The Geography of Path Dependence
Allen and Donaldson (2020, NBER WP)
Referee Report

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The Geography of Path Dependence is an important paper, as it introduces a tractable dynamic economic geography model, an element so far missing in the economic literature of path dependence. It creates a framework to study the dynamics of economic geography and path dependence in a more systematic (theoretical) way. The authors derive analytical conditions under which an economy with an arbitrary number of locations may converge to different steady states depending on its initial conditions. They then estimate the model and run counterfactual simulations to study how shocks on fundamentals affect long term geographical distribution of population and welfare. Their results suggest an ample role for path dependence in both long run geographical distribution of economic activity, and aggregate welfare levels. The theoretical framework appears to be flexible enough to have the potential to accommodate multiple types of shocks, even though it does not account for any purely spatial linkage.

I see the theoretical model as the main contribution of this paper. In the vein of most economic geography models (see the canonical Fujita, Krugman and Venable, 1999), the model generates four new theoretical results by adding a couple of distinctive features. First, following macroeconomics' historical choice to study the convergence hypothesis, an overlapping generation structure (OLG) replaces the infinitely lived agents, which improves the tractability of the model's dynamics. Second, the main theoretical innovation is to model both contemporaneous and historical productivity and amenity spillovers. More precisely, productivity levels A_{it} (utility u_{it}) are determined both by fundamentals $\bar{A}_{it}$ ($\bar{u}_{it}$) and contemporaneous and historical populations. α_1 and α_2 (β_1 and β_2) determine the strength of contemporaneous and historical net agglomeration forces on productivity (amenities). The authors do not take a stand on the microeconomic foundations of such spillovers, allowing their model to accommodate multiple microeconomic theories. This complementarity between contemporaneous and historical spillovers directs the model's dynamics through optimizing agents' migration decisions. As is standard in the literature, a static equilibrium

exists and is unique if contemporaneous agglomeration forces $\alpha_1 + \beta_1$ are small enough. The paper innovates as the model's dynamic equilibrium(a) and steady-state(s) are governed by the net sum of contemporaneous and historical spillovers. For large enough $\alpha_1 + \alpha_2$ and $\beta_1 + \beta_2$, shocks on fundamentals may lead to different steady states. The authors derive analytical bounds on welfare associated with each possible steady state. Thereby, they investigate the quantitative role of path dependence, and propose a first quantitative answer to Rauch 1993's famous question: "*does history matter only when it matters little?*".

However, as many pioneering theoretical works, it still lacks in empirical robustness. In particular, I believe the empirical estimation, and numerical simulation sections can be significantly improved. First, the authors should acknowledge that the assumption of constant elasticity of distance on trade flows κ (equivalent to constant trade costs) is unrealistic, and investigate potential workarounds. Second, the instrumental variables (IV) strategy should be strengthened. Third, the novelty of the model together with estimated parameters being so close to the boundary conditions, call for the presentation of simulation results for a larger range of parameters for robustness. Fourth, the paper's treatment of productivity and amenity fundamentals needs to be improved.

First, I believe that the assumption of fixed κ, the elasticity of trade flows with respect to distance, casts doubts on the validity of the estimates from Section 3 (and subsequently Section 4). The authors argue that a constant κ is "*broadly consistent with the patterns in international trade data surveyed by Disdier and Head (2008)*". However, Disdier and Head (2008)'s meta-study is about international, rather than intranational flows, and does not present evidence that this elasticity ($\hat{\theta}$ in the paper) is constant. Figure 3 indeed features a net increase in estimated distance effect post 1960 (from 0.6 to 1.1 is almost a 100% increase). Using 1997 data is therefore likely to overestimate κ for earlier years. This is consistent with $\hat{\kappa} = -1.35$ being at the upper end (in absolute value) of other studies' estimates cited in the paper.
Alternatively, could a workaround be found in looking directly at iceberg trade costs τ_{ij}, especially if the authors are to take σ and θ as given as they currently do (see further discussion below)? From equation (6) to equation (25) they recover the following relationship: $\kappa \ln dist_{ij} = (1 - \sigma) \ln \tau_{ij}$, where τ_{ij} are the bilateral iceberg trade costs. Given σ, I wonder if estimating κ is even necessary. If it is, would data on τ_{ijt} allow to recover time-varying κ_t? A potential fallback solution could be to exploit changes in the price indices of different cities / regions over. The paper published by one of the authors on the Interstate Highway System (IHS) (Allen and Arkolakis, 2014) presents evidence that the IHS created changes in price indices across cities. This is both evidence that a constant κ is misleading, and potential direction for relaxing this assumption.
At the very least, the authors could discuss the bias potentially introduced by a constant

$\hat{\kappa}$ estimated using 1997 data. Indeed, $\kappa_{1800} < \hat{\kappa}$ would lead to a downward bias of early $\hat{P}_{it}^{1-\sigma}$ and $\hat{\mathcal{P}}_{it}^{1-\sigma}$ (outward and inward market access). It could also interact with the bias introduced from estimating κ using state-to-state data but working with observations i at the subcounty level. It is unclear from the current version of the paper how large and consequential these biases could be.

Second, the last of the three-step identification relies on an instrumental variables (IV) strategy, as is standard when estimating a simultaneous system of demand and supply equations. While the exclusion restrictions seem to be quite standard in the literature, I believe that changes in technology allowing better adaptation to extreme temperatures in terms of residential amenities could also impact productivity. More importantly, the first stage Sanderson and Windmeijer (2016) F-Statistic of the instrument used to estimate amenity spillovers β_1 and β_2 is 7.4 (Table 3), suggesting potential finite-sample TSLS bias. While I understand that the F-Statistic threshold for multiple endogenous variables is lower than in the canonical case of a single endogenous variable, the justification given in footnote 29 is not sufficient. Indeed, it is mentioned that the estimates from Limited Information Maximum Likelihood are larger in absolute value, but where are these estimates presented? Why does this address the weak IV threat? The authors should also explicitly display the range of Sanderson and Windmeijer (2016) F-Statistics in Table C.1. (after differentiating out the other instruments) rather than simply reporting the aggregate F-Statistic, as it makes it impossible to link the footnote mentioned above to the table.

This leads me to my third point, still related to the identification strategy and the estimation. When estimating the parameters $\{\alpha_1, \alpha_2, \beta_1, \beta_2, \sigma, \theta\}$ the authors choose to exogenously set $\sigma = 9$ and $\theta = 4$ (relying on the literature) in order to precisely estimate $\{\alpha_1, \alpha_2, \beta_1, \beta_2\}$. However, should we expect σ and θ to exactly map estimates from the literature, given that the model is different in a way that previous literature's parameters may have accounted for elements now captured by $\{\alpha_1, \alpha_2, \beta_1, \beta_2\}$? The dismissed σ, going from 0.268 (2.524) to 3.065 (4.986) raises concerns; similarly for $\theta = 0.620$ (0.858) (see Tables 2 and 3). Besides, if the authors end up taking σ and θ as given, could the identification procedure be simplified? For example, could the author recover τ_{ijt} from $dist_{ij}$?
More generally, the estimated $\{\alpha_1, \alpha_2, \beta_1, \beta_2\}$ are so close to the boundary conditions for convergence (Figure 3), that I would like to see how sensitive the simulations in Section 4 are to small changes in the parameters.
Additionally, the estimand for α_2 ranges from 0.040 to 0.045 and is not precisely estimated[1], suggesting that while historical spillovers are important for residential amenities ($\beta_2 = 0.330$ (0.179)), they are not for productivity. Does this imply differentiated persistence for amenity- and productivity-related shocks? While I agree that shocks on productivity fundamentals

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[1]Standard errors of 0.030 and 0.033 respectively.

$\bar{A}_{it}$ are easier to think of than shocks on residential amenity fundamentals $\bar{u}_{it}$, would swapping $\bar{u}_{i,1900}$ in Section 4 lead to very different conclusions because $\alpha_2 << \beta_2$?

Swapping $\bar{A}_{i,1900}$ in Section 4 would have a very different interpretation if $\bar{A}_{i,t} = \bar{A}_i, \forall t$ and could not be interpreted as a one-time historical shock.
More generally, it is unclear whether the simulation exercise actually captures the full extent of a historical shock. I would expect $\bar{A}_{i,t+1}$ to be correlated with $\bar{A}_{i,t}$ (rather than simply $A_{it} = \bar{A}_{it}L_{it}^{\alpha_1}L_{it-1}^{\alpha_2}$ and $A_{it+1} = \bar{A}_{it+1}L_{it+1}^{\alpha_1}L_{it}^{\alpha_2}$ through L_{it}). While this might be indistinguishable in the model, it impacts the interpretation of the simulation exercise. Swapping only $\bar{A}_{i,1900}$ and taking all $\bar{A}_{i,t}, t > 1900$ as estimated in Section 3 could be overestimating (or underestimating) the effect of a one-time shock if $\bar{A}_{i,1900}$ and $\bar{A}_{i,1900+t}$ were correlated or constant. Are estimated $\bar{A}_{i,t}, \forall t > 1,900$ endogenous to the model? Does this affect the theoretical conclusions about a steady state?
Additionally, because of the centrality of such estimates, a map of productivity and amenity fundamentals over time, as in Allen and Arkolakis (2014), would clarify the paper.

I now list a few minor comments and suggestions, some of which apply directly to this paper while others could be simply discussed, as their implementation is well beyond the (acknowledged) scope of this paper. Comments are roughly ordered by decreasing generality and importance.

First, as mentioned in the first paragraph, spatial spillovers of technology and amenity fundamentals across regions i are totally absent in the model (except through migrations entering A_{it} and u_{it}). Incorporating these spatial linkages would help consolidate the bridge between economic geography and urban economics, as these are central to the latter.

Second, one of the key assumptions of the model is that all agglomeration parameters are constant over time and space. This is necessary for tractability, but it is overwhelmingly strong, especially when taken to data spanning multiple centuries.

Third, I believe that the interpretation of the welfare measures deserves a little more discussion. In particular, is the "*up to scale*" identification of $P_{it}^{\sigma-1}, \Pi_{it}^{\theta}, \Delta^{\theta}$ important for welfare? How would welfare dispersion be affected if the model featured firm or worker heterogeneity? Does the ranking of welfare across simulations bring additional evidence to the literature initiated by Henderson (1974) on the optimal size of cities? In particular, do the paper's conclusions support cities being too large, as it would be if larger cities featured lower aggregate welfare? More generally, what should we expect the relative agglomeration and dispersion forces be if we were to introduce such heterogeneity? I understand that heterogeneity goes beyond the paper's scope. However, considering its importance in explaining the strength of agglomeration and spatial sorting of skills and sectors, a discussion on the trade-off between added complexity and accuracy gains could be beneficial.

Fourth, in Section 4, the authors make the claim that term (i) ($\phi_0^T \ln L_{i0}$) of equation (33) should be interpreted as a lower bound on the role of history on (log) population. This implies that the term (iii) ($\frac{1}{\gamma} \sum_{s=1}^T \phi_1^{T-s} \ln(\Lambda_{is}^\sigma P_{is}^{1-2\sigma})$) goes in the same direction as term (i). Can this claim be proved?

Fifth, from proposition 1 to 2, we lose track of the case with unit root ($\rho(\mathbf{A}(\alpha_1, \beta_1)) = 1$). Could this "stationary process" arise? Would the model break down if this was the case? How do we go from $\rho(.) \geq 1$ implying divergence, to $\rho(.) \geq 1$ implying multiple steady states?

In closing, both consequential and minor typos should be corrected. By order of importance: equation (6) should feature $\tau_{ij}^{1-\sigma}$ rather than $\tau_{ij}^{-\theta}$; in equation (32), β_1 should be *"contemporaneous amenity spillovers"* rather than *"contemporaneous productivity spillovers"* ; footnote (29) states that the problematic F-statistic is from Table 2, but it is actually from Table 3 ; in the note on Figure C.2., my reading of the figure suggests that *"[...] with the red indicating a higher population and blue indicating a lower wage"* should be replaced by *"[...] with the red indicating a higher wage and blue indicating a lower wage"*. There is also a typo below proposition 2 (*"the spillover parametersare"*). Finally, should the indices in the welfare equations p.11 be adjusted? Changes between W_{ijt} and W_{jit} are confusing.