity with German language skills—unsurprising since this is a requirement for citizenship. Examining the results without controlling for German language skills (which are unlikely to change much for our sample of first-generation immigrants and are thus mopped up by individual fixed effects anyway), German citizenship is still highly significant in the pooled OLS specification, but is no longer significant when we include individual fixed effects (please see columns 3 and 4 in Table 4). While very few immigrants had citizenship in 1984, this increased to about 25 % by 2010; 299 first-generation immigrants are observed changing citizenship in our sample. In relation to our sample size in Table 4, we would expect this to be large enough to show a detectable effect. This thus suggests that we may be picking up a selection effect in the pooled OLS results rather than a positive effect of obtaining citizenship.¹³ This also seems more likely given the size of the effect and that German citizenship brings few additional benefits relative to permanent rights of residence. We return to a discussion of this effect in greater detail in Section 5.4 when looking at the impact of changes in the German citizenship law.

5.3 Does segregation affect life satisfaction?

As there is a large literature on the effects of segregation on ‘objective’ measures such as the education or employment outcomes of immigrants, we examine whether the ethnic composition of neighbourhoods matters for their subjective well-being.¹⁴ Unfortunately, the GSOEP only contains a rough, self-reported measure of the ethnic composition of neighbourhoods (respondents were asked whether there are ‘no’, ‘few’ or ‘many’ foreigners in their neighbourhood). Looking first at this self-reported measure, we find that while a more ‘immigrant’ neighbourhood seems to have a negative and significant effect on the life satisfaction of immigrants in the pooled OLS results, this effect disappears once we control for fixed effects (please see the first row of Table 5) and is no longer significant in either specification once we control for housing quality (please see the second row in Table 5).¹⁵

However, as we are worried about potential biases in such a self-reported measure, we also analyse the effects of segregation using a unique dataset, which provides information on the ethnic composition of neighbourhoods at various levels of aggregation. We combine information on the ethnic composition of neighbourhoods from the Microm indicators with personal information from the GSOEP. We use two measures of the ethnic composition of neighbourhoods: (1) a one-to-nine scale, which was

Table 5 Does segregation affect life satisfaction?

	POLS	FE
Ethnic composition of neighbourhood	-0.284**	0.011
(subjective measure)	(0.091)	(0.152)
Number of obs.	1552	1552
Number of ind.	1117	1117
Ethnic composition of neighbourhood	-0.18	-0.122
(subjective measure), controlling for housing quality	(0.119)	(0.193)
Number of obs.	771	771
Number of ind.	622	622
Ethnic composition of neighbourhood	-0.025	0.06
(1–9 scale, 'house' level)	(0.02)	(0.08)
Number of obs.	1052	1052
Number of ind.	487	487
Ethnic composition of neighbourhood	0.002	
(percent of foreigners, 'pl8' level)	(0.009)	
Number of obs. (ind.)	540	

Note: Standard errors in parentheses (clustered at the individual level). * denotes significance at 5 %, ** at 1 % and *** at 0.1 %. Control variables as above. 'POLS' refers to a pooled ordinary least squares specification, 'FE' to fixed effects

constructed so that roughly 10 % of the population falls into each category; this is measured at the 'house' level, so it includes at least five households, or more if located within the same building (this measure is available for the years 2000–2010) and (2) the fraction of foreigners in the population measured at the 'pl8' level—German post-codes consist of five digits; this subdivision adds a further three digits creating areas of roughly equal sizes (this measure is only available for 2010).¹⁶

Examining pooled OLS and fixed effects regressions using the same control variables as above, neither of these objective measures of the ethnic composition of the neighbourhood has a significant effect in any of the specifications (please see the third and fourth rows in Table 5). These results hold up when looking at immigrants and natives separately and when looking separately at the employed and unemployed. As the immigrant literature highlighted different effects of segregation for higher/lower educated immigrants,¹⁷ we repeated the above analysis for education levels above and below 12 years—the ethnic composition of the neighbourhood still did not have a significant influence on the life satisfaction of either of these groups.

We also restricted the sample to those who have not moved recently (varying the cut-off points) to avoid reverse causality due to life satisfaction affecting residential choices and looked separately at those who do/do not express a wish to move. Results were very much in line with those above, thus increasing our confidence in our findings. Including 'years since move' as a control variable did not affect results and was not significant, in line with the psychological evidence that such effects should die out quickly. We also examined whether for those who moved in the years for which we have data on ethnic composition (2000–2010) there is a different effect before and after the move: ethnic composition of the neighbourhood still did not have a significant effect in either period. Including percentages of Turkish or ex-Yugoslav immigrants instead of the general percentage of foreigners did not change the results either.

5.4 Changes in the German citizenship law

Returning to the results in Table 4, we found a large, positive and highly significant effect of having German citizenship. We now examine the source of this effect in greater detail by exploiting the variation coming from changes in the German citizenship law. We rely on two natural experiments: the 2000 change, introducing elements of *jus soli* into the previously *jus sanguinis*-based framework and the earlier 1990/1991 changes to naturalization requirements.

In May 1999, the German parliament amended the Citizenship and Nationality Law of 1913. Under the original law, a child born in Germany was granted German citizenship only if at least one parent had German citizenship at the time of its birth. The new reform introduced elements of the birthright citizenship system: a child born to foreign parents on the 1st of January 2000 or after was eligible for citizenship at birth if at least one parent had been ordinarily resident in Germany for 8 years when the child was born and had been granted a permanent right of residence.¹⁸ We examine the impact of this reform by comparing households composed of foreign parents whose youngest child was born in Germany between 1990 and 1999 who had resided in Germany for more than 8 years at the time of the child's birth (the treatment group), with those who have children in the same age group, but who did not satisfy the residence requirement (the control group).¹⁹ We examine the effect of eligibility (intention to treat) using the following regression:

$$L_{ijt} = \beta_0 + \beta_1 T_j + \beta_2 D_t + \beta_3 T_j D_t + \gamma X_{ijt} + \mu_t + u_{ijt} \quad (4)$$

where L_{ijt} is the subjective well-being of parent i living in household j at time t ; T_j is the treatment dummy, differentiating the treatment and control groups defined above; and D_t takes the value 1 for surveys after the reform was passed in parliament (May 1999) and is 0 otherwise. X_{ijt} are personal characteristics including age, gender, marital status, years of education and employment status. Year dummy variables, μ_t , control for time-specific shocks affecting all individuals. Our parameter of interest is thus β_3 , measuring the average effect of the introduction of *jus soli* citizenship on parental life satisfaction. A change in the citizenship law could affect parental well-being as parents' preferences and attitudes towards the host country might change when their descendants get their 'new' status, thus improving their economic opportunities as well as possibly sending a more symbolic message of acceptance.

Unfortunately, the GSOEP does not provide information on children's citizenship, so we are unable to see what percentage of children in our sample actually changed citizenship as a result of the law. Aggregate numbers, however, suggest that there was a significant increase in naturalizations for the eligible cohorts relative to other cohorts and the change in the citizenship law also received a lot of attention in the media; thus, as far as the 'symbolic' message is concerned, we could expect to see an effect on parents of eligible children even if in the end they did not apply for citizenship.

Estimating the above specification using OLS, clustering standard errors at the individual level, we are unable to reject our null hypothesis of no effect of the change in the citizenship law on parents' life satisfaction (please see the first row of Table 6; 77 % of our sample are in the treatment group). As this could be

Table 6 Effects of changes in the German citizenship law

	Coef. (Std. error)	Number of obs.	Number of indiv.s
2000 change	-0.143 (0.104)	6787	1015
1991 change—16–23 year olds, broad treatment group	-0.159 (0.135)	6427	2149
1991 change—16–23 year olds, narrow treatment group	-0.166 (0.133)	6001	1971
1991 change—over 23 years old	0.027 (0.12)	7341	1277

Note: Of the number of individuals reported in rows 2–4, 79, 34 and 63 % fall in the treatment group, respectively

partly explained by the fact that only own (rather than children's) citizenship plays a role in subjective well-being, we also examine the changes in naturalization requirements in the early 1990s.²⁰ The 1990 law (enacted in April 1990, effective from the 1st of January 1991) made it easier for young immigrants between the ages of 16 and 23 to obtain citizenship and also liberalized naturalization requirements for those over 23 years, who had resided legally in Germany for 15 years and applied for citizenship before the 31st of December 1995.

In the following, we examine the effects of the 1990/1991 reform on those aged 16–23 and those over 23. Our first specification compares those aged 16–23 at some point in the years 1990–1995 (i.e. those born 1967–1979) with those born after 1979 and those born 1960–1967, who had not resided in Germany for 15 years yet so were not affected by other changes (immigrated after 1976). As it is unclear *ex ante* how near- or far-sighted individuals are with respect to such changes, we also examine a narrower treatment group, looking only at those who were immediately affected, i.e. 16–23 in the years 1990–1991 (born 1967–1975), comparing them with the same control group as above. Furthermore, we examine the effect on those who had been in Germany for more than 15 years in 1991, comparing them with those who had been there less than 15 years. We compare those who arrived 1970–1976 with those who arrived 1976–1980 to restrict the difference in age between treatment and control groups. Our results are robust to varying the 'time windows' of the treatment and control groups.

Our key finding is that none of these effects was significant (please see rows 2–4 in Table 6). Combined with our earlier result that citizenship did not have a significant effect once we controlled for individual fixed effects, we believe that our analysis looking at changes in the German citizenship law confirms the hypothesis that the pooled OLS specification was most likely picking up a selection effect, that even after controlling for observables those immigrants with German citizenship are systematically different from those without it. In a sense, German citizenship may just be identifying those who are 'of better types,' i.e. there may be a positive correlation between German citizenship and certain unobserved personality traits, so the effect we are picking up is a selection story not a 'jump' in subjective well-being due to obtaining citizenship.

While these changes in the German citizenship law provided an interesting natural experiment as elements of birthright citizenship were introduced into a previously descent-based system and naturalization requirements were liberalized,

further research could examine whether and how the effects of having/obtaining citizenship in birthright-based systems such as the US, Canada or France differ from those found here.

5.5 Robustness checks

Although the results presented in Section 5.1 controlled for a number of personal and family characteristics, we may be worried that we are still not ‘comparing like with like’ when contrasting the results for the first-generation immigrants with those for the natives. This may be, for instance, because, although we are controlling for age or employment status, there may be unobserved variables correlated with these, which may bias results if natives and immigrants are systematically different along these lines. To overcome the extrapolation problem of linear regressions, we examine the robustness of our results when explicitly enforcing common support on all of the control variables discussed above, which were applicable to natives as well.²¹ Relying on this restricted ‘comparable’ subset, we obtain very similar results in terms of signs and significance, thus increasing our confidence in our findings.

Although our dependent variable, life satisfaction, is an ordered categorical variable with response categories ranging from 0 to 10, the above pooled OLS and fixed effects models treated it as continuous and assumed linearity. Unfortunately, as noted earlier, there is no straightforward generalization of the simple logit with fixed effects framework to ordinal variables. Suggested solutions include the Chamberlain estimator, which collapses the outcome to a binary variable and picks a single cut-off point; the Das and van Soest (1999) two-step estimator, which estimates the model for all cut-offs and combines the estimates in a second step; Baetschmann et al.’s (2011) blow-up and cluster estimator, which creates a dataset where each individual is repeated $K - 1$ times (where the dependent variable is coded $1 \dots K$) each time using a different cut-off to collapse the dependent variable; and Ferrer-i-Carbonell and Frijters’ (2004) estimator where an optimal cut-off is defined for each individual, but this is in general inconsistent (Baetschmann, Staub and Winkelmann 2011). In a simulation experiment, Baetschmann et al. (2011) found that the Das and van Soest and Baetschmann estimators generally perform well; Dickerson et al. (2011) found that the difference between the estimators is fairly minor. However, as there is no clear best choice among the above estimators and relying on these would raise difficulties concerning the clustering of standard errors, we chose the above linear models as our preferred specification. Comparing the pooled OLS and fixed effects results obtained above with a simple ordered logit without fixed effects, results are very similar in terms of signs and significance of coefficients.

We also examine the robustness of our results by looking at different subsamples, repeating the above analysis for Turkish and ex-Yugoslav immigrants (the two largest immigrant groups in Germany), employed/unemployed, different education levels and men and women separately. Our results for all subsamples are very much in line with those above. As expected, the citizenship results are driven by the Turkish and ex-Yugoslav groups; very few of the guest workers from Greece,

Italy or Spain in our dataset have German citizenship (please see Table 11 in the Appendix).

6 Conclusions

This paper aimed to complement existing studies of migrant integration by looking at the subjective well-being of immigrants once settled in the host country. In particular, we examined two questions: (1) whether immigrants are less satisfied than natives (as suggested by the existing literature) and (2) what determines the life satisfaction of immigrants. Looking at the first question, we found that while in terms of raw means immigrants seem less satisfied than natives, this difference can be accounted for entirely in terms of observable characteristics, in particular details of employment conditions such as whether working in the occupation trained for or the degree of job insecurity. Furthermore, similar factors seem to affect the life satisfaction of immigrants and natives: while immigrants are hurt by adverse shocks such as becoming unemployed, they are not hurt more than the native population.

Turning to the second question, we tried to examine ‘who does better and who does worse’ within the sample of immigrants, looking at socio-economic as well as cultural factors. We found no evidence supporting the concern that feelings of not belonging or loyalties towards immigrants’ countries of origin may have negative effects on their life satisfaction. Relying on a unique dataset including measures of the ethnic composition of neighbourhoods at various levels of disaggregation, we showed that segregation *per se* does not affect life satisfaction. Although having German citizenship appeared to have a large and highly significant positive effect in the pooled OLS specification, looking at the specification including individual fixed effects as well as examining the effects of changes in the German citizenship law, we believe that this is a selection effect rather than an increase in life satisfaction due to obtaining citizenship.

Overall, we believe that our findings are very encouraging in that although in the short run migration involves ‘acculturative stress’, in the long run (in contrast to the findings of the earlier literature), once immigrants have settled, they are not less satisfied on average than the native population. Furthermore, the effect of ‘integration’ on life satisfaction comes primarily through economic factors—elements that policymakers can influence—rather than ‘cultural’ ones, with details of employment conditions playing a particularly important role.

Endnotes

¹The words subjective well-being and life satisfaction are used here interchangeably, as measured using the standard life satisfaction question: ‘How satisfied are you with your life, all things considered?’

²See Park’s ‘marginal man’ (1928) or Handlin’s ‘uprooted’ and ‘children of the uprooted’ (Handlin 1951, 1966). Concepts like ‘acculturative stress’ have been used to designate this immigration ‘psychopathology’ (Berry et al. 1987; Berry 2001), highlighting migrants’ cultural uprooting and their ‘establishing themselves’ again in the host society (Bourdieu and Sayad 1964; Sayad 1999; see also, e.g. Li 2015).

³Amit and Bar-Lev (2015) proposed a model in which life satisfaction is a major predictor of immigrants' sense of belonging and found that, as expected, life satisfaction had a significant influence on immigrants' sense of belonging.

⁴The 'trait-state-error' model was originally proposed by Kenny and Zautra (1995). It has been later used by, e.g. Ehrhardt et al. (2000), Schimmack and Lucas (2007), Lucas and Donnellan (2007) and Schimmack et al. (2010) to look at well-being.

⁵Ideally, given the discrete nature of our dependent variable, an ordered logit or probit model should be used. However, this raises the issue of how to incorporate individual fixed effects in an ordered logit model; the choice between suggested alternative estimators is not straightforward and raises further difficulties relating to the clustering of standard errors in panel data. Results in the following are thus based on pooled OLS with fixed effects; alternative specifications are examined as a robustness check and are discussed in Section 5.5.

⁶Data were extracted using PanelWhiz, a Stata add-on written by John P. Haisken-DeNew (Haisken-DeNew and Hahn 2010).

⁷Due to privacy regulations, this is only accessible on site at DIW Berlin.

⁸The question also maps underlying thoughts and emotions well, i.e. shows patterns consistent to those which look at answers to questions such as 'Overall, to what extent do you feel the things you do in your life are worthwhile?' Positive and negative affect scales showed high independent correlations with a global well-being item (Beiser 1974; Bradburn 1969; Moriwaki 1974). The scales also correlate as expected with happiness ratings made about respondents by others and with other non-self-report data, e.g. Weinstein (1982) found that self-reported happiness was strongly related to an unobtrusive measure of smiling and laughing in an interview.

⁹Interactions between the first-generation immigrant dummy and employment or education were not significant either, suggesting that while these factors influence life satisfaction, they do not impact immigrants differently.

¹⁰In fact, extending the baseline specification of columns 1 and 2 by only adding this variable already makes the first-generation dummy variable lose its significance.

¹¹The question on job insecurity was only asked for currently employed respondents—to avoid such a restriction in the sample, we used a measure of job insecurity of the last job the respondent had. Results are very similar if we only use the currently employed sample, who were actually asked about the insecurity of their current job. Results are also robust to using dummy variables instead of the one-to-three scale.

¹²Discrimination is not included in the above preferred specification as we are worried that such subjective perceptions may be affected by reverse causality from general life satisfaction. However, as the existing literature suggested discrimination as a possible explanation for the relative dissatisfaction of immigrants, we examined the robustness of our results to its inclusion. Its effect is negative and highly significant as expected, while results on other variables are very similar to those discussed above. In particular, job insecurity and working in the occupation trained for retain their significance. Years since migration has a negative effect on life satisfaction (in line with the findings of Obućina 2013), though in most specifications this effect is not significant.

¹³For an in-depth study of the determinants of naturalization of non-EU immigrants in Germany, see Zimmermann et al. (2009).

¹⁴For the impact of ethnic spatial dispersion on immigrant identity, see Constant et al. (2013).

¹⁵Unfortunately as few individuals show up more than once in these subsamples, it should be noted that even the fixed effects estimator cannot be seen as providing completely 'clean' measures, parsing out the effect of time-invariant characteristics.

¹⁶In terms of raw means, as expected, immigrants live in neighborhoods with a higher proportion of foreigners (6.7 versus 4.4 on the one-to-nine scale, 10 % versus 5 % using the percentage of immigrants measure; please see Table 10). Those living in more immigrant neighborhoods are younger, less educated and are much less likely to own the house/apartment (these differences are significant at the 5 % level). However, they are not more likely to be unemployed; differences in terms of job insecurity and working in the occupation trained for are not significant either at conventional levels. Looking at the ethnic composition of neighborhoods over time, both immigrants and natives seem to be living in less immigrant neighborhoods, suggesting decreasing segregation (data was available for the years 2000–2010).

¹⁷See, e.g. Edin et al. (2003), who relied on a natural experiment to identify the effect of ethnic enclaves on the labour market performance of immigrants in Sweden; a similar natural experiment in Denmark was exploited by Damm (2009).

¹⁸The law also introduced a transitional provision for the children of foreign residents under the age of 10 on the 1st of January 2000. These children would be naturalized upon application (to be completed before the 31st of December 2000) if at least one parent had been ordinarily a resident in Germany for 8 years at the time of the child's birth. In order to avoid potential problems of endogeneity related to the child-bearing decisions of immigrants, and variations over time in the composition of immigrant inflows, we focus on the retrospective component of the 2000 reform.

¹⁹This approach is in line with the work of Avitabile et al. (2010), who examined the effects of the change in the citizenship law on parental integration. We depart from their approach by using a narrower control group (they also used those parents with children born between 1980 and 1989) as we believe that this makes the treatment and control groups more comparable. Our results are robust to using either control group.

²⁰Dual citizenship was officially not allowed for adults; however, this was subject to a number of exceptions, such as if renouncing citizenship would incur excessive costs (as in the case of ex-Yugoslavs) or would bring serious economic disadvantages or problems with property and assets (as in the case of Turkish citizens). Immigrants could also renounce their Turkish citizenship upon acquiring German citizenship but immediately thereafter to reacquire their Turkish passports. Although unfortunately the data on dual citizenship in the GSOEP is not good enough to see what percentage of immigrants in our sample had not renounced their original citizenship when obtaining German citizenship, anecdotal evidence would suggest that it is unlikely that this was a strongly binding constraint.

²¹This could, for instance, overcome bias arising from the fact that we are looking at a specific subsample of immigrants who, in terms of age, are at the minimum of their 'well-being curves'.

Appendix

Table 7 Control variables

Variable	Explanation
Age	Individual well-being tends to decrease with age though the relationship is often argued to be U-shaped rather than linear, with a minimum around age 50 (though this varies somewhat across countries, Bruni and Porta 2005).
Gender	Women tend to report higher life satisfaction than men though the difference is often small, and women's day-to-day emotions tend to fluctuate a lot more than men's.
Marital status, children	People with a partner report, on average, higher satisfaction scores than those without. Children have also been shown to increase well-being. (Given our focus on first-generation immigrants, who got married and had children many years ago, we would not expect these variables to be affected by current life satisfaction.)
Education	The existing evidence seems somewhat ambiguous on the relationship between subjective well-being and education (Powdthavee 2010): while most studies find that people with more years of education report higher satisfaction scores than those with fewer years of education (e.g. Helliwell 2003; Bruni and Porta 2005; Stutzer 2004; Graham and Pettinato 2002), there is some evidence that people who have completed at least a university degree report lower levels of job satisfaction and higher levels of mental distress compared to those from a lower educational background (Clark 2003), holding health and income constant. These findings may be explained by the fact that in addition to increasing income, education may also raise aspirations, resulting in a potentially ambiguous overall effect. As we are looking at adults who have completed their education a long time ago, this variable should not be affected by current life satisfaction.
Employment	Empirical findings stress the harm done by unemployment, affecting income as well as status/social expectations. Having a job includes many aspects that provide flow experiences and satisfy intrinsic needs, like being in the company of workmates, applying expertise and experiencing autonomy. Accordingly, being unemployed is repeatedly found to have large negative effects on people's subjective well-being, with little habituation. We also include a measure of whether the respondent works in the occupation they were trained for and a variable for the degree of job (in)security, which may be of particular relevance to immigrants. To deal with possible reverse causality from life satisfaction to employment, we examine the robustness of our results to using lagged values as proxies.
Owns house/apartment	The effect of income on subjective well-being has been shown to be positive but non-linear, both at the micro and at the macro level (Easterlin 1974, 2001). Stutzer (2004) found that the positive effect of higher income can be offset by rising income aspirations. As we are worried that income may be endogenous (and may be measured with error), we do not include it in our preferred specification and use whether the respondent owns a house/apartment as a proxy. (Results are very similar if we use income instead.)
Parental characteristics	We control for parents' education in levels and include dummy variables for whether the respondent grew up in a large/medium/small city or in the countryside.
Health	It is widely accepted that an adverse change in health reduces life satisfaction. Furthermore, the literature on mental and physical health reports great inequalities in this field among ethnic groups (Vega and Rumbaut 1991; Rumbaut 1994).
Years since migration	If the number of years in the destination country improves the economic position of immigrants, we would expect this to also increase their life satisfaction. However, this variable may also capture expectations, regret or comparisons with the home country. This may be of particular interest in our sample of guest workers, who originally arrived as temporary migrants.
Identity	To avoid the endogeneity of identity (if respondents are unhappy in Germany, they may be less likely to feel German), we use lagged identity measures as a proxy. We believe this may be reasonable given considerable variation in subjective well-being over time as responses are influenced by the mood of the day as well as random events. We use identity from 2003 as a proxy for identity in 2010 (unfortunately, the identity question was only asked in these years); life satisfaction in 2003 predicts only around 16 % of the variation in satisfaction in

Table 7 Control variables (*Continued*)

	2010, whereas the identity variables (especially majority identity) are more correlated over time.
German language skills	Language plays a central role in the integration of immigrants in the new labour market (Chiswick 1998; 2002) but is also important for social contacts with the host population. Given our focus on first-generation immigrants who arrived over 30 years ago, we believe that current life satisfaction should not affect German language skills.
Discrimination	Although discrimination is often put forward as a possible explanation for the lower life satisfaction of ethnic or racial minorities, general life satisfaction is likely to affect subjective perceptions of discrimination making the identification of a causal effect difficult. As instruments are hard to find, we do not include discrimination in our preferred specification but examine results both with and without this variable.
Ethnic composition of neighbourhood	Residential location is often portrayed as a key element of immigrant integration. In a study among adolescents from immigrant families, those living in ethnically homogeneous neighbourhoods reported a higher level of satisfaction with their lives than those living in heterogeneous neighbourhoods (Neto 2001), contradicting the assumption that immigrants who are in social contact with local natives and live in heterogeneous neighbourhoods should be more socially integrated and thus more satisfied. To avoid the endogeneity of the location decision, we examine robustness by restricting the sample to those who have not moved recently. We also examine results separately for those who want to/ do not want to move.
German citizenship	Having German citizenship may affect the respondent's economic opportunities as well as subjective perceptions of security or uncertainty and may also carry a more 'symbolic' value on the perception of immigrants in the host country. Variation due to changes in the German citizenship law is also explored.

Table 8 Examining robustness to the fall in the number of observations

	POLS	FE
Age	-0.007* (0.004)	-0.029** (0.011)
Male	0.141 (0.074)	(omitted)
Married	0.033 (0.103)	0.096 (0.161)
Separated/divorced	-0.567** (0.187)	0.041 (0.267)
Widowed	-0.932*** (0.353)	-0.907 (0.509)
Years of education	0.023 (0.016)	0.053 (0.142)
Employed (lagged)	0.242* (0.106)	0.085 (0.109)
Immigrant	-0.247* (0.113)	(omitted)
<i>Number of obs.</i>	29,835	29,835
<i>Number of ind.</i>	3929	3929

Note: Standard errors in parentheses (clustered at the individual level). * denotes significance at 5 %, ** at 1 % and *** at 0.1 %. Other control variables are as in columns 1 and 2 of Table 3. 'POLS' refers to a pooled ordinary least squares specification, 'FE' to fixed effects

Table 9 Do shocks hurt immigrants more?

	POLS		POLS	
Lost job	-0.551***		Less secure	-0.309***
	(0.068)			(0.050)
Lost job * immigrant	-0.084		Less secure * immigrant	0.064
	(0.079)			(0.055)
Number of obs.	71,775		Number of obs.	37,637
Number of ind.	8256		Number of ind.	5796

Note: Standard errors in parentheses (clustered at the individual level). * denotes significance at 5 %, ** at 1 % and *** at 0.1 %. 'POLS' refers to a pooled ordinary least squares specification

Table 10 Ethnic composition of neighbourhoods

		Natives		Immigrants	
		Mean	Std. dev.	Mean	Std. dev.
Fraction of foreigners (1–9 scale)		4.441	2.472	6.776	2.349
Fraction of foreigners (percentage)		5.201	4.572	9.763	7.623
By ethnic origin	Balkans	0.637	0.866	1.263	1.304
	Greece	0.404	0.491	0.672	0.717
	Italy	0.702	0.747	1.171	1.166
	Spain-Portugal	0.185	0.251	0.29	0.348
	Turkey	1.492	2.615	3.876	5.415

Table 11 Results by country of origin

	Turkish, ex-Yugoslav		Spanish, Greek, Italian	
	POLS	FE	POLS	FE
Years since migration	-0.050***	(omitted)	-0.011	(omitted)
	(0.018)		(0.013)	
Min. identity (lagged)	0.034	0.066	-0.021	0.017
	(0.032)	(0.042)	(0.029)	(0.035)
Maj. identity (lagged)	0.060	0.026	0.010	0.034
	(0.051)	(0.059)	(0.058)	(0.064)
German citizenship	3.337***	(omitted)	(omitted)	(omitted)
	(0.670)			
Speaks German	0.261***	0.162	0.112	0.008
	(0.092)	(0.107)	(0.084)	(0.092)
Writes German	-0.044	0.01	0.045	-0.037
	(0.076)	(0.093)	(0.067)	(0.077)
Number of obs.	1432	1432	1405	1405
Number of ind.	602	602	568	568

Note: Standard errors in parentheses (clustered at the individual level). * denotes significance at 5 %, ** at 1 % and *** at 0.1 %. Other control variables are as in Table 4. 'POLS' refers to a pooled ordinary least squares specification, 'FE' to fixed effects.

Competing interests

The IZA Journal of Migration is committed to the IZA Guiding Principles of Research Integrity. The author declares that he/she has observed these principles.

Acknowledgements

I am very grateful to the editor, an anonymous reviewer, Toke Aidt, Paul Baker, Pramila Krishnan, Hamish Low, John Micklewright and Alexander Plekhanov for very helpful discussions and comments on earlier versions of the paper. I also wish to thank Jan Goebel for his kind help at DIW Berlin.
Responsible editor: Amelie Constant

Received: 4 November 2015 Accepted: 14 January 2016

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