

Assimilation without groups?

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ABSTRACT

In "Origins and Destinations", Luthra, Soehl and Waldinger aim at "extending the canon" in research on immigrant integration. They do so by studying group and individual level characteristics across a large number of origin groups, thereby replacing group labels with theoretically relevant variables at the group level. In this review, three challenges in this endeavour are discussed: First of all, the analyses do not allow to disentangle the impact of group and individual level characteristics for all variables alike. Secondly, a primary data collection more tailored to the specifics of their conceptual framework is needed. This would many conclusions about how contexts of emigration and immigration truly matter above and beyond individual characteristic on a more solid ground. And third, the authors' aim to abandon the use of origin group categories in empirical research on immigrant integration may come at a price.

KEYWORDS Immigration; integration; assimilation; culture; reception context; USA

Renee Luthra, Thomas Soehl and Roger Waldinger's "Origins and Destinations: The Making of the Second Generation" is an important contribution to the ongoing debate about how second generation assimilation is best understood and analyzed. The book is ambitious in its critique of existing studies (though less so in their final assessment, see below), in its detailed sketch of a conceptual framework for analyzing second generation assimilation, and in its empirical test. The analyses presented differ from many others on this well-studied topic, above all because the authors barely refer to specific origin groups at all. It is the authors' self-declared goal to not present another study in the sense of an "ethnoracial Olympics", i.e. a study that identifies groups that integrate at a faster pace than others and groups that lag behind.

A great advantage of "Origins and Destinations" is that it truly builds upon existing research. In chapter 2, the authors provide a broad and concise overview of existing seminal studies of assimilation. These include *Legacies*, *Inheriting the City*, *Parents without Papers*, *The Asian American Achievement*

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Paradox and Remaking the American Mainstream – all studies that aim to explain group specific integration trajectories and that draw either from Assimilation Theory or from the Theory of Segmented Assimilation. The authors identify two major weaknesses in the studies: First, they claim that homeland culture is not openly addressed in either of these studies but “creeps in through the back door”. By this they mean that “unexplained” group specific patterns of assimilation are attributed in an ad hoc manner to cultural aspects such as a group’s unusually strong family orientation and cohesion. Secondly, they argue that the effects of migration control systems and group specific differences in access to citizenship are not taken into account in a systematic way, and if so only in those studies that are limited to a few migrant groups. Luthra, Soehl and Waldinger aim at “[e]xtending the canon” by studying group and individual level characteristics across a sufficiently large number of origin groups. This enables them to replace origin group labels with theoretically relevant variables at the group level, a step they claim also has practical advantages given the growing diversity of immigrant inflows.

This way they aim to replace the concept of “reception contexts” as prominently put forward within the framework of the Theory of Segmented Assimilation. In this account, group differences reflect, above all, group specific conditions of incorporation that render assimilation impossible for some groups and link it to negative outcomes for others. Luthra, Soehl and Waldinger’s critique crystallizes around this concept because

the proponents of segmented assimilation have been caught in a circular loop, contending that knowledge of each nationality’s mode of incorporation provides the basis for predicting nationality effects, but then using the coefficients from dummy variables for specific nationalities as evidence of the importance of mode of incorporation.

In this respect “Origins and Destinations” takes up the critique by Waldinger and Catron (2016) who have already demonstrated that empirical evidence for the pivotal role of reception contexts is at best limited. At the same time, the authors of “Origins and Destinations” refute culturalist interpretations of group residuals even though they resonate well with public debates on the long lasting impact of homeland cultures.

Accordingly, they differentiate between group level characteristics that relate to the “context of immigration” and those that relate to the “context of emigration”. The former include a group’s location in the host country’s system of skin colour stratification, its “legal status prevalence” measured by the share of undocumented migrants and refugees, and the ethnic capital available to its members, measured by groups’ mean level of education. Group level variables referring to the “context of emigration” include value orientations in the sending countries such as “traditional versus

secular-rational values" and "survival versus self-expression values". The big advantage of this approach is that it provides insight into which line of differentiation *between* groups, the legal, cultural, phenotypical or resource dimension is more important. In their multilevel analysis, the authors can then replace ethnic origin, "a nominal variable lacking in rank order" and instead "unpack theoretically relevant characteristics [...] and turn them into measurable variables, the consequences of which can then be assessed" (p. 11). While this strategy is convincing, two practical challenges arise.

First of all, the problem behind nationality is not so much that it is "a name, not a variable" but that the mechanism by which it impacts integration are so complex that origin effects can at best be interpreted in an ad hoc way. For sure, the authors do get closer to the mechanisms at work behind this general variable by dismantling it into more specific ones: average levels of education indicate resources that can be mobilized via ethnic networks such as referral networks and group reputation. Similarly, members of lighter skinned groups may be less susceptible to discrimination, no matter how dark skinned they are individually. Group level legal status ("legal status prevalence") is assumed to affect integration via the stigma attached to groups with, for example, large shares of undocumented. But while the authors convincingly spell out which mechanisms might be at work behind average levels of education, this remains more opaque for other variables, especially those related to the context of emigration. The arguments why coming from a more survival oriented or a more secular group might be beneficial for education above and beyond individual value orientations remains less conclusive.

This leads to the second challenge, the fact that the empirical test of the analytic strategy in "Origins and Destinations" is limited by data availability. Unlike individual level of education and legal status, neither parental value orientations nor individual phenotypical differences are included in the dataset. In this respect, group level value orientations or phenotypical group characteristics are more of a proxy for individual characteristics. Thus, the analyses do not allow to disentangle the impact of group and individual level characteristics for all variables alike. It remains an open question whether the former would still have an effect on integration after controlling for the latter.

Most of these limitations are due to the fact that the authors analyze secondary data in order to test their novel framework. The next step would thus be to conduct a new survey of second generation youth and to capture parental attitudes or individual skin colour as individual level variables. This would put many conclusions about how contexts of emigration and immigration matter on a more solid ground. The same applies to intraethnic contacts that would allow them to measure differences in actual access to group

resources more directly. This way, three questions could be tackled: First, do the results hold after introducing more encompassing individual level controls? Second, to what extent are the findings on the impact group level factors triggered by single origin groups? And finally, which findings are robust across a broader set of integration outcomes?

A dataset more tailored to their conceptual scheme would also allow the authors to be a bit braver with respect to the lessons learned from their study. In the book's final chapter, the empirical results are summed up nicely but the authors seem reluctant in drawing conclusions about the two large approaches behind the theoretical concepts and empirical studies that their endeavour starts out from. After all, they introduced their empirical analysis as a "comprehensive assessment" of the approaches reviewed in Chapter 2. Many of their findings are dramatically at odds with predications of the Theory of Segmented Assimilation. Almost none of the systematic group comparisons on educational and occupational attainment yield the effects suggested by the theoretical framework spelled out in *Legacies*. Furthermore, there is no evidence that the "ethnic toolkit", including speaking the home country language at home, has any positive impact on education. If ethnic resources matter at all, they seem to do so on the group rather than on the individual level: "We generally fail to find evidence indicating that a specifically 'ethnic' toolkit creates second generation success". (p. 267). This is in line with the findings from many European studies that consistently refute the argument that groups benefit from ethnic resources such as ties to coethnics or fluency in the origin country language. Some descriptive findings run against some core assumptions of the Theory of Segmented Assimilation as well. For example, the share of families showing patterns of dissonant acculturation is almost non-existent. Given these findings, I was wondering why the authors don't state more boldly that there is in fact very little empirical evidence backing the ideas of selective acculturation – not to mention downward assimilation – as empirically relevant alternatives to mainstream acculturation.

It makes sense that the authors shy away from siding with one approach or the other given their claim of a new perspective. But in sum, their empirical findings present a lot of support for a reformulated assimilation theory. Most importantly, this means that they show once again support for the decisive role of individual and family level variables. While they claim to find support for group level factors, there is reason to assume that their effect would be even smaller if it weren't for the shortcomings in the data mentioned above. And taking into account the family situation, i.e. transnational family ties, when predicting cross border activities is perfectly in line with assimilation theory. It seems, for example, likely that the strong effect of cultural orientation on homeland visits would be much smaller if more encompassing controls for these ties were taken into account. Other findings that

point to the role of group level factors, such as the share of non-citizens affecting transition to citizenship, seem quite predictable. What is more interesting is that a group's "status prevalence" does not even affect the exercise of citizenship, namely political engagement.

This leads me to a more general question about the consequences of this empirical endeavour for future studies on immigrant integration. In sum, the impact of group level variables such as cultural background, legal status or position within the system of skin colour stratification on integration outcomes is clearly limited. Against this backdrop, abandoning the "ethnoracial Olympics" sounds appealing at first but it may come at a price. There are many advantages of identifying groups, as transitory and volatile as they might be and as important as it is to define and delineate them according to the research question. Analyses of immigrant integration may resonate much more with public debates – and be more likely to eventually translate into policy recommendations and measures – when challenges that specific origin groups face can be identified. As important as the result is that group level education has a positive effect on educational attainment above and beyond parental education, it is harder to communicate this story than to state that the educational advantage of groups x and y as compared to other groups is not merely explained by selective immigration of highly educated migrants. It goes without saying that such a finding must not just be explained on an ad hoc basis but should always kick off tailored research designs aimed at testing conflicting explanations.

In any case, Luthra, Soehl and Waldinger have presented a piece of research that is in many regards exemplary. They start out with a careful discussion of existing studies rather than by attacking a "paper tiger" and they come up with a thoroughly outlined conceptual framework that translates into testable assumptions. And finally, they present complex empirical analyses that add more pieces to the existing empirical puzzle of immigrant integration rather than eagerly finding support for seemingly novel concepts.

Disclosure statement

No potential conflict of interest was reported by the author.