

UNITED STATES DISTRICT COURT  
SOUTHERN DISTRICT OF NEW YORK

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SHAUNA NOEL and EMMANUELLA SENAT,

Plaintiffs,

-against-

15-CV-5236 (LTS) (KHP)

CITY OF NEW YORK,

Defendant.

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**Declaration of Professor Andrew A. Beveridge  
in Reply to Defendant's Feb 2021 Submissions**

March 8, 2021

ANDREW A. BEVERIDGE declares, pursuant to 28 U.S.C. § 1746, that the following is true and correct:

1. I write this declaration in reply to defendant's February 2021 submissions. It would be, I think, overly burdensome on the Court were this declaration, part of plaintiffs' sur-reply, to rehearse each and all of the arguments that I made in my previous two declarations. Accordingly, I am deliberately trying here to be judicious in making a limited number of points; the fact that I have not necessarily responded to an argument presented in Dr. Siskin's most recent declaration<sup>1</sup> means simply that I am relying on my previous submissions to speak for themselves.<sup>2</sup>

2. I note as a preliminary matter that for all critiques that Dr. Siskin has made of my approach to the data, when the Court asks the question "do these critiques actually change the fundamental picture of what defendant's policy does?" the answer is a resounding "no." As shown in the analyses presented in my last declaration regarding Dr. Siskin's flawed approaches, those approaches of his actually confirm both the existence of disparate impacts and the stymieing of the integrative moves made and sought to be made by outsider applicants.

### Disparate Impact

3. I agree with Dr. Siskin insofar as he states that disposition of the plaintiffs' claim of disparate impacts boils down to whether the Court accepts defendant's citywide analysis or, alternatively, accepts plaintiffs' analysis of impacts by community district ("CD") typology.<sup>3</sup>

4. A citywide approach to disparate impacts conceals disparities; a CD-typology

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<sup>1</sup> Feb. 10, 2021 declaration of Dr. Bernard Siskin, ECF 939 ("SD 2021").

<sup>2</sup> See my declaration of March 4, 2020, ECF 883 ("BD") and my declaration of Oct. 29, 2020, ECF 914 ("BROD").

<sup>3</sup> See SD 2021, at 2-3, ¶ 4.

approach reveals them. Because the citywide approach is not valid, citywide data as to impacts – the only data Dr. Siskin provides in connection with disparate impacts – should be disregarded (I do not discuss them further here).

5. Defendant must by now have referred dozens of times to the fact that the preference policy is applicable to each affordable housing development with a lottery, but that universality of application does not change the fact that lotteries are operated one at a time, and the community preference is implemented one lottery at a time. And each time the preference is applied, it is being applied to a sub-pool of the lottery’s applicants that have been generated from *within a community district that has particular demographic characteristics*,<sup>4</sup> with the policy separating that sub-pool from the balance of applicants to that lottery who can come from anywhere in the city.

6. Dr. Siskin complains that by recognizing different demographic typologies at the CD level I have somehow “manipulated” the data.<sup>5</sup> I have done no such thing. It is *defendant* who created a system whereby, as Dr. Siskin puts it using majority-race areas as the example, the preference is applied to a demographic grouping that “forces” the outcome that there will be fewer members of non-majority races.<sup>6</sup>

7. It is critical to pause here to reflect on Dr. Siskin’s statement. Think about a single lottery. His statement actually reflects agreement with an intuitive principle: the likelihood is that the implementation of the policy in respect to that single lottery will favor the majority-race group

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<sup>4</sup> As noted before, in a limited number of cases the “community district preference area” consists of a combination of more than one CD.

<sup>5</sup> See SD 2021, at 6, ¶ 13.

<sup>6</sup> See *id.*

of the CD in distributing preference and its benefits and disfavor the non-majority-race groups.<sup>7</sup>

8. Defendant, by choosing to have the policy, chooses to create the likelihood of this problem every time it runs a lottery.

9. I simply do not understand why these facts should have been ignored.<sup>8</sup> They are exactly what the issue of disparate impact is about: something that is seemingly neutral, but which operates in a way that results in racial disparities. And there was certainly no harm in testing the hypothesis that patterns could be identified within a grouping of lotteries with similar dominant-group properties: if defendant's system did not cause impacts, the data would have revealed that.

10. As noted last time, and not disputed by Dr. Siskin, I accounted for each applicant of each racial group in each typology.<sup>9</sup> (There is no serious claim that there is any problem with the specific typologies chosen – differentiating the majority ones from the plurality ones – nor is there any serious claim that there is a problem with aggregating projects to the typology level so as to have a stronger foundation of data on which to reach my conclusions.)

11. The outcomes were what they were – not theoretical, but based on who applied and what was done with each application. In brief, those outcomes were disparate impacts found in all seven typologies in respect to entrants; in six typologies in respect to apparently eligible applicants;

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<sup>7</sup> It could thus not be more of a contradiction when Dr. Siskin later says, SD 2021, at 30, ¶ 71, that defendant could not have been expected to predict the disparate results because no one would have thought to think of the perspective of CD typologies. But defendant only needed to think in CD-level terms – just as Dr. Siskin has as referenced in paragraph 6, above – and the disparities surely would have been anticipated. Aggregating from individual project to CD-typology simply makes clear whether a pattern exists.

<sup>8</sup> To repeat the obvious, this is precisely and explicitly what defendant wants the Court to do. *See, e.g.*, SD 2021, at 3, ¶ 5 (asserting that there is no reason to “create” subgroups). Again, the subgroups are a function of how defendant applies its preference.

<sup>9</sup> *See* BROD, at 6, ¶¶ 24-25.

and in all four majority typologies when it came to awards.<sup>10</sup> It is also the case that one of the follow-on consequences of the way the policy “tilts” the lottery at the inception is that a CD-typology level analysis of Dr. Siskin’s “consideration” data shows disparate racial impacts in six typologies.<sup>11</sup>

12. It is true that not every single non-dominant group was materially disadvantaged in every analysis in every CD typology. Aside from the fact that this is a vivid demonstration that I let the data speak for themselves (and reported them out, regardless of outcome), defendant apparently has a theory I have never heard of: that there is some multiple of impacts that must accrue before a policy can be said to be discriminatory.

13. In fact, in most of the housing cases with which I am familiar, there is one racial group, in one context, that is disadvantaged. This case, in other words, presents an unusually diverse and robust set of disparities due to *many groups being hurt* by the policy in *many typologies* at *multiple* stages and consequences of the lottery process.

14. Dr. Siskin again tries to distinguish the question of “whether” an applicant can compete fairly from the question of “where” the applicant can obtain housing.<sup>12</sup> In fact, the questions are intertwined: does the applicant have a level playing field regardless of where he or she applies? This has nothing to do with what CD typologies an applicant “prefers” beyond the applicant preferring (and having a right) to be able to compete equally at all times.

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<sup>10</sup> For entrants: *see* BD, at 21-28, ¶¶ 64-90. For apparently eligible applicants: *see id.* at 28-35, ¶¶ 91-114. For actual awards: *see id.* at 35-38, ¶¶ 115-24, and at 40-43, ¶¶ 129-38; *see also* BROD, at 21-24, ¶¶ 68-76. For simulated awards: *see* BD, at 38-39, ¶¶ 125-28, and at 41-43, ¶¶ 131-38; *see also* BROD, at 16-21, ¶¶ 58-67.

<sup>11</sup> *See* BROD, at 7-11, ¶¶ 28-36; *see also* BD, at 20, ¶ 60.

<sup>12</sup> *See* SD 2021, at 4, ¶ 8.

15. Similarly, Dr. Siskin again claims that my approach makes “being a CP beneficiary” the outcome of interest, trying to distinguish that from whether an applicant’s group can compete fairly.<sup>13</sup>

16. But the heart of having a level playing field is . . . having a level playing field. If the playing field is tilted, it is by definition not level. Dr. Siskin casually admits that “the CP policy provides a preference” and that this preference includes “greater odds to the CP beneficiary group.”<sup>14</sup> This is dispositive in terms of how the data must be approached. It simply is not relevant or sufficient to say, as Dr. Siskin does<sup>15</sup> that the policy has “no impact on whether an ‘entrant’ can submit an application” or that the policy does not have an impact on “whether an applicant is apparently eligible or not.”

17. What is relevant is how the entrants and apparently eligible applicants are *treated* by the policy, and this does come down to how the advantages and disadvantages of the policy are distributed racially within the different CD typologies – from the outset of the policy and onward.

18. To put it another way, here is a circumstance where there is no question that insider and outsider applicants are being offered different “terms and conditions.” The only question is whether the different terms and conditions (superior for insiders, inferior for outsiders) are allocated in a way to aid or harm disproportionately one or more racial groups in a typology.

19. When Dr. Siskin returns to causation, he fares no better than he has previously. All of the basic characteristics – including subjective ones – pre-exist the lottery.<sup>16</sup> We indeed do have

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<sup>13</sup> See SD 2021, at 14, ¶ 33. See also *id.*, at 13, ¶ 31.

<sup>14</sup> See *id.* at 19-20, ¶¶ 45-46.

<sup>15</sup> See SD 2021, at 19, § ¶ 44.

<sup>16</sup> See BROD, at 14, ¶¶ 47-50. Dr. Siskin attempts to take a crack at a new version of his “taking a test” analogy in a long note, see SD 2021, at 34-35, ¶ 82 n.24. The fact remains that the lottery does not involve a performance test as part of the lottery process: one’s pre-existing attributes are only revealed during the process. It also remains true that

a natural experiment. Applicant characteristics being what they are, the lottery just splits the pool in ways that help and hurt different racial groups in different CD typologies.

20. That some unspecified number of applicants can decide not to “follow through” due to a life change or become ineligible due to an income change,<sup>17</sup> is immaterial.<sup>18</sup> To the extent these things happened, they represented changes that existed independent of the lottery process. Those changes, in other words, would have happened in an equal-access lottery.

21. The difference the policy causes, however, is that, among those candidates who “follow through” and remain qualified, it is the policy and only the policy that disfavors the more racially diverse outsider pool *that would not even exist as a distinct sub-pool had the policy not separated it out*, both in placing that group of candidates at the back of the line and in putting a hard cap on the maximum percentage of awardees they can be. (This is another circumstance where Dr. Siskin is creating heat and not light: the results from his simulations confirm disparate impacts, and Dr. Siskin has said that the simulations eliminate any “confounding” factor and provide “a very good estimate of the expected impact of the CP policy on the lottery results.”)<sup>19</sup>

22. Likewise, Dr. Siskin’s attempt to separate out from that which is caused by the policy “shortfalls” and variations between groups,<sup>20</sup> is unavailing. Those differences existed; the policy’s decision was to favor the insider group that had the characteristics it did and to disfavor

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the preference plays an important role in whether one is considered and when one is considered, and also determines whether the quota for (cap on) outsider awards – regardless of “test performance” – has been met.

<sup>17</sup> See SD 2021, at 25-26, ¶ 59-60.

<sup>18</sup> It bears emphasis that this line of attack does not even purport to be relevant to the policy’s disparate impacts to entrants and apparently eligible applicants (who are harmed by the policy’s tilt independent of qualifications or “follow through”) – the argument is directed only to actual awards.

<sup>19</sup> See Aug. 13, 2020 declaration of Dr. Siskin, ECF 897 (“SD”), at 53, ¶ 104 and ¶ 104 n.77.

<sup>20</sup> See SD 2021, at 24, ¶ 56.

the outsider group that had the characteristics it did.<sup>21</sup> The action of the policy, which Dr. Siskin prefers to ignore, is that of allocating units disproportionately to the insider group (with its existing demographic characteristics) and of sequencing applicants to favor that insider group.

23. When it comes to consideration rates, I have shown both that the consideration rate for insiders is dramatically higher than it is for outsiders, and that consideration, like other important benefits of insider status, flows disproportionately to the dominant racial group in a CD typology to the detriments of other racial groups.<sup>22</sup> Dr. Siskin is now discomfited by my direct demonstration of the strong pattern by which the insider-advantage in consideration favors the dominant racial group over other racial groups in all CD typologies but plurality-Hispanic. This is shown in my previous declaration both in Table 17 and in the 80-percent chart depicted on the following page; the latter shows that in six of the typologies, *all* of the non-dominant groups are disadvantaged in a material way.<sup>23</sup>

24. While Dr. Siskin says that what I have done was “unnecessary” and “undermine[d] the purpose of the consideration study,”<sup>24</sup> I would argue that this is yet another circumstance where analysis by CD-typology *uncovers* disparities the Dr. Siskin’s analysis had kept covered up.

25. The balance of Dr. Siskin’s discussion of disparate impact (when you separate out the proceeding-citywide part) consists principally of renewed challenges to method or materiality. But the Court will note that these are, quintessentially, tempests in a teapot.

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<sup>21</sup> See BROD, at 23, 73-75.

<sup>22</sup> See BROD, at 7-10, ¶ 28-35.

<sup>23</sup> See BROD, at 9 (Table 17), and at 10, ¶ 34 n.19.

<sup>24</sup> See SD 2021, at 32, ¶ 77.



26. Dr. Siskin accepts that my analysis of simulated awards accurately reflects disparities at the CD-typology level.<sup>25</sup> Those show material disparities within all majority-race CD typologies and the plurality-Black typology.<sup>26</sup>

27. Dr. Siskin cannot dispute an alternate analysis I performed on actual awards (other than the dispute between the parties about citywide versus CD typology) *because I used his exact method*.<sup>27</sup> Those results include material disparities in all four majority-race typologies and the plurality-Black typology.<sup>28</sup>

28. In short, while I continue to assert that my preferred methods for analysis of actual and simulated awards are the proper ones and continue to criticize Dr. Siskin's methods for understating disparity at the awards stage, these disputes do not create meaningful issues for the Court. That is because the Court is *not* faced with a question as to whether or not the results are on one side of the permissible line or not. Regardless of method, the majority-race typologies for actual and simulated awards are always on the impermissible side; the differences that the alternative methods yield have to do whether the results (at minimum) demonstrate disparities that are very much material or demonstrate disparities that are even more material than that.

29. This is conveyed very well in the chart that was incorporated into plaintiffs' PRO Brief.<sup>29</sup> By four different methods, material disparate impact at the awards levels was shown for

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<sup>25</sup> See *id.*, at 2, ¶4.

<sup>26</sup> See BROD, at 20, Table 20.

<sup>27</sup> See *id.*, at 21-22, ¶¶ 68-70.

<sup>28</sup> See *id.*, at 22, Table 21.

<sup>29</sup> See plaintiffs' Nov. 5, 2020 brief, ECF 928 ("PRO Brief"), at 26, Chart 5.

each of the four majority-race typologies. In other words, variations between methods did not change the core results.

30. And this is in addition to the standalone racial disparities that the policy imposes on entrants and apparently eligible applicants. By “standalone,” I mean the denial of a level playing field consummated at the very beginning of the process when otherwise equal odds are distorted by the policy.<sup>30</sup> In my original declaration, I show that, by two methods, there are material racial disparities in all seven typologies for entrants and for all typologies except plurality-Hispanic for apparently eligible applicants.<sup>31</sup>

31. In all cases, I confirm the meaningfulness of the disparities with an 80 percent test.<sup>32</sup>

Note that Dr. Siskin has at no time offered alternative calculations.

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<sup>30</sup> Dr. Siskin cannot deny that this tilt occurs independent of applicant qualification, interest, or lottery number. Faced with this, Dr. Siskin attempts to put the onus of the policy’s decision to disfavor outsiders on individual applicants. *See* SD 2021, at 22, ¶ 53 n.18. This is highly misleading. *All* outsider entrants and *all* outsider apparently eligible applicants are “sent to the back of the line,” and all are foreclosed from 50 percent of the units. That some applicants are randomly assigned lottery numbers “low in the queue” only highlights the fact that *it is the policy* that creates two *different* queues. A lottery number that is “bad” for an outsider can be entirely “good enough” for an insider – until the preference is exhausted, the *worst-possible* insider number is *better than the best-possible* outsider number.

In any event, without the policy, *all* applicants (insiders and outsiders) would have to face the prospect of a bad draw on a single line – that is what is expected in an equal-access lottery and what is denied in the lottery process as warped by the policy.

Dr. Siskin tries to shift the burden of lower odds to lottery participants in another way. He cannot do this with apparently eligible applicants (who, by definition, are apparently eligible), but does do so with the subset of entrants who are *not* apparently eligible (who are, in other words, apparently *ineligible*). *See id.* at 21, ¶ 48. But Dr. Siskin admits members of this subset could be reached by a developer, *see* SD 2021, at 35, ¶ 83, and it is clear that the policy reduces their opportunity to do so. (Whether the *apparently* ineligible would ultimately be found to be *actually* ineligible is not known.) Regardless, it was my understanding – though it is of course the court’s decision – that everyone is supposed to be given an equal opportunity to compete (even if they would not ultimately be awarded a unit). The policy takes what would have been equal prior odds for everyone and reduces them substantially for all outsider entrants (as well as for all outsider apparently eligible applicants), while improving the odds for insiders.

<sup>31</sup> *See* BD, at 22, ¶ 69 and Table 3, and at 24-25, ¶ 76 and Table 4 (entrants); *see id.* at 31-32, at ¶ 102 and Table 6, and at 33, ¶ 106 and Table 7 (apparently eligible applicants). Note that Dr. Siskin never addresses follow-on consequences of the policy’s “tilt” at the CD-typology level for any purpose.

<sup>32</sup> *See id.*, at 23-24, ¶¶ 72-73 and ¶ 73 n.36 and at 26, ¶¶ 80-81 and ¶ 81 n.37 (entrants); *see id.* at 32, ¶ 104 and ¶ 104 n.46, and at 33-34, ¶¶ 107-108 and ¶ 108 n.48 (apparently eligible applicants).

32. In all cases – and this is true for entrants, apparently eligible, and actual awards,<sup>33</sup> I have shown statistical significance. I will restate my rebuttal to Dr. Siskin’s critiques in a moment, but I should first note that here, too, Dr. Siskin has not taken the opportunities offered by his declarations to submit what he considers to be a more accurate calculation of statistical significance. My calculations are the only ones before the Court.

33. And, as noted before, the criticisms by Dr. Siskin do not hold up. Dr. Siskin claims my method of assessing statistical significance is inappropriate and also that my units of standard deviation are “faulty,”<sup>34</sup> while not specifying what is faulty about them. As noted in my declaration, and the accompanying citations to the option in SAS for the procedure used, as well as citation to relatively recent statistical literature, I confirmed that I did, in fact, use the appropriate measure.<sup>35</sup> Please note that my previous declarations have pointed Dr. Siskin to the documentation for the “RISKDIFF” option that I used, and he has not responded to that documentation.

34. To the extent that (either with respect to the 80-percent rule or to tests of statistical significance) Professor Siskin is insisting that they can only be applied to “selection rate,” his critique encounters two problems. First, some of the award analyses are selection-rate analyses; namely, the selection rate analyses as applied to simulated awards and actual awards in Sections I and J of the BROD.<sup>36</sup>

35. Second, and more fundamentally, he is mistaken. As previously noted, the point of

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<sup>33</sup> As noted in BD, the statistical significance of the 1,000 simulations is self-evident for all cases. *See id.* at 42, ¶ 133.

<sup>34</sup> *See* SD 2021, at 30, ¶ 72.

<sup>35</sup> *See* BROD, at 13, ¶ 44-46, noting, *inter alia*, at ¶ 45, that my test of statistical significance “examined the exact same underlying data that were the basis of comparing racial groups to one another. Here, however, the test examined each racial group in comparison to *all others*. This is exactly [ ] how the procedure is intended to work.”

<sup>36</sup> *See* BROD, at 15-21, ¶¶ 51-67, and at 21-24, ¶¶ 68-76, respectively.

both the 80 percent test and of measuring statistical significance is to assure the Court (or whomever else may be assessing data) that disparities are meaningful.<sup>37</sup> To repeat (briefly) the thrust of what I have written briefly: “Housing cases simply provide different variations than do employment cases, and methods need to be attuned to measuring what the challenged policy is actually doing.”<sup>38</sup>

36. As it happens, the entrant and apparently eligible data can be understood as “selection rates” if one recognizes that what the policy puts at issue is the racial composition of who is *selected* to get preference (and the benefits accruing from that preference).

37. It is true, as I have said before, that I find the outsider-to-insider-change method particularly suited to capturing simultaneously the various things that the policy does to different groups. But my other method – highest-insider-share – also offers valuable insight into disparate impact and reflects the relative *rate of selection* for community preference of different groups. Moreover, either translated to the 80-percent rule or as measured by statistical significance, my highest-insider-share method is material within all typologies for entrants and within all typologies except plurality-Hispanic for apparently eligible applicants.

38. I add only a few additional points as to disparate impact to illustrate some of the misdirection that Dr. Siskin continues to engage in. Importing citywide analysis as usual, Dr. Siskin derides the scope of awards that would have to change for there to be “parity.”<sup>39</sup>

39. But, as explained previously, even comparing his “with preference” to “without preference” simulations (a comparison he prefers and that I have explained is suboptimal), a CD-

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<sup>37</sup> See *id.*, at 12-13, ¶¶ 42-43.

<sup>38</sup> See *id.*, at 12 n.26.

<sup>39</sup> See SD 2021, at 40, ¶ 93.

typology analysis reveals powerful disparities. In the majority-White typology, for example, White advantage from the policy is 65.77 percent and Black disadvantage is -30.17 percent, not the much smaller +15.1 percent for Whites and -3.9 percent for Blacks that Dr. Siskin wanted the Court to accept.<sup>40</sup>

40. Moreover, Dr. Siskin refers to a base of 10,245 awards, but the simulated awards in the majority-White typology are only 2,031, and, within that, Whites gained 173 units from the policy and Blacks lost 199 units from the policy, hardly trivial numbers.<sup>41</sup>

41. Dr. Siskin also tries to create an anomaly where there is none by citing a theoretical *individual* who is an outsider in two lotteries but who is “considered part of the protected class” in only one.<sup>42</sup> This is a fundamental misunderstanding – and also a reason why my methods are so useful. They account for members of *all* racial groups, whether insiders or outsiders, in *all* typologies. Everyone is potentially a member of a protected-class group. Members of the hypothetical applicant’s racial cohort may or may not be hurt relative to other racial cohorts in a typology – the data decide that by seeing whether advantages of preference and disadvantages of the lack of preference are distributed disproportionately.

42. As for predictability, Dr. Siskin is mistaken here, too.<sup>43</sup> He offers no reason why the patterns that emerged in 168 lotteries would change. The policy was and is designed to help insiders at the expense of outsiders, and the outsider pools are consistently more racially diverse

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<sup>40</sup> See BROD, at 16-19, ¶ 59-64, and *compare* Table 18 with Table 19.

<sup>41</sup> *Id.* at 19, ¶ 64.

<sup>42</sup> See SD 2021, at 6-7, ¶ 14.

<sup>43</sup> See SD 2021, at 29-30, ¶ 70.

than the insider pools. Saying that “maybe it will all be different in the future” completely undercuts the point of analyzing an ongoing policy that has produced decisive results.

43. Finally, Dr. Siskin introduces a hypothetical<sup>44</sup> which is designed to defend himself against my argument that the differences caused by the policy are diluted when comparing the “with preference” to the “without preference” simulations. The point I had made can be summarized as follows: even though all of the difference to be observed arises from the half of the units that are preference units versus an equivalent number of non-preference units, Dr. Siskin includes non-preference units that could not add to the difference but that do double the base as to which the difference is calculated. This artificially reduces the magnitude of the difference.<sup>45</sup>

44. Dr. Siskin’s hypothetical is a distraction that in no way grapples with the problem I identified. The question in this case is not a comparison between two different policies (outsider-restriction versus municipal employee preference, in the hypothetical). The question is what impact the challenged *community preference policy* actually has. The hypothetical is, however, illuminating in one respect. By including in the base for the calculation the 5,000 units not reserved for CP beneficiaries, Dr. Siskin reveals the manipulation: he has once again artificially cut the impact from the 2 percent that his Illustration shows for the relevant units to *half that* because he includes units that would not be expected to have any effect.

#### Perpetuation of segregation

45. Here, again, I can begin with an agreement I have with Dr. Siskin: the issues in

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<sup>44</sup> See SD 2021, at 38-39, ¶ 90 and Illustration 1.

<sup>45</sup> See BROD, at 16, ¶ 56 and ¶ 56 n.36.

dispute . . . are very limited.”<sup>46</sup>

46. What cannot be disputed is that there are six, two-race pairings, and they were examined in terms of actual awards, awards sought, and simulated awards. In 17 out of 18 cases (all but actual awards in respect to the Hispanic-Asian pairing), the scope of the net-integrative moves for outsiders (non-beneficiaries) was materially higher than that for insiders (CP beneficiaries). This is true even if one, like Dr. Siskin, improperly includes those who are not part of the racial pair at issue.<sup>47</sup>

47. The same is true when it comes to the question of whether to compare insiders and outsiders in the “with preference” model (my approach) or to compare results as between the “with preference” simulation as a whole with results in the “without preference” simulation as a whole (Dr. Siskin’s approach). Here, again, there are material disparities pursuant to both analyses (showing less integration under the “with preference” simulations than under the “without preference” simulations, even though Dr. Siskin’s preferred version understates the differences).<sup>48</sup>

48. In other words (as with disparate impact) the policy is firmly on the impermissible side of the line regardless of which method is selected.

49. The foregoing represents a serious – indeed, fatal – dilemma for defendant and Dr. Siskin. So, Dr. Siskin cycles once more through a series of diversions.

50. As noted previously, the dissimilarity index is not used by housing segregation experts to measure whether a *policy* perpetuates segregation.<sup>49</sup> By definition, the dissimilarity

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<sup>46</sup> See SD 2021, at 40, ¶ 94.

<sup>47</sup> See BROD, at 36-39, ¶¶ 131-36 and Tables 22-24, and see plaintiffs’ initial Mar. 6, 2020 brief, ECF 882 (“PI Brief”), at 38, Chart 4.

<sup>48</sup> See BROD, at 39-43, ¶¶ 137-46. Tables 25 and 26.

<sup>49</sup> See *id.* at 30-32, ¶ 108-113.

index is the measure of the percentage of households *in the entire city* that would have to move in order to achieve a demographic result where the same population proportion as between two racial groups would exist in each census tract. That has nothing to do with whether a policy operates to distribute *the units to which the policy applies* in a way that stymies integrative moves (perpetuates segregation) or not.

51. Dr. Siskin references my view that stymieing of integrative moves constitutes perpetuation of segregation,<sup>50</sup> but beyond some repartee intended to belittle my decades of experience in the field of housing segregation, he does not argue the point.<sup>51</sup>

52. In fact, stymieing captures *exactly what the policy does*. Without the policy, an enormously higher percentage of units would be awarded to outsiders. Those outsider moves and outsider moves-sought yield a distinctly higher relative percentage of net-integrative moves as compared with insider moves and insider moves-sought. The policy restricts those moves, forcing there to be fewer net-integrative moves.

53. Notwithstanding the complications that Dr. Siskin attempts to interpose, it is a simple concept: unrestricted by the policy, a particular level of net-integrative moves would be achieved. The policy, in forcing a reduction in that level, is forcing there to be more segregation for a longer period of time.

54. This issue is one that has enormous implications not just for this case but for a wide variety of housing segregation cases, including exclusionary zoning. Take a town (there are too many to count) that intentionally excluded Blacks for decades, resulting in a dramatically low percentage of Blacks living in the town. Today, intentional discrimination is outlawed by the Fair

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<sup>50</sup> See SD 2021, at 41-42, ¶ 97.

<sup>51</sup> See *id.*



Housing Act, and so there are some Blacks who can move in. In this context, any Blacks moving in count as people making net-integrative moves. But the town has extraordinarily restrictive zoning, so very little housing is permitted to be built. When the zoning is challenged, the town responds that its exclusionary zoning scheme actually allows for *some* integrative moves and hence is immune to challenge. That position is utterly at odds with the reality of the situation and would allow massive amounts of what defendant would call “mere” stymieing to occur.

55. I am sorry to say that Dr. Siskin’s attempt to justify the inclusion of “not-in-group” movers or prospective movers<sup>52</sup> is bafflingly off-point. He does not (and cannot) explain, for example, why Asian and Hispanic movers would be relevant to calculating net-integration as it specifically and exclusively pertains to the net-integrative effect of moves (and prospective moves)<sup>53</sup> between Whites and Blacks.

56. Dr. Siskin asserts that, to get “[s]tandardized ratios,” one needs a “common base.”<sup>54</sup> He fails to appreciate that there are six *separate* comparisons being made (one for each pair). They are independent from each other.

57. In sum, Dr. Siskin’s point has nothing to do with including those individuals relevant to a two-pair comparison and everything to do with artificially enlarging a denominator

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<sup>52</sup> See *id.*, at 52-53, ¶ 122.

<sup>53</sup> Note that Dr. Siskin, though he used apparently eligible applicants originally, prefers to ignore the perpetuation of segregation consequences in terms of moves sought – no surprise given how damning they are.

<sup>54</sup> See SD 2021, at 52-53, ¶ 122 and ¶ 122 n.36.

with awardees whose moves, definitionally, are all “no effect.”<sup>55</sup> In this manner, he suggests an artificially low percentage of net-integrative moves. This attempt should be rejected.

58. If one is somehow able to disentangle Dr. Siskin’s insertion of the dissimilarity index (it is enmeshed in every aspect of his discussion), it appears that he *agrees* that the number of net-integrative moves has to be analyzed. But he fails to discern the value of analyzing *rates*.

59. The rate of those moves is important for two reasons. First, when you have a rate you are able to see what happens when one type of move (an outsider move) is restricted. That is, the policy is restricting moves that yield more net-integration (because the policy caps the percentage of units that can go to outsiders at 50 percent, even though the percentage of apparently eligible outsider applicants is vastly greater than that). So, obviously, if they were not capped, the proportion of moves going to outsiders, whose moves are more integrative, would be higher.

60. The second reason actually reflects my concern not to overstate the case. A rate *is* what allows you to compare differently sized baskets, and thus allow one to calculate the

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<sup>55</sup> Looking at actual awardee data from BD Ex 16 (summarized below), it becomes clear that the number of not-in-group awardees is quite substantial. In four cases, the not-in-group awardees that Dr. Siskin wishes to include *outnumber* the awardees in the pair actually being studied (W v AA, W v A, AA v A, and H v A). In a fifth case, the not-in-group awardees are similar in number to the pair actually being studied (W v H). And in the sixth case (AA v H), the not-in group awardees are still substantial. These data represent both insider and outsider awardees combined (the rightmost of three vertical groupings in Ex 16).

Racial Pairing	All within pairing (segregate, plus no effect, plus integrate)	All not in group	Number not in group as percentage of number in racial pair being studied
W vs. AA	3,857	4,367	113.22%
W vs. A	1,693	6,531	385.76%
W vs. H	4,259	3,965	93.10%
AA vs. H	6,000	2,224	37.07%
AA vs. A	3,434	4,790	139.49%
H vs. A	3,836	4,388	114.39%

proportionate or disproportionate effect on integrative moves for various racial groups. Take, for example, the moves that apparently eligible applicants sought to make, and use the White-Black pairing as an example. The net-integration that would be yielded from outsiders is -358,187, whereas the net-integration that would be yielded from insiders would be -5,609.<sup>56</sup> Part of this differential is due to the fact that the size of the outsider group is much larger. If we only compared insider versus outsider net-integration by dividing the raw insider net-integration number by the raw outsider net-integration number, the result would be that insider net-integration was only 1.6 percent of outsider net-integration.

61. Instead, I first took the *net-integrative* moves sought by outsiders from within the race-pair being studied as a percentage of *all moves* sought by outsiders within the race-pair being studied and yielded a percentage. I then did the same for insiders. Finally, I calculated the relative net-integration by dividing the insider net-integration percentage by the outsider net-integration percentage (the “relative percentage” approach). In that way, I was able to show that insider net-integration in relative terms was 29.51 percent of outsider net-integration (extraordinarily low).<sup>57</sup> In other words, I took care not to overstate the case.

62. One can take the absolute numbers if one wishes and determine in one step absolute insider net-integrating moves as a percentage of absolute outsider net-integrating moves. That can be done if one directly divides the number of net-integrating insider moves by the number of net-integrating outsider moves (the “one-step method”). Here I use the example of actual awards. When one follows that procedure, it is again clear that my relative percentage approach does not

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<sup>56</sup> See BD Ex 17, at 1 (counts), first horizontal section, net number in the left panel versus net number in the central panel. As previously noted, the negative value is perhaps confusing: it represents the net number of moves that are reducing segregation, so the more negative, the more net-integrative.

<sup>57</sup> See *id.* at 2 (percentages) first horizontal section, rightmost column (“relative percentage”).

overstate the matter as compared with the one-step method. Table 27 shows on the left side the relative percentages for actual awards previously reported in Exhibit 16. The Table shows on the right that calculation for the one-step method.<sup>58</sup>

<b>Table 27</b>		
<b>Different methods of comparing insider net-integration as percentage of outsider net-integration (actual awards)</b>		
<b>Two-group pairing</b>	<b>Relative percentage</b>	<b>One-step method</b>
W v AA	32.71%	28.09%
W v A	45.87%	50.00%
W v H	65.98%	62.81%
AA v H	12.84%	10.53%
AA v A	25.43%	19.94%
H v A	89.52%	79.48%

63. In five of six cases the one-step method yields marginally *stronger* results (even further below 80 percent) than does the relative percentage method I have used.<sup>59</sup> In the sixth case (W v A), the results are marginally weaker (but still far below 80 percent).

64. One thing that is useful about looking at the actual numbers is to reemphasize the importance of comparing insiders and outsiders (whether in respect to actual moves, moves sought by apparently eligible applicants, or simulated moves). In each case, one is able to see that the net-integration that comes from outsider moves (the type that the policy constrains) contributes disproportionately to all net-integration in the pairing being studied. An illustration is found in the AA v H comparison across the 1,000 simulations with preference.<sup>60</sup> The total number of simulated

<sup>58</sup> For data underlying the middle column of Table 27, *see* BD Ex 16, at 2 (rightmost column). For the data underlying the rightmost column of Table 27, *see id.* at 1 (net number from central panel divided by net number from left panel).

<sup>59</sup> This includes the H v. A pairing, which I had reported for actual awards as not having had a sufficient disparity for actual awards to satisfy the 80-percent test.

<sup>60</sup> *See* BD Ex 18, at 1 (counts).

awards to members of this racial pair who were outsiders was quite similar to the total number of simulated awards to those who were insiders.<sup>61</sup> Yet net integration across the 1,000 simulations was -530,630 for outsiders and only -68,976 for insiders.<sup>62</sup>

65. Whether you look at the simulations in the aggregate or scale them down to a single average simulation (-531 outsider; -69 insider), that is a significant difference in contribution to integration between outsiders and insiders.

66. As for what I have described as the inferior alternative of comparing the entire “with preference” simulation to the entire “without preference” simulation,<sup>63</sup> I already in my last declaration executed the one-step method and demonstrated that the results remained material both with an insider-outsider comparison in the “with preference” simulation, and when comparing the entire “with preference” results to the entire “without preference” results.<sup>64</sup>

67. Ultimately, Dr. Siskin is forced to argue against application of the 80-percent rule even though he has previously acknowledged that, while not universally accepted, it “is a commonly used measure to define practical significance.”<sup>65</sup> He says that the statistics at hand with perpetuation are not true “selection rates” and that three groupings are involved.<sup>66</sup> He misses the mark on multiple fronts. First, we are comparing not three things but one thing: net-integration rate. Second, we are in fact looking at disparate outcomes. That the net-integration rate “achieved”

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<sup>61</sup> *See id.* This was true for all pairs in the simulations.

<sup>62</sup> *See id.*

<sup>63</sup> As opposed to comparing insiders and outsiders as they were treated within the “with preference” simulations. *See* BROD, at 39-41, ¶¶ 137-40.

<sup>64</sup> *See id.* at 39-41, ¶¶ 137-42 and Table 25, and at 41-43, ¶¶ 142-46 and Table 26, respectively.

<sup>65</sup> *See* SD, at 50, ¶ 100.

<sup>66</sup> *See* SD 2021, at 48, ¶ 113.

is not literally a “selection” does not change the fact that “selection” and “achievement” are both outcomes whereby rates have to be compared.

68. Third, there is no reason offered why the 80-percent rule is not a reasonable rule of thumb here: it connotes a difference that is in fact material.<sup>67</sup> What is particularly impressive about the results here is that the issue is not “over or under 80 percent,” but rather “under 80 percent by a huge amount or by a more modest amount.”

69. And, as discussed above, there is no substance to Dr. Siskin’s critique of my application of statistical significance. Note that he offers no alternative calculations.<sup>68</sup>

70. So defendant’s assertions of differences being “small” is just that: an assertion that is contradicted by the comparisons of net-integration *to one another*. So long as the Court rejects, as it should, defendant’s argument that change-in-the-citywide-dissimilarity-index should be the measure, there is actually no dispute as to materiality.

71. In the end, the data are what they appear to be: there is significant stymieing of integration (perpetuation of segregation) not just for one pairing, but for every pairing for all three types of comparison except H v. A for actual awards (in other words, 17 out of 18 comparisons).

72. As for predictability, Dr. Siskin is mistaken here, too. He offers no reason why the patterns that emerged in a study of 145 lotteries (the number of lotteries studied for perpetuation

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<sup>67</sup> Dr. Siskin’s statement that a differential that does not exceed 80 percent is material if one were comparing change in dissimilarity index (from .80 to .64), *see* SD 2021, at 48, ¶ 114, only underlines my point. Yes, of course a 16 percent reduction in the percentage of the entire city’s population that would have to move in order to achieve a demographic result where the same population proportion as between two racial groups would exist in each census tract is practically significant. The fact that a disparity that failed the 80-percent test (in the sense of the disparity being insufficient) can nevertheless be practically significant does nothing to suggest that 80-percent is not a useful line of demarcation. In other words, Dr. Siskin’s example does not show that a *lower* percentage than 80 percent would *not* represent a practically significant disparity (which would be the only circumstance that could aid his argument); only that percentages *higher* than 80 percent could still be practically significant (hence the test being widely treated as a rule of thumb).

<sup>68</sup> *See supra* at 10-11, ¶¶ 32-35.

of segregation purposes was slightly smaller than the number studied for disparate impact purposes) would change given the fact that the policy was and is designed to help insiders at the expense of outsiders, even though outsiders are consistently more racially diverse.

Distance study

73. Dr. Siskin reconfirms that very little of the variance he reports between and among applicants in terms of the likelihood of applying to a particular lottery is explained by distance; he further confirms that he could not identify or quantify the factors that accounted for the lion's share of the variance.<sup>69</sup>

74. He then tries to divert attention with a discussion of what the racial impact of changing the CP boundaries to a two-mile radius would be.<sup>70</sup> This misses the point. I had identified a two-mile radius because that was the *furthest* an insider applicant could move to a project from his or her home in 70 percent of the lotteries under consideration. If the policy were intended to help applicants stay within that distance, "the policy is not organized to capture these 'close to home' applicants."<sup>71</sup> Far more outsiders, not the beneficiaries of the preference, live within two miles of a project applied for.<sup>72</sup> In other words, what is being demonstrated is the lack of a logical nexus between the policy's ostensible goal and how it operates.

\* \* \*

75. There is no question but that the policy causes material racial impacts and materially perpetuates segregation.

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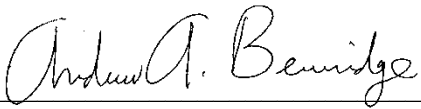
<sup>69</sup> See SD 2021, at 56, ¶ 129.

<sup>70</sup> See *id.* at 57, ¶¶ 132-33, and Table 10.

<sup>71</sup> See BROD, at 45, ¶ 156.

<sup>72</sup> See *id.* at 45-46. ¶¶ 157-58 and Chart 2.

Executed in Westchester County, New York on March 8, 2021.

  
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Andrew A. Beveridge