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Dear Authors,

Thank you for submitting your manuscript to the *Journal of Financial Economics*. I apologize for the delay in getting back to you. I had an accident that resulted in a fractured hand requiring surgery, and as you can imagine, this slowed me down.

I have now read the paper and considered the referee's report. The referee views the topic as interesting and timely and sees potential for the paper to make a contribution. At the same time, the referee raises a number of substantive concerns regarding interpretation, scope, and clarity that would need to be addressed before the paper could be considered further. In light of these comments and my own reading of the paper, I am willing to offer you the opportunity to revise and resubmit.

Below I list my comments. Some of these echo points raised by the referee:

1. I am still a bit unsure how you construct the branch-level deposit beta. If I understand correctly, you first estimate equation (2), where you use bank-level deposit betas and regress them on local characteristics. The local characteristics are constructed by averaging characteristics of the markets in which a given bank is operating. Then, in the next step, you take those characteristics and predict branch-level betas for each branch. Now, if I understand correctly, you are not using any bank-level information in the second step. So that means if two banks have different bank-level betas but operate in the same locations, they will have the same branch deposit betas. Later, you include bank or bank-time fixed effects, thereby netting out the average bank effect. I think that this can work, but I do think you should explain much more explicitly what you are doing. You should show the equation that defines how you construct the branch-level betas, and you should explain clearly that they do not contain any bank-level information beyond what is captured through local characteristics. You should also explain how we should think about these betas in the context of your empirical design. If I have misunderstood what you are doing, please clarify precisely how the branch-level betas are constructed and how they should be interpreted.

2. You assert that entrants choose high-beta areas because it is hard to move low-beta customers across banks. I certainly agree that it is harder to move low-beta depositors relative to high-beta depositors. However, low-beta deposits are also more valuable than high-beta deposits. Hence, at least theoretically, I do not see that there is a clear prediction on where banks should enter with new branches. Please clarify how you think about this tradeoff and what the underlying intuition or framework implies for entry decisions.

3. The referee suggests that the paper would benefit from drawing out the broader implications of branch closures. In particular, it would be helpful to examine how closures affect local market structure and competition, whether they generate “banking deserts” in which households face limited access to physical bank branches, and how nearby branches respond. The referee also notes that exploring downstream outcomes would strengthen the contribution. For example, it is unclear a priori whether small-business lending should decline following branch closures. Such an effect may arise if physical branches play an important role in screening and monitoring small firms. Even evidence showing that certain outcomes are not affected would be informative, as it would help clarify which economic margins branch closures do and do not operate through.

4. I agree that there is significant variation in betas across deposit types. I do not think that this fundamentally changes your main point, but it is important to account for it explicitly and to be clear about how composition affects your interpretation.

5. You cite Begenau and Stafford (2025) in connection with uniform pricing. I should note, however, that this paper bundles together a number of additional claims that I view as problematic. More importantly, the distinction between uniform pricing—specifically, rate setters versus non–rate setters—was already discussed in the original work of Drechsler et al. (2017). In addition, there is an earlier and growing literature that studies uniform pricing in banking. In particular, Dlugosz et al. (Management Science, 2023) use uniform pricing for identification, and Granja and Paixao (JFE, forthcoming) examine uniform pricing in the context of bank mergers. At a minimum, these papers should be cited to accurately reflect the literature.

6. Finally, the deposit franchise valuation formula you attribute to Drechsler et al. (2023) has since been updated in Drechsler et al. (JF, forthcoming). You should incorporate this in your discussion.

I hope these comments are helpful as you revise the paper. This is not a guarantee that the paper will be published in the JFE. I will need full support from the referee to proceed. Please keep this in mind when deciding whether to resubmit the paper.

Sincerely,

Philipp Schnabl
Co-Editor, *Journal of Financial Economics*