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Place / Date

Brno  
30 September 2023

## **Report on PhD thesis by Ketevani Kapanadze: “Essays in Regional Economics and European Integration”**

The submitted dissertation comprises three empirical papers focused on the effects of disintegration and integration events in European countries. Each of these papers tackles relevant questions using innovative approaches, showcasing the author's commitment to advancing the field. I believe these papers hold considerable promise for publication in esteemed journals within the domains of economic geography and regional economy.

I find that the dissertation successfully meets the formal and content requirements for the awarding of a Ph.D. degree. Consequently, I am pleased to wholeheartedly recommend it for a defense.

In the following sections, I provide feedback for each of the three essays (chapters) included in this collection. My intention is to highlight comments that the author is likely to encounter in quality journals within the aforementioned fields while also suggesting potential avenues for further development of the papers.

### **Chapter 1**

The first essay in this collection focuses on the contribution of European integration to regional development in Central and Eastern European (CEE) countries. The author rightfully highlights a critical issue in the existing literature, which often makes the strong assumption that integration has no impact on the inner regions of integrating countries. The paper's primary contribution lies in its adoption of an identification strategy (SCM) that not only allows for estimating the effects of integration on all regions but also avoids reliance on comparisons between border and inner regions. This represents a significant advancement in the field of regional economics.

In her analysis, the author, as reflected in the paper's title, concludes that inner regions benefited while border regions suffered from the integration process. However, I believe there is an opportunity for the author to delve deeper into the potential underlying mechanisms at play. It might be worthwhile to explore the historical context of the CEE region. For decades, the economies of CEE countries were centrally planned, leading to a concentration of

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resources, such as highly educated individuals in the R&D sector, in specific urban centers located further away from the borders. This historical aspect could help explain why these centers were better positioned to take advantage of the increased market potential resulting from integration. The results of the robustness check, as reported in Table 1.A.2, suggest that large population centers might be playing a significant role in the observed effects.

(a) It's worth noting that regional GDP per capita may suffer from measurement errors that could be country-specific. As an alternative outcome, the author could consider using night-light intensity.

(b) I would recommend expanding the robustness check presented in Table 1.A.2 (p.32) to exclude large population centers, beyond just Prague and Budapest. Additionally, it would be beneficial to include this robustness check in the main body of the paper.

(c) It's important to acknowledge that regressions (1.3) and (1.4) are likely affected, at the very least, by spatial autocorrelation in the error term. To address this, the author could consider presenting results with Conley (1999) standard errors.

(d) In preparing the version for journal submission, attention should be given to improving the technical details of the paper. For instance, the Appendix Figures should be made more legible for readers.

## **Chapter 2**

The second essay explores a truly captivating idea—examining the impact of integration on 22 European cities that were previously divided by national borders. The author employs a straightforward empirical framework to identify the effects of integration on the spatial distribution of economic activity, measured through remotely sensed night-light intensity and the number of established companies. The author's conclusion that integration shifted spatial equilibria closer to historic city centers is both intriguing and commendable, making this essay the standout contribution within the thesis. However, I believe there are opportunities to further enhance the analysis of effect heterogeneity.

(a) Integration undoubtedly brought additional market potential to divided cities, and this potential likely varied significantly across countries in the estimation sample. It would be intriguing to explore the heterogeneity of treatment effects based on wage differentials.

(b) Some divided cities have their origins in violent historic events that included the expulsion of the original population and the resettlement of cities, resulting in the reshaping of social and economic structures in at least one half of the affected divided city. Such events can have lasting effects, including impacts on education structures (see e.g., Becker et al. 2020, Testa 2021, or Guzi et al. 2021). It would be worthwhile to consider how these historical events may shape the impact of integration.

(c) Building on the previous point, ethnic cleansing in certain cities and the subsequent elimination of cross-border interactions could have reduced the population's ability to speak the language of the other part of the city. In this context, using language similarity indices might be misleading. Exploring heterogeneity based on the size of diaspora communities could provide additional insights.

(d) The paper employs two measures of economic activity. It would be beneficial to discuss their empirical relationship and consider the possibility that the place of registration may not necessarily coincide with the actual location of economic activity. Comparing registry data on companies with alternative data on business locations, such as data from maps or telephone directories, could be enlightening.

(e) Unfortunately, the source of data for tracking Czech companies from 1900 to 2020 is not identified in Appendix B. Clarifying this data source would enhance the paper's transparency.

(f) It's unclear whether the data on companies includes self-employed individuals. If it does, the place of registration is likely to coincide with the place of residence. A robustness check excluding self-employed individuals would be advisable.

(g) Similar to the first essay, addressing the issue of spatial autocorrelation would be valuable in enhancing the robustness of the analysis.

### **Chapter 3**

The third essay addresses a highly relevant and crucial question: the impact of COVID-related border closures on economic activity. Much like the second essay, the author employs remotely sensed night-light intensity (NTL) to gain granular insights into economic activity.

(a) In my view, the paper may be moving too swiftly from findings to conclusions. It would be beneficial for the author to allocate more space to the connection between border closures and NTL. Understanding how a decline in services could result in a decrease in night-light intensity at 1:30 a.m. is challenging, and it remains somewhat mysterious how NTL could significantly increase in regions where cross-border visits with friends and family are common. In the latter case, the author argues for zero effects, but the results are positive and significant. It might be worthwhile for the author to focus on pixels with substantial pre- and post-treatment differentials in NTL. Leveraging data sources like Open Street Map could help identify the businesses driving the treatment effect.

(b) If I understand the data correctly, VIIRS NTL data is available on a daily basis. Investigating potential differences in weekly cycles could shed light on the mechanisms behind the treatment effect.

(c) Additionally, the author could consider re-estimating her regressions using the data on divided cities, where she has detailed information on economic structure.

(d) The extensive discussion of the differences between DMSP-OLS and VIIRS data may not be necessary and could be moved to the Appendix for brevity.

(e) The definition of small and large cities based on population density might be misleading. Furthermore, municipalities located near the country border could inherently have lower population density.

(f) The rationale for classifying municipalities by the dominant economic sector is not entirely clear. It may be more intuitive to use employment shares of individual sectors. Moreover, census data should provide the author with the means to construct these measures at the municipal level.

(g) The adoption of border closures essentially constituted a treatment with staggered rollout. Yet, the paper does not seem to reflect the latest developments in DiD literature. Additionally, considering potential spatial autocorrelation could enhance the robustness of the analysis.

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