----------X

SHAUNA NOEL and EMMANUELLA SENAT,

PLAINTIFFS,

----- against ----- **15-CV-5236 (LTS) (KHP)**

CITY OF NEW YORK,

DEFENDANT.

----------X

SUR-REPLY EXPERT REPORT OF BERNARD R. SISKIN, Ph.D.

Originally submitted OCTOBER 25, 2019

Initially Amended November 12, 2019

Amended December 13, 2019

This amended sur-reply report[1] responds to the criticism of my September 4, 2019 amended expert report, contained in the Reply Expert Report of Professor Andrew A. Beveridge dated September 19, 2019 and amended October 27, 2019. This report identifies and explains the flaws in his criticisms. In those cases in which he raises issues that have merit, this report addresses the impact those issues have on my analysis and findings. My report is divided by headings that correspond to the topics presented in Dr. Beveridge's report.

1. Understanding Disparate Impact Across the City

Dr. Beveridge claims that I am trying to mask the impact of the City's community preference ("CP") policy by focusing on the disparate impact of the CP policy Citywide and by ignoring the CP policy's alleged "racial/geographic sorting process."[2] However, Dr. Beveridge's arguments demonstrate his confusion in executing a proper disparate impact analysis. Simply put, Dr. Beveridge does not undertake a proper disparate impact analysis, but instead, he does a convoluted analysis that combines the theories of disparate impact and perpetuation of segregation, and in fact, demonstrates neither.

Disparate impact and perpetuation of segregation are separate and distinct theories, which are examined and calculated differently. By focusing on the racial demographics of where the housing is located, Dr. Beveridge's analysis, which he calls a "disparate impact analysis," is really a flawed perpetuation of segregation analysis, and is not a disparate impact analysis. In fact, his perpetuation of segregation section in his April 1, 2019 report simply refers back to his "disparate impact analysis" and does no additional analysis.[3] Dr. Beveridge's comments on the

[1] This report amends and replaces my prior sur-reply report dated October 25, 2019, as well as my prior amended sur-reply report, dated November 12, 2019.

[2] See Dr. Beveridge's September 19, 2019 reply report ("Beveridge September 2019 report") at ¶ 5.

[3] See Beveridge September 2019 report at ¶¶ 30, 31, and 101.

"racial/geographic sorting process" raise the issue of *where* applicants are able to compete for housing, not *whether* they are able to compete for housing fairly.[4] The former is a question of segregation while the latter is a question of disparate impact. As I made clear in my deposition while discussing a hypothetical posed to me (attached to the September 2019 Beveridge report as Exhibit 1 at page 78 line 24-25), a policy can perpetuate segregation while not having a disparate impact based upon race. They are distinct analyses. Unlike Dr. Beveridge, I did not conflate the questions, but undertook separate analyses to properly address both claims independently.

Dr. Beveridge further incorrectly argues that I have adopted a "separate but equal" approach because I only conducted a disparate impact analysis of the impact on the CP policy overall,[5] and ignored the impact of the location of the apartments that were awarded[6] and thus the effect of the CP policy favoring the "dominant" race.[7] He is wrong. Dr. Beveridge ignores the fact that I separately addressed the issue of the extent to which the "sorting process" of the CP policy (i.e., the impacts caused by where the apartments were awarded to applicants by race) impacts segregation, because unlike Dr. Beveridge, I recognized that it is relevant to the issue of perpetuating segregation and addresses a different question than whether the CP policy had a disparate impact on being awarded an apartment by race.

Fungibility of Apartments

Although Dr. Beveridge's analysis assumes that all apartments awarded in the same racial typology are fungible, he now claims that the apartments are not fungible because individuals may

[4] See Beveridge September 2019 report at ¶ 3.

[5] See Beveridge September 2019 report at ¶ 9.

[6] See Beveridge September 2019 report at ¶ 3.

[7] See Beveridge September 2019 report at ¶ 9.

prefer one apartment over another.[8] However, Dr. Beveridge did not rank the apartments or CD typologies to discern which is "better." At his deposition, Dr. Beveridge stated that people's preferences to live in certain neighborhoods are often based upon "neighborhood characteristics."[9] However, for disparate impact analysis, whether an apartment is fungible or not depends upon objective criteria, not individual desires that cannot be quantified.[10] While some of the pros and cons of a particular neighborhood discussed by Dr. Beveridge during his October 4, 2019 deposition[11] may be objective and could have been used to rank the CD typologies for purposes of a disparate impact analysis, he did not do that, and thus, in effect treated the CD typologies as well as all apartments as fungible. Since Dr. Beveridge did not rank the apartments or CD typologies, I too treated the apartments as fungible.

To the extent that Dr. Beveridge is using the racial demographics of the CDs in which the projects are located (i.e., his CD typologies, which are based upon racial demographics) to distinguish the quality of the awarded apartments (as he insists that the awarded apartments are not fungible),[12] he never ranked the CD typologies for purposes of his analysis. Thus, he states no opinion on whether awarded apartments in one CD typology are better than awarded apartments

[8] See Beveridge September 2019 report at ¶ 6.

[9] See Beveridge October 4, 2019 deposition transcript ("October 4th Transcript"), page 79 line 2 through page 80 line 3. Copies of all pages cited to in this report from the October 4th Transcript are collectively annexed hereto as Appendix I.

[10] To demonstrate the difficulty in determining whether one awarded unit is better than another, consider the following example. An applicant applies for an apartment in Project A, which is located in the CP area in which they live, and also for an apartment in Project B, which is *not* located in the CP area in which they live. Here, the CP policy increases their odds of getting an apartment in Project A and decreases their odds of getting an apartment in Project B. Neither Dr. Beveridge nor I have any objective way to assess whether getting an apartment in Project A is better or worse than getting an apartment in Project B, and therefore we must assume them to be fungible for purposes of a disparate impact analysis.

[11] See Beveridge September 2019 Report at ¶ 8.

[12] See October 4th Transcript, pages 75 through 76; Beveridge September Report at ¶ 6.

in another. Nor does he provide any rationale for why an awarded apartment in one CD typology may be better than an awarded apartment in another. The only difference between the CD typologies is the racial mix.

Thus, to the extent that Dr. Beveridge is assuming that the "better" CD typology is the majority white CD typology (consistent with Plaintiffs' position in paragraphs 100-102, 178-182 of the Second Amended Complaint), and therefore, the awarded apartments in the majority white CD typology are "better," that is an unstated and unsupported assumption.[13] His disparate impact analysis simply does not support any conclusions regarding applicants' ability to access "better" apartments, despite his insistence that the apartments are not fungible.

Dr. Beveridge's new argument that the CP policy's impact is the manner in which it affects an applicant's "personal decisions regarding competing in the affordable housing lotteries of their choice"[14] fails for similar reasons. Not only is this a new approach to disparate impact that was not raised in the Second Amended Complaint (which asserts a disparate impact in the opportunity to compete for housing opportunities), but it is based upon an applicant's subjective preference of where they want to live. Dr. Beveridge admits that he cannot know applicants' preferences.[15]

---

[13] Moreover, to undertake a disparate impact analysis based upon whether the protected class was disparately impacted in being awarded a better apartment, as opposed to whether a protected class is disparately impacted based in being awarded apartments, one must clearly rank the awarded apartments with objective criteria. Opposing experts can then directly address those criteria and ranking, as well as the underlying selection rate analysis. Dr. Beveridge failed to undertake a disparate impact analysis in this manner, as he never ranked awarded apartments or CD typologies, or even provided objective criteria data. Only through the reply report and his deposition did it become clear that Dr. Beveridge was using race to attempt to distinguish the quality of the awards. Moreover, focusing only on the majority white areas, at best, is insufficient unless Dr. Beveridge is concluding all the other racial CD typologies (for example, majority African American, majority Hispanic, majority Asian) are fungible. That is, majority white CD typologies are distinct but all other CD typologies are interchangeable. Dr. Beveridge is silent on this issue.

[14] See Beveridge September 2019 report at ¶ 4.

[15] See October 4th Transcript, page 83, lines 16-18.

Moreover, if he attempted to examine applicants' choice of projects through the lens of his CD typologies, he would have found that there is clearly no consensus that white majority CDs are preferred. Of the 283,680 applicants[16] who applied to projects in only one racial CD typology outside of their CD area,[17] only 34.3% chose to apply to a project in a majority white typology. Only 25.7% of African Americans, and 30.9% of Hispanic applicants applied to projects in only one racial CD typology outside of their CD area. African Americans were almost equally likely to apply only to projects in majority white (25.7%), majority African American (21.7%), or majority Hispanic (20.7%) areas.

In sum, Dr. Beveridge cannot, and did not, support any "disparate impact" finding based upon not providing people with the opportunity to choose[18] based upon their preferences.[19]

---

[16] This represents approximately 40% of the 711,867 applicants who applied for at least one of the 168 projects.

[17] The preference for a racial CD typology for applicants who applied to multiple racial CD typologies outside of their CD area cannot be determined, so they are not studied. That is, if an applicant applies for projects in majority white and majority African American CD areas, we cannot determine which they might prefer.

[18] Dr. Beveridge's points regarding lack of "choice" are essentially meaningless, as there is little practical chance that an applicant would actually have this type of "choice." An applicant has a very small chance of being awarded an apartment if they are a NP application and they have a small, although better, chance to be awarded an apartment if they are a CP application. Therefore, an applicant that has applied to both CP and non-CP projects may be so fortunate to be awarded an apartment through their CP application and their NP application. However, given the number of applications to each lottery, this would be rare. If there was no CP, the chances of an application being awarded an apartment twice in the lottery would be exponentially smaller. Thus, without CP, because there is such a small chance that an application would have the choice between two apartments, such arguments are inconsequential. For example, if someone applies for 30 projects (90% of all applicants applied for fewer than 30 projects), their chance of getting at least two apartments is less than .005. Hence, if 1,000 such applicants applied for 30 projects each, only 5 would have a choice. Furthermore, a substantial proportion of applicants (39.2%) have only applied for a single project, and therefore, there is no possibility that these applicants would have such a "choice" because of their own decisions and such arguments would not be applicable for them.

[19] Dr. Beveridge's argument that using the CD typologies is necessary to measure this "impact" of the alleged lack of choice makes no sense and is based upon unproven assumptions. First, it appears that Dr. Beveridge is assuming that people's decisions of where to live are wholly if not fundamentally driven by racial demographics of the location of the project. Dr. Beveridge has not established this as true. It also assumes that all applicants will agree on which racial demographic typology they want to "choose" to live

2. Correlation and Causation

Dr. Beveridge's comments regarding causation[20] simply demonstrate that he does not understand the difference between correlation and causation and how it impacts either his analyses or my analysis.

Dr. Beveridge correctly states that the CP policy is a preference and the question is whether the benefits and harms of the CP policy are equally distributed among racial and ethnic groups. Here we agree.[21] We disagree in that he believes one can measure the benefits and harms of the CP policy by simply looking at the difference in results between those with and without the CP, while I believe that one must look at the difference in outcomes between what happens with the CP policy in effect and without the CP policy in effect. Dr. Beveridge believes that all "personal characteristics" of the applicants are the same, except for CP status, and thus improperly concludes that any differences in awards between the CP beneficiaries and non-CP beneficiaries is attributed to the CP policy.[22] Statistically, Dr. Beveridge relies on correlation, while I rely on causation.

Critically, Dr. Beveridge's method of determining the disparate impact of the CP policy is not statistically valid. If you look at Table 1 of the Beveridge October 18, 2019 Amended report,[23] you see that he compares the race distribution of awards among those who were selected from the

---

in if there were no community preference policy. Based upon the statistics provided in Section 1, *supra*, no such consensus exists.

[20] See Beveridge September 2019 report at ¶ 11.

[21] Dr. Beveridge argues in paragraphs 15 and 16 of his September 2019 report that, since I found that factors such as being African American and not in the community preference area make it less likely that apparently eligible African Americans will actually be eligible and interested, this makes it more important that the CP policy be eliminated. This is a policy issue, not a statistical issue, and I therefore offer no opinion. Regardless, one should not erroneously include impacts correlated with CP status and race but not caused by the CP policy when measuring the impact of the CP policy by race.

[22] See Beveridge September 2018 Report at ¶ 12.

[23] See also Table 8 of Dr. Beveridge's Amended October 2019 report.

CP list to the race distribution of awards of those not selected from the CP list. He attributes the increase or decrease in the representation of a race to the CP policy.

To show how this methodology can lead to an incorrect conclusion, let us apply his methodology to a fact pattern where there is no effect of the CP, and see if Dr. Beveridge's methodology correctly indicates that there is no racial effect of the CP policy. In Appendix B of my prior report, I presented a hypothetical situation in which there were exactly the same number of CP and non-CP applications whose log numbers and likelihood of being actually eligible and interested were perfectly independent of CP status (see Illustration 1). The illustration demonstrated that the same 10 CP area applicants and 10 non-CP area applicants would be selected irrespective of whether the CP policy was or was not in effect. In this case, the CP policy has no impact on who gets an award. Let us further assume that two out of every three applications in the CP area are white, and the other is African American, and two out of every three non-CP area applications are African American while the other is white, and the race and log numbers are perfectly independent. That is, two out of every three applications in the CP area that were awarded apartments will be white and one will be African American, while two out of every three non-CP area applications that were awarded apartments will be African American and one will be white.

Thus, the percent of awards to residents in the CP area by race is 66.7% (or 2/3) white and 33.3% (or 1/3) African American and the percent of non-CP area awards will be 33.3% (or 1/3) white and 66.7% (or 2/3) African American. Now, let us apply Dr. Beveridge's methodology and see if it correctly shows that the CP policy had no effect on selections overall or by race. To assess the impact on awards of the CP policy by race, Dr. Beveridge would compute the percent difference between in-CP awards and non-CP awards by race and attribute the difference to the

impact of the CP policy. In this case he would measure the impact on whites as (66.7%)/(33.3%)-100% or a 100% increase, and he would measure the impact on African Americans as (33.3%)/(66.7%)-100% or a 50% decrease. However, as established in this hypothetical, the CP policy actually had no impact in who was selected. Thus, Dr. Beveridge's analysis is statistically invalid in that it incorrectly measures the impact of the CP policy and, in this case, would lead to the incorrect conclusion that the CP policy significantly helped whites and hindered African Americans.

This shows that Dr. Beveridge's method does not properly measure the impact of the CP policy insofar as it picks up other effects, such as the large difference in the number of applications by race in this case. However, this does not mean that his method would not pick up any racial impact of the CP policy that might exist, but only that it would also pick up other effects that are correlated with CP status but not caused by CP status. To properly measure the racial impact of the CP policy, one must isolate the cause of the disparity resulting from the use of the CP. One cannot simply compare the racial mix of outcomes of those with the CP to the racial mix of outcomes of those without the CP because the CP policy impacts both outcomes and other factors that impact the outcome may also be correlated with CP status. That is what my discussion of causation explained, and what Dr. Beveridge fails to understand.

Failing to isolate the actual impact of the CP policy by comparing what occurs with the CP policy in effect with what would occur absent the CP policy confounds the actual impact caused by the CP policy with the impact that is correlated with CP status but is not necessarily caused by the CP policy. Dr. Beveridge's analysis based on correlation statistically assumes that if the CP policy did not exist, then living in the CP area (i.e., CP status) should not impact the likelihood of applications being awarded an apartment. To more clearly illustrate this problem using real data

instead of hypothetical data, let us analyze the apparently eligible application data presented in Exhibit 6 and the award data presented in Exhibit 7 of Dr. Beveridge's October 18, 2019 amended expert report Citywide, overall, and by CP and non-CP status.

If we look at the application data and the award data in Dr. Beveridge's Exhibits 6 and 7 in his Amended October 2019 report, we find that the distribution of awards by race among those with the CP does not mirror the distribution of applications by race. For instance, the overall selection rate of applications within the CP group is 2.74%, while the selection rates for whites within the CP group is 4.09% (representing a surplus of 196 white awards) and the selection rate for African Americans within the CP group is 2.16% (representing a shortfall of 388 African American awards). Since these African Americans and whites both have the CP, the differences in their selection rates as compared to the overall application rates cannot be attributed to the CP policy.

Similarly, when we look at those without the CP status who were awarded apartments, we find that the selection rates by race within the non-CP group differ and do not match the overall selection rates for the non-CP group. For instance, the overall percentage of selections among applications without the CP is 0.18% while the selection rates for whites without the CP is 0.23% (resulting in a surplus of 100 white awards), and the selection rate for African Americans without the CP is 0.17% (resulting in a shortfall of 111 African American awards). However, because you are comparing only applicants without the community preference, any differences between the selection rates by race cannot be attributed to the CP policy. That is, whatever the effect of the CP status is, since none of the applicants in the non-CP group have CP status, they must be equally impacted by the effect of not having CP status. Therefore, because we have established that both the CP and non-CP groups have racial disparities within the groups that cannot be attributed to CP

status, any difference between the two groups (CP v. non-CP) in selection rates by race will be confounded with the effects within the two groups.[24]

Dr. Beveridge's correlation studies cannot measure the racial impact caused by the CP policy because they do not account for factors other than the CP policy that impact outcomes by race. Therefore, Dr. Beveridge's analysis and conclusions, which conflate causation and correlation, are flawed and cannot be relied upon.

3. Stages of the Lottery

Dr. Beveridge states that my model of the lottery does not comport with the actual lottery process.[25] He notes that the first stage is applying and assigning random numbers, not being apparently eligible.[26] Dr. Beveridge's comments only demonstrate that he does not understand the purpose of what the stages represent.[27] They are not intended to replicate the actual lottery process,

[24] Having established the shortfalls/surpluses by race within each CP status group, we can better isolate the impact of the CP policy, although this will still only measure a correlation and not a causation, as it does not compare what would have occurred with and without the CP policy. Absent the policy, other factors might result in those within the CP area still being more likely to be given awards, as demonstrated by Table 2. To isolate the effect of CP status on the racial mix of awards when we compare the difference by race in selection rates of those with and without CP, we must subtract the racial shortfalls we found when we compared applications with the same CP status. To do this, we first adjust the awards by race among each CP status so that each race has the same selection rate and there is no shortfall or surplus by race among applications with the same CP status. We can then compare the number of selections by race to what would occur if the overall selection rate of those with and without the CP was the same. That is, we can compare the actual number of awards to each race to the adjusted number of awards to each race if the selection rates among all applications were the same for each race, regardless of CP policy. The difference in awards by race would then be attributed to the CP policy. Table SR1, set forth in Appendix H shows the results of that analysis. As SR1 shows, there is no disparate impact against any race, even when following Dr. Beveridge's flawed analytical approach to compare awards with the CP to awards without the CP. In fact, African Americans and whites have essentially the same adjusted selection rates, .34% for whites and .33% for African Americans.

[25] See Beveridge September 2019 report at ¶ 17.

[26] See Beveridge September 2019 report at ¶¶ 17, 19 and 20.

[27] Dr. Beveridge's confusion is surprising, since he also separately studies the apparently eligible applications.

but to structure the process into an analytical framework which allows one to isolate the impact of the CP policy on selection by race, which is the objective of my analysis.

To understand the difference, consider my analytical stages. Stage 1 separates applicants by whether or not they are apparently eligible. I created Stage 1, which removes applications that are apparently ineligible from the analysis, to isolate the effect of the CP policy by race on the ability to compete for housing and awards.

Stage 2 is restricted to those applications that have been deemed apparently eligible, and focuses on those whose log numbers were reached and would have been awarded an apartment if they were found actually eligible and interested (in other words, were apparently eligible and there was a unit available at the time their log number was reached, which I call the "Consideration Stage"). This allows us to focus on the racial impact of the decision of who can compete for an apartment. It is only in this selection process that the CP policy plays a critical role. Therefore, Stage 2 is designed to allow the analysis to focus on assessing whether the part of the process that the community preference policy directly impacts has a racial impact.

Finally, Stage 3 refers to the process of confirming an application's actual eligibility and interest after it has been reached and would be awarded an apartment if it is actually eligible and interested. The CP policy is not relevant to the process of determination of eligibility or interest in Stage 3.

Both Dr. Beveridge's and my definition of apparently eligible refer to whether an application is apparently eligible for any apartment type initially available (that is, when all possible apartments are available).[28] Hence, someone apparently ineligible would never be

[28] In fact, it is my understanding that this method is consistent with the way apparent eligibility is undertaken during the lease up. Thus, whether a developer codes the entire log in the first instance to determine apparent eligibility or assess it in batches as they work their way down the log, there is no difference in the outcome. Each apparently eligible application is then subsequently reached by the developer in log/preference order

considered for any unit, and could not be awarded an apartment even if they had the best lottery number and all the preferences, unless they succeed in an appeal to that decision.[29] Therefore, someone who is apparently ineligible would not get an apartment, regardless of whether they have CP. For analytical purposes, such applications must be removed when assessing the impact of the CP, so as not to confound the effect of not being apparently eligible with the impact of the CP policy.

To the extent my stages do not exactly mirror the actual process, they accurately reflect the decision process in a way which allows one to best analyze the impact of the CP policy. The alleged differences between the actual process and these analytical stages does not affect the validity of my analysis, and Dr. Beveridge fails to demonstrate otherwise.

Dr. Beveridge argues that there is a higher percentage of CP applications that are reached and considered by developers than non-CP applications. While that may be true, the relevant question is then whether that results in a difference by race in who gets considered by a developer. My analysis in Table SR2 demonstrates that of the apparently eligible applications, there is no meaningful difference by race in who is considered when the CP policy is in effect.

Moreover, to the extent Dr. Beveridge is claiming that the use of my stages results in ignoring the potential impacts of the CP policy on those found apparently ineligible, as I previously explained in my September 4, 2019 Amended Report, Dr. Beveridge does no analysis as to the impact of the CP by race on apparently ineligible applications. Further, given my finding that the likelihood of an apparently eligible household being considered was not different by race, there is no reason to believe that there would be a difference by race in apparently ineligible

for further evaluation of eligibility at that time, including whether an apartment is still available (which is what I call the Consideration Stage).

[29] In my analysis, I separately address the racial impact of the CP policy on this unlikely event.

applications that are not reached by a developer as a result of the CP policy. In fact, as set forth below in Table SR2, there is very little difference in the racial makeup of the CP applications and non-preference applications for apparently ineligible applications. This gives further credence to the conclusion that there is no meaningful racial impact of the CP policy on those who are apparently ineligible.

Table SR2
COMPARISON OF RACIAL DISTRIBUTION
OF APPLICATIONS WITH CP AND WITH NO PREFERENCES

Apparently Eligible Applications Only

	Percent		Shortfall/(Surplus)
	Have CP (No MB/HV)	No MB/HV/CP/ME Preferences	in CP as Percent of Applications
White	8.6%	8.2%	(0.30%)
African American	39.7%	36.6%	(0.52%)
Hispanic	35.3%	37.0%	0.28%
Asian	4.1%	6.2%	2.11%

Apparently Ineligible Applications Only

	Percent		Shortfall/(Surplus)
	Have CP (No MB/HV)	No MB/HV/CP/ME Preferences	in CP as Percent of Applications
White	7.0%	7.3%	0.17%
African American	40.8%	38.0%	(0.38%)
Hispanic	37.6%	38.0%	0.06%
Asian	3.4%	5.5%	1.99%

Amended December 12, 2019

4. The Lottery Simulation

In Dr. Beveridge's September 19, 2019 reply, subsequently amended on October 27, 2019[30], Dr. Beveridge incorrectly used my simulation results (reported in my September 4, 2019 amended report). He uses the results of the simulation when the CP policy is in effect to "measure" the impact of the CP policy, and to show that these results are similar to his findings in Table 8 of his original report. However, neither Dr. Beveridge's Table 1 in his October 27, 2019 amended reply report, nor his Table 8 in his April 1, 2019 report properly demonstrate the racial impact caused by the CP policy, as discussed in depth *supra* and further illustrated in Table SR1.[31] In short, the problem is that his correlation analysis focused on a comparison of CP beneficiaries and non-CP beneficiaries within a lottery in which the CP is in effect, confounds the racial impact of the CP policy with the racial impact of other factors.

The simulation, on the other hand, precisely isolates the impact by race on selection caused by the CP policy by measuring the number of selections by race that occur with and without the CP in effect.[32] Dr. Beveridge's analysis (see Table 1 in his October 27, 2019 amended reply report) of the simulation data excludes simulation results from when the CP policy was not in effect.

[30] Dr. Beveridge's September 19, 2019 reply report had incorrect data set forth in Exhibits 3 and 4, which was then relied upon in his Table 1. This issue was raised during his deposition and, subsequently, Dr. Beveridge amended his reply report to correct this issue.

[31] These errors are in addition to the broader error in undertaking disparate impact analysis by CD typology.

[32] Assuming that the race of those considered and whether they are actually eligible and interested are independent.

5. Consideration

Dr. Beveridge correctly notes that both sides agree that the data derived about consideration of applications is certainly not perfect.[33] However, a key issue in this matter is whether apparently eligible applications of one race are less likely than apparently eligible applications of another race to be able to compete for an apartment as a result of the CP policy. Here, passing the Consideration Stage (Stage 2) allows an application to compete for housing. The Consideration Stage is also important to study because the CP policy plays a significant role in the selection process which occurs at this stage, or in other words, the CP policy plays a significant role in the selection of applications that are "considered" and thus able to compete for housing. The CP policy has no role in determining who is apparently eligible (Stage 1), nor in determining if the application is actually eligible and interested once it is considered (Stage 3).

Thus, while perhaps imperfect, assessing the racial impact of who is considered is an appropriate analysis to permit an inference of whether the CP policy has a disparate impact by race in the ability to compete for housing. The critical part of the determination is the racial mix of those considered, not the number of those considered. Dr. Beveridge never looks at the impact on the racial mix of estimating the population of "Considered Applications". If he had, he would have seen that the racial mix does not change significantly as one refines the estimate of those considered and, hence, the conclusion based on studying Considered Applications does not change as the elusive exact number of Considered Applications is estimated.

Dr. Beveridge points out that I amended my initial analysis and the number considered fell by almost half.[34] Then, when he partially corrected my computation, the number considered

[33] See Beveridge September 2019 report at ¶ 35.

[34] See Beveridge September 2019 report at ¶ 48.

dropped again by approximately twenty percent. He called this "partially corrected" because he noted other errors but did not correct for them. He also noted that the "failure to follow the rules" would result in overstating those competing, and also most likely overstate those who had no preferences. He further noted that it would be very difficult to accurately correct the data to take into account how or when the rules were not followed.

These errors and other errors will inflate the number of applications determined to be considered. However, estimating the number of applications considered is not the issue here: evaluating the racial mix of those considered is the purpose of the analysis. It is interesting that Dr. Beveridge does not compare the racial mix of the number of Considered Applications used in my original report with the racial mix of the number of Considered Applications in my amended estimate or his partially corrected number. Instead, he notes that "the error types illustrated in paragraphs 51-62 remain to be corrected, and thus even the partially corrected identification and count of 'considered' applicants is still polluted in a way that exaggerates the numbers and improperly skews the relative shares of 'considered but not selected,' *artificially reducing the CP beneficiary share and artificially inflating the non-beneficiary share*" (emphasis added).[35] Again, Dr. Beveridge focuses on CP beneficiaries compared to non-CP beneficiaries, even though the issue is the race of those considered, not the CP status of those considered and not considered. There are only small differences between the racial mix of those with the CP (whom Dr. Beverage claims are underestimated in the consideration population) and those without any preference.[36] Hence, there is little reason to expect that refining the estimate is going to alter any conclusion concerning race.

[35] See Beveridge September 2019 report at ¶ 63.

[36] See Table SR2.

Nevertheless, I have recomputed those considered, correcting for all the errors that Dr. Beveridge points out (see Table 4 in my Expert Report amended on December 13, 2019), and have confirmed that despite the fact that the numbers have changed, my conclusions have not changed. Table 4 shows that despite the change in the number of Considered Applications, the white, African American and Hispanic consideration rates are very similar (12.80%, 12.54% and 12.30, respectively) and all the races pass the 80% rule. Moreover, the surplus/shortfalls in awards[37] caused by the consideration process is actually smaller. Thus, even based upon the corrected "considered" numbers, the Consideration Stage, of which the implementation of the CP policy is part, has no disparate impact.

Additionally, since Dr. Beveridge also raises the issue of potential errors that occur when the rules are not followed by certain developers[38] (errors which I cannot correct since it is not clear what they are), I also conducted a sensitivity study.[39]

The sensitivity study attempts to minimize errors which might occur because of data errors or the rules not being followed by developers.[40] To the extent that these are not data errors or

[37] The awards are based on the assumption that the distribution of awards by race among the considered population equals the racial distribution of those considered (this isolates the consideration effect and eliminates any racial impact of the confirmation process) and is compared to what the racial distribution of the awards would be if it mirrored the racial distribution of all apparently eligible applications.

[38] Which Dr. Beveridge discusses in paragraph 49 of his September 2019 report.

[39] This study would also account for any errors in the creation of Dr. Beveridge's database on which we relied (albeit with some correction of errors that we identified and could correct).

[40] In order to try to eliminate the impact of data errors or the rules not being followed by developers, I eliminated from the computation of which applications are considered any last selection from the CP list or the no preference list which appears to be an aberration. Specifically, I flagged the highest log number awarded which had a log number 5,000 above the last award. I removed this award from the computations as long as the 5,000 jumps in log numbers from the last award was at least twice that of any prior jump between awards. This excluded 97,372 applications. To be conservative and not include applications on the NP list as considered, I also eliminated any selection from the NP list that represented the maximum log number that had any preference. Thus, the last selection from the NP list was always assumed to be someone without any preference. This excluded 42,328 applications.

selections that do not follow the rules, this sensitivity study tends to artificially increase the CP beneficiary share and artificially decrease the non-beneficiary share among those considered.[41]

Table SR3 represents the disparate impact analysis results based upon the sensitivity study. Table SR3, which presents the findings based upon the sensitivity data, also shows that all the races pass the 80% rule and no disparate impact on African Americans or Hispanics. Moreover, the sensitivity study results are an aggressive (i.e., more likely to underestimate the actual number considered) estimate of the extent to which the definition of which applications are considered affect the AIR and impact on the awards by race. Hence, we can reasonably conclude that if the actual errors in the data could be corrected, and we could identify when the rules were not followed, the most accurate estimate of racial impact would probably fall between the results presented in Table 4 in my Amended December 13, 2019 Expert Report and the results of the sensitivity study in Table SR3 below. These results would show no disparate impact.

TABLE SR3
RACIAL/ETHNIC DISPARATE IMPACT OF CONSIDERATION PROCESS STAGE 2
ON APPARENTLY ELIGIBLE APPLICATIONS
CONSIDERED POPULATION BASED ON SENSITIVITY ANALYSIS SISKIN SUR-REPLY

	Asian	African American	Hispanic	Other	White	Total Known Race
Number of Apparently Eligible Applications	181,053	1,180,915	1,132,704	194,719	241,932	2,931,323
Number Considered	13,236	92,304	88,590	16,101	21,596	231,827
Consideration Rate	7.31%	7.82%	7.82%	8.27%	8.93%	7.91%
AIR	81.90%	87.56%	87.62%	92.63%		
Difference in Actual - Expected Consideration Rate	-0.60%	-0.09%	-0.09%	0.36%	1.02%	
Surplus (-Shortfall) Awards	(44)	(45)	(41)	29	101	

Amended December 12, 2019

[41] This is because the maximum number not used was most often from the NP list and thus the changes disproportionately eliminated NP applications as being considered.

Table SR4 compares the resultant AIRs and award shortfalls (1) in my original analysis, (2) in my amended September 4, 2019 amended Expert Report, (3) based on Dr. Beveridge's partial correction analysis presented in his September 19,2019 Reply Report, (4) in my amended December 13, 2019 Expert Report, and (5) in the results of the sensitivity analysis (Table SR3). Table SR4 shows that the impact on the awards by race, and the AIRs do not change significantly as corrections are made to which applications are considered, as the AIRs are over 80% in all cases. Table SR4 further demonstrates that, as the estimate of those considered is more refined, the resultant total shortfall in awards tends to decline. Table 4 in my initial Expert Report showed that changing the race of 199 of the 10,245 awards would result in perfect racial parity. That computation was based on defining the considered population as 1,059,039 applications. Based on Table 4 in my amended December 13, 2019 Expert Report which redefined the considered population as only 387,679 applications, changing only 88 out of the 10,245 awards would result in perfect parity. However, the consideration rate for whites relative to other races increased and, as a result, the 80% Rule shows the consideration rates trended towards equality by race. Moreover, Table S3 (my sensitivity analysis) shows that conservatively reducing the number considered by another approximately 100,000 applications (primarily from the no preference list) while continuing to increase the white consideration rate relative to other races reveals little racial difference in that changing only 130 out of the 10,245 awards would result in perfect parity, and all races pass the 80% Rule.

Therefore, despite the errors identified by Dr. Beveridge in the calculation of the number of Considered Applications, while the number of applications determined as considered changes significantly, there has only been a small change in the racial mix of who is considered, and thus

my overall conclusion that the Consideration Stage does not have a disparate impact against African Americans, as shown by the AIR or difference in awards with racial parity, is still valid.

TABLE SR4
COMPARISON OF ANALYSIS FINDINGS BASED ON DIFFERENT DETERMINATIONS OF WHICH APPLICATIONS WERE CONSIDERED

		80% Rule (AIR)				Award Surplus as Result of Consideration Process				
	Estimated Number Considered	African American	Hispanic	Asian	Other Races	White	African American	Hispanic	Asian	Other Races
Siskin Report June 27,2019	1,059,039	112.88%	104.17%	101.36%	108.52%	(54)	193	(112)	(33)	6
Siskin Amended Report September 4, 2019	551,668	120.38%	110.43%	93.62%	113.76%	(88)	258	(76)	(99)	6
Based on Beveridge Partially Corrected Considered	429,266	107.26%	104.00%	89.42%	105.93%	(32)	112	(7)	(83)	10
Fully Corrected Siskin Amended December 13, 2019	387,679	97.97%	96.09%	86.80%	100.16%	25	43	(29)	(61)	22
Sensitivity Analysis Sur-Reply Amended December 13, 2019	247,979	87.56%	87.62%	81.90%	92.63%	101	(45)	(41)	(44)	29

Note: ( ) = Shortfall

Amended December 12, 2019

6. Regression Study of Factors Impacting Outcome at Confirmation Stage

Dr. Beveridge claims that my regression cannot be sound and has a problem of multicollinearity, because the regression depends on the accuracy of the determination of those considered but not selected.[42] With respect to the issue of multicollinearity, as I explained in my deposition,[43] this problem did not affect the estimates of the effect of race or preferences on the likelihood of someone who was considered being found actually eligible and interested. Furthermore, if one employs the common statistical approaches for forcing convergence and eliminating the problem of multicollinearity, the impact of CP status (not the CP policy) would

[42] See Beveridge September 2019 report at ¶ 66.

[43] See pages 239-242 of my deposition dated August 6, 2019.

increase.[44] Also, if Dr. Beveridge's argument that my regression cannot be used since it is difficult to accurately determine who is considered were the standard applied to all statistical studies, most statistical analyses would be deemed unacceptable. Most statistical analyses do not consider all variables, and almost every large database has some degree of error. This issue has been recognized by the Courts, which have nevertheless accepted the use of statistics as evidence despite the failure to address (or take into account) all factors and the inevitable error of measurement.[45] From a statistical and legal point of view, the issue is whether the unaddressed factors are actually important and significantly correlated with the factor(s) of interest whose effect is being estimated, which in this case are race and preference. Absent evidence that the unconsidered factors are important and correlated with the factor of interest whose effect is being estimated, there is no reason *not* to rely on the statistical findings. With respect to errors in measurement, if the errors are random with respect to the factors being analyzed, the errors would not bias the results and, in fact, would be expected to underestimate the impact. As demonstrated below, as the definition of considered was refined, the conclusions from the regression results remained the same. Thus, Dr. Beveridge's critique of my regression is meaningless.

Dr. Beveridge further opines that my regression has no bearing on the issue of the impact of the CP policy.[46] He is correct, but fails to understand that the whole purpose of this analysis was to show that race and preferences had an impact on the outcome of the Confirmation Stage (Stage 3), which is the stage at which the CP policy has no role. Given that CP status and race

[44] See Bernard Siskin August 26, 2019 deposition transcript ("August 26th Transcript") at pages 241 line 14 through page 242 line 16. Copies of all pages cited to in this report from my August 6th deposition transcript are collectively annexed hereto as Appendix J.

[45] See *Bazemore v. Friday,* 478 U.S. 385 (1986); *Palmer v. Schultz,* 815 F., 2d 84 (D.C. Cir.1987); *Sobel v. Yeshiva,* 839 F. 2d 18 (2nd Cir. 1988).

[46] See Beveridge September 2019 report at ¶ 69.

impact the awards independent of the CP policy, then studies which simply compare awards of those with and without the CP status confound and inflate the actual impact of the CP policy.

Dr. Beveridge correctly points out that I defined the apartments for which an application is apparently eligible as of the beginning of the lottery process, but there may be fewer apartments available for which an applicant is apparently eligible as of the time when they are actually considered, since certain apartments types that were originally available to them may have been already awarded (or what Dr. Beveridge calls partially closed out).[47] Moreover, this is likely to have occurred to those reached later in the lottery process, so it is more likely to impact those with no preference.[48] Although Dr. Beveridge is correct in this assessment, his adjustment for it is incorrect.

Dr. Beveridge creates a variable called prop-left that he added to his re-run of my regression. The variable is the number of apartments already awarded when someone was considered. This variable, however, fails to consider the actual apartments for which the applicant was initially apparently eligible. Consider a case in which the last application considered was awarded a one-bedroom unit which was the only unit for which that application was initially apparently eligible. Dr. Beveridge would assign the application the lowest prop-left value, when in fact it was not actually partially closed out because the only unit size that it was apparently eligible for was still available. Similarly, an application selected much earlier which would have a higher prop-left value may actually have been partially closed out because one of the unit sizes the application had been apparently eligible for was already filled. Rather than creating the "prop-

[47] See Beveridge September 2019 report at ¶¶69-74.

[48] See Beveridge September 2019 report at ¶ 75.

left" variable, the proper way to address the problem of partial close outs is simply to compute the apartments for which an application was apparently eligible when *actually considered*, rather than at the beginning of the lottery. In addition, although this adjusts for the types of apartments for which an application was actually apparently eligible when considered, it does not fully capture the impact of being partially closed out.

Consider a situation in which we have two applicants at the beginning of the lottery. One applicant is apparently eligible for a one bedroom and a studio, while the other is eligible for only a studio. However, both are only eligible for a studio when considered, since the first applicant was partially closed out (the one bedroom was already awarded to someone else). Looking at "when considered," the two would be considered identical. However, the applicant who was partially closed out may have really wanted a one bedroom, as Dr. Beveridge noted in his deposition[49] and hence, that applicant may be less likely to be interested in the studio unit than the applicant who knew they were only eligible for a studio when they applied. Therefore, the fact that someone was partially closed out of some unit could reduce the likelihood of being interested in the type of unit(s) for which they remain apparently eligible. Thus, in addition to recalculating the apartments for which the application is apparently eligible when considered, I added a variable to note that they were partially closed out of some unit(s).

Table 2 in my Amended December 13, 2019 Expert Report shows the results of a re-run of my original Table 2 with the corrected considered population. I then further modified the analysis by defining apparently eligible "when considered" and added a variable for partially closed out. Based upon this model, I re-ran the regression for the following two different estimates of the considered populations: my estimate after correcting for the errors noted by Dr. Beveridge in his

[49] See October 4th Transcript, page 21 through page 22, line 1.

rebuttal report (see Table SR5), and the estimate aggressively accounting for possible data errors and variations from the selection rules (the sensitivity study estimate) (see Table SR6).

Table 2 in my Amended December 13, 2019 Expert Report and Tables SR5 and SR6 below, show that, although the relative impact of having the CP on the likelihood of following through and being awarded an apartment is lower,[50] the conclusion that CP status has a significant impact on the likelihood that a considered application will be found actually eligible and interested remain unchanged. The adjustment for partially considered changes the adverse relative effect of being African American from non-statistically significant to statistically significant (although small). However, CP status is still clearly the most significant factor.

[50] The measures of impact presented in Tables 2, SR5 and SR6 are relative measures. That is, they measure the impact of the factors on the likelihood of being awarded a unit for someone who was considered versus the average applicant. Thus, if the number of applications considered is reduced, the average likelihood of getting an award increases, since the odds of someone getting an award overall is equal to the number of awards (which is constant) divided by the number of applications considered. Hence, the relative impact of any factor will decrease simply because the considered population is increased, even when the actual impact of the factor on a considered application's likelihood of following through and getting an award is unchanged.

TABLE SR5
IMPACT ON PREDICTION THAT A CONSIDERED APPLICATION WILL BE FOUND INTERESTED AND QUALIFIED

387,679 Applications Estimated as Considered (Best Estimate)

Factor	Increase/Decrease in Probability of Passing Stage 3*	Change in Units of Standard Deviation	Statistically Significant?
Race (Compared to White)			
African American	-3.00%	2.03	YES
Hispanic	0.71%	0.47	No
Asian	6.32%	2.84	YES
Preference			
MB	-8.42%	5.99	YES
HV	-9.94%	5.28	YES
CP	76.27%	45.84	YES
ME	-3.62%	3.28	YES
Whether Partially Closed Out	-15.32%	16.57	YES

Notes

* = Provides valid evidence that Considered Applications are eligible and interested in unit.

Controls for project and type of apartments eligible for when considered and whether partially closed out.

Amended December 12, 2019

TABLE SR6
IMPACT ON PREDICTION THAT A CONSIDERED APPLICATION WILL BE FOUND INTERESTED AND QUALIFIED

247,979 Applications Estimated as Considered (Sensitive Estimate)

Factor	Increase/Decrease in Probability of Passing Stage 3*	Change in Units of Standard Deviation	Statistically Significant?
Race (Compared to White)			
African American	-2.36%	2.53	YES
Hispanic	-0.06%	0.00	No
Asian	4.09%	2.88	YES
Preference			
MB	-5.63%	6.27	YES
HV	-8.16%	7.08	YES
CP	34.36%	36.85	YES
ME	-2.09%	2.94	YES
Whether Partially Closed Out	-5.91%	8.98	YES

Notes

* = Provides valid evidence that Considered Applications are eligible and interested in unit.

Controls for project and type of units eligible for when considered and whether partially closed out.

Amended December 12, 2019

Dr. Beveridge seems to be arguing that the impact of the CP policy is not dependent on the characteristics of applicants, except for their CP status.[51] Thus, the results of my regression (which shows the impact of awards on persons considered by race and preference statuses) should not be relevant in assessing the impact of the CP policy. If you correctly measure the impact of the consideration process (see my Tables 4 and 6 in my amended December 13, 2019 Expert Report) or correctly assess the impact of the CP Policy through my simulation (see my Table 3 and Table 7 of my amended December 13, 2019 Expert Report which are based on correlation not causation), he is correct. However, you do have to control for these factors if you are doing a correlation study, as Dr. Beveridge does, in which he compares the difference in awards by race between those with and without CP status. As discussed in Section 2 *supra* and illustrated in Table SR1, at a minimum[52] one must control for the race differences among persons with the same CP status, but Dr. Beveridge fails to do so. My regression clearly shows that these differences do exist.

7. Perpetuation of Segregation

As discussed in Section 5 above, I refined my estimate of the population of applications considered. I refer to one estimate as my best estimate of those considered and I refer to the other estimate (which attempts to minimize the impact of possible data errors and the impact of the rules not being followed) as my sensitivity estimate. Table 6 in my amended December 13, 2019 Expert Report uses my best estimate of the considered population,[53] and Table SR7 uses my sensitivity estimate.

---

[51] See Beveridge September 2019 report at ¶ 68.

[52] I say minimum because the regression clearly also illustrates if the CP policy were removed those with CP will still be more likely to get awards because of their "sticktoitiveness". See Beveridge September 2019 report at ¶ 68.

[53] Table 6 also corrects for all other programing errors.

TABLE SR7
ANALYSIS OF LOTTERY AWARDS OVERALL AND THE IMPACT OF THE CONFIRMATION AND SELECTION POLICIES ON SEGREGATION IN CITY
SENSITIVITY STUDY

Impact of Actual Awards (Stage 1, Stage 2, and Stage 3)									
	Awards by Effect on Segregation								
Races	Total	Segregate	No Effect	Integrate	Net Effect Seg-Int	Numerical Change in DIS Index	Direction of Effect DIS	Direction of Effect of Stage on DIS	Numerical Change in DIS Due to Stage
W vs. AA	8,224	203	7,435	586	-383	-0.00055	Lowers		
W vs. A	8,224	227	7,599	398	-171	-0.00015	Lowers		
W vs. H	8,224	312	7,136	776	-464	-0.00063	Lowers		
AA vs. H	8,224	697	6,389	1,138	-441	-0.00060	Lowers		
AA vs. A	8,224	176	7,493	555	-379	-0.00055	Lowers		
H vs. A	8,224	237	7,339	648	-411	-0.00048	Lowers		
Impact of Awards if No Impact of Confirmation Process (Stage 1 and Stage 2)								Impact of Stage 3	
W vs. AA	8,224	240	6,958	1,026	-786	-0.00106	Lowers	Increases	0.00052
W vs. A	8,224	183	7,687	354	-171	-0.00035	Lowers	Increases	0.00019
W vs. H	8,224	356	6,856	1,013	-657	-0.00081	Lowers	Increases	0.00018
AA vs. H	8,224	799	5,676	1,749	-950	-0.00132	Lowers	Increases	0.00072
AA vs. A	8,224	207	7,070	948	-741	-0.00121	Lowers	Increases	0.00066
H vs. A	8,224	296	7,080	848	-552	-0.00080	Lowers	Increases	0.00032
Impact of Awards if no Impact of Selection Process and Confirmation Process								Impact of Stage 2	
W vs. AA	8,224	243	6,547	1,434	-1,191	-0.00167	Lowers	Increases	0.00061
W vs. A	8,224	180	7,652	392	-212	-0.00058	Lowers	Increases	0.00023
W vs. H	8,224	364	6,482	1,377	-1,013	-0.00129	Lowers	Increases	0.00048
AA vs. H	8,224	886	5,272	2,066	-1,180	-0.00166	Lowers	Increases	0.00034
AA vs. A	8,224	211	6,650	1,363	-1,152	-0.00182	Lowers	Increases	0.00061
H vs. A	8,224	335	6,683	1,206	-871	-0.00122	Lowers	Increases	0.00042

Amended December 12, 2019

The results presented in Tables 6, and Table SR7 are almost identical. As with the disparate impact results, changing the number of Considered Applications did not change the perpetuation of segregation results. The lottery with the community preference policy still has an integrative effect, and although the Consideration Stage reduces the amount of integration, that reduction is trivial and meaningless. With respect to isolating the impact of the CP policy through the simulation, Table 7 in my amended December 13, 2019 Expert Report shows a similar pattern. The effect of the CP policy is to trivially reduce the level of integration for all pairs of races, with the impact of eliminating the CP policy measured in the 4th decimal place.

Dr. Beveridge incorrectly argues that I exaggerate the number of selections[54] that have no effect on segregation by including in the two-group comparison the selection of persons of other races.[55] This is a bogus argument. My analysis measures the impact on each of the dissimilarity indices of segregation due to awarding apartments. If we are concerned with African American and white segregation, then selection of those who are neither African American nor white *cannot* impact the index. Hence, if I am interested in the effect of filling 1,000 apartments and 500 are awarded to Hispanic applications, these 500 Hispanic selections do not impact the index. The question of how filling the 1,000 apartments impacts segregation must include these decisions.

Dr. Beveridge next points out that more apartments will be filled over time, and therefore there will be more change in the dissimilarity indices.[56] While this is true, the nature of that actual change is not so obvious. New apartments will be developed over time and, if the pattern going forward is like the pattern studied, then the impact would increase approximately[57] proportionately. That is, if we assume that 10 times more apartments will be filled over the next 30 years (so 104,250 new apartments will be filled via the lottery process) it would be reasonable to assume that both the integrative effect of the actual awards and the slight reduction in this effect

[54] My simulations assume that the projects and the location of the projects which are being filled would not change if the CP policy were eliminated. If the ability of the City to attract developers to build affordable housing projects, or the location of such projects is altered if the CP policy is eliminated, this would alter the simulation results. This would alter the conclusion concerning the impact of the CP on the dissimilarity index. If the number of projects were drastically decreased, the overall effect of the awards that reduce the dissimilarity index would be lessened and, hence, eliminating the CP would increase rather than decrease the level of segregation in the City.

[55] See Beveridge September 2019 report at ¶ 79.

[56] See Beveridge September 2019 report at ¶ 82.

[57] The base population would change over time, so the impact of moving a household from an area where they are in the majority to one where they are in the minority would change somewhat.

because of the CP policy would be about 10 times larger. However, this would nevertheless still be trivial, as it would be observable at only the 3rd decimal place.

Dr. Beveridge further argues that I do not compare the effect on the dissimilarity index of the CP awards with the effect of the non-CP awards, and instead I only report the "net effect" of the selection or CP policy.[58] I do not compare the impact on the dissimilarity indices of those selected from the CP list with the impact of those not selected from the CP list, as that does not measure the impact of the CP. In Section 2 of this report I discussed in depth the statistical flaw in Dr. Beveridge's approach, which confuses correlation studies with causation studies. I compare the difference in the effect on the dissimilarity index of the awards that occur with the CP policy in effect versus the effect of the awards without the CP policy in effect. The CP policy affects which of the CP beneficiaries and non-CP beneficiaries will be selected. If the CP policy is removed, the selection of those living in the CP areas and those living outside the CP area will change. The question is how these changes impact the dissimilarity index, which is exactly what the simulation measures. It is irrelevant whether the changes in the selection of those living within the CP area or those living outside the CP area are causing the change in the index.[59]

---

[58] See Beveridge September 2019 report at ¶ 84.

[59] Dr. Beveridge attached a new study (Beveridge Table 6-Sections 1 through 3) to his November 1, 2019 errata sheet to his October 4, 2019 deposition transcript and issued a new opinion about those tables in the errata sheet. While I am advised that what Dr. Beveridge did is procedurally improper, I will nevertheless respond to that new study. In that new study, Dr. Beveridge attempts to break down my perpetuation of segregation analysis (Table 6 in my December 13, 2019 Amended Report) by comparing the impacts on segregation of those with the CP and those without the CP. The actual impact of the CP policy on the Dissimilarity Index is conservatively estimated by the simulation study in my amended December 13, 2019 report (see Table 7). Nevertheless, even if it were appropriate, *arguendo,* to use the outcome data with the CP policy in effect to estimate the results caused by the CP policy, Dr. Beveridge's new study is incorrectly done and misleading. Dr. Beveridge's calculation of the net percentages and relative percentages is incorrect, and thereby greatly inflates the percentages and relative percentages. Specifically, Dr. Beveridge excludes the "not in group" units when calculating his percentages. This fails to capture the complete set of units that could integrate or segregate, and results in a different number of possible units for each race pair. It also treats the "no effect" group differently than the "not in group" group. Neither of these two groups have an impact on the dissimilarity index. However, by excluding the "not in group" from the total

Dr. Beveridge also argues that it does not matter how much or how little an index of segregation moves as a result of the lottery process studied. He asserts that what matters is whether it is "reasonably predictable that the development would be tenanted in a less segregated way than would be the case with the challenged policy."[60] This concept of a measure of segregation makes no sense in this case. First, Dr. Beveridge never tells us how to measure segregation in a development. Is a project that has all African American tenants as segregated as a project with all white tenants? Is a project that is 60% African American and 40% white as segregated as a project that is 40% African American and 60% white? If the question of perpetuation of segregation is determined by who is "tenanted" in a development, the neighborhood that development is located in would appear irrelevant, which is inconsistent with Dr. Beveridge's CD typology approach and is also inconsistent with measuring segregation in a City. If all one cares about is the tenanting of the project, then it does not matter where a tenant comes from. However, measuring the impact of segregation in the City fully depends on the racial demographics of where the person moves from.

number of units that could be impacted, he is falsely implying that there is some difference between the impacts of the "no effect" group and "not in group" group. He is also making the percentages much larger than they should be. Done properly, the net effect percentages are significantly smaller and are consistent with my simulation results which show a trivial and meaningless difference in the extent of integration with and without the policy. Even accepting his inflated relative impact percentages, his conclusion that the CP policy had a significant impact on the degree of integration is misleading. As shown in Table 6 in my amended December 13, 2019 Expert Report, the overall effect of the lottery on the white and African American Dissimilarity Index was to integrate by a trivial amount (a 0.00055 decrease in the Dissimilarity Index). This was based on the difference between the number of applications which segregate minus the number that integrate ("net seg-int" of -383). Dr. Beveridge's analysis finds that the difference in the net seg-int effect of those without the CP was 147 percent larger than the net seg-int of the total population. Dr. Beveridge incorrectly implies that if the CP policy were not in effect, the net seg-int effect would mirror that of the non-CP population so that net sig-int would be 147% larger than the impact on integration. Thus, according to Dr. Beveridge, if the CP policy were eliminated, the integrative effect would be increased by 147%. This would result in the Dissimilarity Index being reduced by an additional .00026, resulting in the lottery having an integrative effect of reducing the Dissimilarity Index by 0.00081, which would still be a trivial effect. In sum, a large percentage increase of a trivial effect remains a trivial effect.

[60] See Beveridge September 2019 report at ¶ 81. Interestingly, here he properly frames the issue as comparing the impact with the policy in effect to the impact without the policy in effect, and not comparing the effect of CP beneficiaries versus non-CP beneficiaries.

Moreover, Dr. Beveridge did not even attempt to do any analysis to support his "measure" of segregation.

Dr. Beveridge further points out that the impact of the lottery process on the dissimilarity index is constrained by the small fraction of housing apartments it impacts, and further by the fact that the index is impacted only by selections related to those two races, but the lottery selection is not restricted to selecting only the two races.[61] In other words, considering the limitation of the number of selections and the fact that selections are not restricted to only the two races, is the relative impact non-trivial?[62] To address Dr. Beveridge's concern and determine the relative impact I created a relative impact index. I first assumed that the selections of the two races from the lottery would be constrained to their representation among apparently eligible applicants.[63] I then studied the impact on the index if application selections were limited to only those that would increase integration or, conversely, what would happen to the index if application selections were limited to only those that would increase segregation. This yields the range of effects on the index that is possible given the number of selections in the lottery and the fact that only decisions concerning the two races can impact the index.

[61] See Beveridge September 2019 report at ¶ 81.

[62] The question is whether the relative or actual effect is appropriate for assessing the impact of a process or decision. For example, consider the following hypothetical. Your salary is $50,000 a year. The company you work for announces that it will give raises in the range of 0 to 0.4%. You are given a 0.3% or $15 a year raise. In relative terms, since the range of possible raises was 0 to 0.4%, your raise was relatively large (75% of what you could be awarded). However, your actual raise was only 0.3% or $15 for the year. One has to consider whether the fact that the relative value of the raise was substantial, or whether the fact that the dollar value of the raise was trivial is a better measure of the impact of the raise process. This hypothetical relative increase of 75 percent is greater than the very small actual relative effects of less than 5 percent I find in this matter. Nevertheless, even in this hypothetical, the logical conclusion is that the actual dollar value of the raise is the more meaningful measure of the impact of the raise policy.

[63] That is, since 51% of the apparently eligible applicants that were race identified were African American or white, 51% of the selections will be African American or white.

I then can place these results on a relative impact index in which an outcome of 1 means maximum possible segregation impact, and an outcome of 0 means maximum possible integration impact. The larger the value of the relative index, the more it results in either more segregation or less integration. The index value at which the impact changes from integrating to segregating varies for each index. As the impact of the awards moves from 0 to 1, it either integrates less or segregates more, depending on which side of the point it falls. I report the value of the relative impact index where this change takes place and label it as "seg/int point." I then place the average impact of the lottery process with and without the CP policy on that scale and compare the difference.

The results of this analysis are presented in Table SR8. When we examine the relative impact of the CP policy on the change in the dissimilarity index (which adjusts for the restriction in the range of possible values caused by the small number of apartments and the fact that any race can be selected, while the dissimilarity index can only be impacted by selections of the two races of interest), we find that the impact of eliminating the CP policy is very small compared to what it could be. The relative impact on the white and African American dissimilarity index, accounting for the number of actual selections and the fact that not all selections will be white or African American, on a scale of 0 to 1 is .044 for whites and African Americans, .022 for whites and Hispanics, and .031 for whites and Asians. These relative reductions in the integrative impacts of the CP policy are small, representing less than 5% of the possible impact. The relative integrative impact on the dissimilarity index of the lottery (with the CP policy in place) is approximately twice as high as the reduction in the index caused by the CP policy. Hence, Dr. Beveridge's concerns about the restriction on the impact of the number of relevant selections (i.e., those that can impact the index) is correct, in that it results in the actual impact being trivial (the actual effect being

measured is in the 4th decimal place). However, when we account for his concerns, we find that the relative impact is not as trivial as the actual impact, but is still relatively *di minimis*, and even with the CP policy the lottery remains integrative.[64]

TABLE SR8
THE RELATIVE IMPACT OF THE CP POLICY ON DISSIMILARITY INDEX ESTIMATED VIA SIMULATION AND IMPACT ADJUSTED TO ACCOUNT FOR CONSTRAINT OF NUMBER OF UNITS AND THE SELECTION OF APPLICATION OF NEITHER RACE

White v.	Max Segregation	Relative Impact if Seg/Int Point*	Relative Impact if CP Policy	Relative Impact if No CP Policy	Max Integration	Relative Impact of Practice of CP Policy	Relative Impact of Lottery Process
African American	1	0.484	0.400	0.355	0	0.044	-0.084
Hispanics	1	0.504	0.417	0.395	0	0.022	-0.087
Asian	1	0.698	0.643	0.612	0	0.031	-0.056

* = Relative values larger than this increase the Dissimilarity Index while relative values smaller than this reduce the Dissimilarity Index.

Amended December 12, 2019

8. Distance

Dr. Beveridge raises three issues with my study demonstrating that there is statistical evidence that applicants are more likely to apply for projects that are nearer to where they live, and that this pattern holds irrespective of whether one considers only projects located in areas outside their community preference areas or only projects located within their community preference areas.

Dr. Beveridge correctly states that I reported that I found "small correlations."[65] This is true, because distance is only one of many factors that would impact applying for affordable housing which, as I noted, is highly desirable and hard to obtain.

[64] This small relative impact is constant, even as the number of apartments increase.

[65] See Beveridge September 2019 report at ¶ 89.

Although the correlations were small, these findings were impressive because of how consistent a factor distance was, insofar as distance was a factor for almost every project. The likelihood of seeing such consistency in the correlations between distance from the application's current residence and projects applied to if distance were *not* a genuine factor is infinitesimally small.

Dr. Beveridge's argument that the correlations are "remarkably small" rather than merely "small," as I stated, is not actually based upon the correlations and is flawed.[66] The values for the correlations that he reports for the 25th and 75th percentiles[67] are not the correlations between distance and the likelihood of applying. Rather, they are the regression coefficients showing the impact of living one mile from the project on the likelihood of applying. Those values he reports are indeed remarkably small (at the third and fourth decimal places), as living a single mile from the project has little impact in restricting one's likelihood of applying. However, if an applicant lives ten miles away, the effect on their likelihood of applying would be ten times larger. When one looks at the 25th and 75th percentile of the correlations (not the coefficients), they are actually measured in the first and second decimal place (i.e., for those outside the CP area the 25th and 27th percentile values of the correlation are .032 and 75th percentile .132). Thus, while these correlations are small, Dr. Beveridge's conclusion that they are "remarkably small" is incorrect.

The correlation is small because, as Dr. Beveridge notes in his second critique, "there are a variety of factors that can influence or determine why (other than distance) a person does not apply to a lottery."[68] He then lists eight factors he suspects a person may not apply to a lottery[69]

[66] See Beveridge September 2019 report at ¶ 89.

[67] See Beveridge September 2019 report at ¶ 89.

[68] See Beveridge September 2019 report at ¶ 90.

[69] The listed factors are: (1) no longer living in New York City, (2) not having the same household composition, (3) not having the household income to be eligible for the lottery, (4) not needing an apartment

and criticizes me for not taking these factors in account.[70] However, as he knows, the reason I did not account for these factors is because I do not have the data to control for these factors. In statistics, such variables are referred to as omitted variables. However, unless the omitted variables are correlated with distance, they would not change the estimate of the effect of distance on the likelihood of applying. All of the factors Dr. Beveridge lists likely impact one's likelihood of applying for a specific project, but logically appear to be independent of distance. That is, there is no reason to believe that their values would change depending on how far the applicant lives from the project. In fact, when asked at his deposition whether some of these factors are related to how close they live to a project, Dr. Beveridge conceded that they did not.[71] Hence, controlling for these factors would statistically be expected to only increase the correlation between distance and applying.

Dr. Beveridge's third point raises a potentially valid data issue. He questions how we can know whether the address of an applicant is correct if that applicant did not apply for an opening at the project being studied.[72] How can we know that their address has not changed and is incorrect at the time when they would be applying? Moreover, how can we know that the applicant was interested in any project at that time if they did not apply to it? To the extent that an address is not the applicant's actual address, and thus the distance measure is in error, statistical theory tells us that if the errors were random, we should expect the errors to cause the estimate of the effect of distance to understate the true effect.

---

any longer, (5) feeling discouraged from not getting an apartment and therefore taking a hiatus from applying, (6) not learning about the lottery, (7) not having time to apply, or (8) something about the particular project other than distance. See Beveridge September 2019 report at ¶¶ 90 and 91.

[70] See Beveridge September 2019 report at ¶¶ 90 and 91.

[71] See October 4th Transcript, page 54 line 15 through page 56 line 10.

[72] See Dr. Beveridge's September 2019 report at ¶ 92.

Nevertheless, I redefined the population of potential but not actual applicants for each project to consist of only those who submitted an application to another project during the period that the project being studied was open. This significantly reduced the sample size, which in turn reduces the possibility of finding statistical significance. As a result of the redefined population, we know the address of the person as it relates to the project being studied is correct, and we also know that the applicant was interested and believed they were eligible for affordable housing in the City when the project was being filled.

Table SR9 reproduces Table 8 from my Amended Report with this new definition of the applicant population. There were 9 lotteries where the actual start and end date for application acceptance were not recorded in the Housing Connect data.[73] Hence, I studied only 159 projects. The pattern continues to overwhelmingly support the conclusion that distance is a factor in the likelihood of a person to apply for projects. That is, even with the more narrowly defined population of potential applicants, the study still shows that applicants typically prefer apartments that are closer to where they live. The probability of seeing such a consistent pattern of the likelihood of applying increasing the closer the applicant's original address is to the project is infinitesimally small if distance was not a factor in an applicant's decision to apply on that project.

[73] This is because these 9 lotteries were paper lotteries that were then entered into Housing Connect for purposes of generating a log. For these projects, the date before the project closed was entered as the "start date" in Housing Connect. Thus, we did not know the open date.

TABLE SR9

RELATIONSHIP BETWEEN THE LIKELIHOOD OF AN APPLICANT* ENTERING THE LOTTERY FOR EACH PROJECT AND HOW FAR THE APPLICANT LIVES FROM THAT PROJECT

Applicants Who Do Not Live in the CP Area of the Project

Likelihood of Applicant Applying for Specific Project	Number of Projects	Percent of Projects
Statistically significantly more likely to apply	141	88.7%
Statistically significantly less likely to apply	12	7.5%
No significant impact (more likely to apply)	3	1.9%
No significant impact (less likely to apply)	3	1.9%

Applicants Who Live in the CP Area of the Project

Likelihood of Applicant Applying for Specific Project	Number of Projects	Percent of Projects
Statistically significantly more likely to apply	77	48.4%
Statistically significantly less likely to apply	4	2.5%
No significant impact (more likely to apply)	52	32.7%
No significant impact (less likely to apply)	26	16.4%

* = Applicant represents those applicants who applied for the project or applied for another project when the project in question was open.

Amended December 12, 2019

Dr. Beveridge argues that even if one accepts the premise that there is a general preference to remain close to one's current home, "the community preference policy is not organized to capture the people it wants to be helping 'stay close'" to where they live.[74] Nonetheless, his analysis does not contradict my studies which demonstrate that applicants have a preference to stay close to home. Rather, his analysis shows that more applications without the CP than

[74] See Beveridge September 2019 report at ¶¶ 94-98.

applications with the CP live within 1.5 or 2 miles of the project. While this finding is true, the issue here is the racial impact, not whether more people have the CP. Dr. Beveridge never addresses what the racial impact of changing the boundaries from CD lines to a 2 mile radius would be, and therefore his analysis is meaningless. In fact, as set forth in Table SR10, the racial mix of applications in a two mile radius appear to be very similar to the racial mix of applications living within their CP area, so there is no reason to expect any difference in terms of disparate impact.

TABLE SR10

COMPARISON OF RACIAL DISTRIBUTION OF APPARENTLY ELIGIBLE APPLICATIONS OF THOSE WITH CP AND THOSE WITHIN 2 MILES OF PROJECT REGARDLESS OF CP STATUS

Race	Percent of Applications*		Ratio of CP Rate to 2 Miles Rate
	With CP	Within 2 Miles	
White	8.7%	8.5%	101.7%
African American	39.7%	36.8%	107.9%
Asian	4.0%	4.3%	93.8%
Hispanic	35.2%	38.2%	92.2%
Other	5.9%	6.0%	98.6%
Refuse	6.5%	6.2%	104.3%
Total	100.0%	100.0%	

* = Those for which distance can be measured.

Amended December 12, 2019

9. Miscellaneous[75]

Dr. Beveridge's assertion that I do not recognize that the bottom-line impact is not relevant when one can estimate the specific impact of the practice being challenged[76] is simply wrong. Unlike Dr. Beveridge's analysis, my analysis attempts to specifically measure the impact of the CP policy, which is the challenged process. My simulation controls for factors that contribute to being awarded an apartment that are not impacted by the CP policy, and compares the lottery process with and without the CP policy, therefore isolating the CP policy's impact. I also look at the impact of the consideration process, to which the CP policy is a major contributor. While not focused solely on the impact of the CP policy, this study does restrict the analysis to that part of the process where the CP policy has its effect. Finally, I do offer a bottom-line analysis. It does not rely on actual applications, but uses the estimated applications based upon the racial distribution of the low-income population of NYC (assuming no discouragement). This bottom-line approach is particularly relevant to the extent that there is an allegation that applications are discouraged from applying (or encouraged to apply) to projects as a result of the CP policy.[77]

Dr. Beveridge's comment about the use of the tract versus community district composition[78] is incorrect and demonstrates a misunderstanding of my analysis. There is no question that the CP policy applies to persons in a CD preference area. The questions concerning

---

[75] Certain arguments raised in the Miscellaneous section were duplicative or closely related to arguments addressed elsewhere in Dr. Beveridge's reply report, and thus, I responded to them elsewhere as well.

[76] See Dr. Beveridge's September 2019 report at ¶ 101.

[77] When asked in his depositions whether the CP policy has an impact on who applies for affordable housing, Dr. Beveridge first said no, but then he refused to rule it out (see page 61, lines 13-17 of October 4th Transcript). This is inconsistent with his statement that "the relevant population for disparate impact purposes consists of those households who have applied, not the overall City population." Beveridge September 2019 report at ¶ 102.

[78] See Beveridge September 2019 report at ¶ 103.

this practice are not whether the CP policy is actually a preference, as its name defines, but whether the preference has a disparate impact by race and whether the practice perpetuates segregation. The use of the census tract applies to the question of segregation, since the measure of segregation is calculated at the census tract level. My discussion about the disconnect between the racial demographics of the CD typology and the racial demographics of the census tract where the project is located was presented to illustrate one of the flaws in Dr. Beveridge's conclusion that selecting a white CP beneficiary in a white CD typology perpetuates segregation. As I explained, the racial demographics of the census tract in which the project is located, as well as the racial demographics of the census tract from which the white CP beneficiary will move, are essential to determining the segregating or integrative impact of that move.

10. <u>Conclusion</u>

None of the criticisms or programming errors Dr. Beveridge addresses in his September 2019 reply report or that were raised in the November 15, 2019 or October 4, 2019 depositions alter any of my prior conclusions regarding the impact of the CP policy on African Americans and Hispanics, or the impact on segregation in New York City.

Bernard R. Siskin, Ph.D.
Dated: December 13, 2019

APPENDIX H

TABLE SR1
IMPACT IN AWARDS BY RACE DUE TO DIFFERENCE IN SELECTION RATES BY CP STATUS

	Awards					
	Overall	White	African American	Hispanic	Asian	All other Races
Surplus/Shortfall due to Difference in Selection Rates by CP Status		41	93	-38	-76	-20
Adjusted* Selections Rate	0.32%	0.34%	0.33%	0.32%	0.28%	0.31%
AIR			97.24%	93.91%	82.34%	92.21%

* Removing differences among applications with the same CP status, since CP status cannot be the cause of the disparity.

Amended December 12, 2019 (previously Appendix K)

A P P E N D I X I

UNITED STATES DISTRICT COURT
SOUTHERN DISTRICT OF NEW YORK
----------X
SHAUNA NOEL and EMMANUELLA SENAT,

PLAINTIFFS,

-against-

Case No.:
15-CV-5236(LTS)(KHP)

CITY OF NEW YORK,

DEFENDANT.
----------X

DATE: October 4, 2019

TIME: 10:17 A.M.

DEPOSITION of an Expert Witness, PROFESSOR ANDREW A. BEVERIDGE, taken by the Defendant, pursuant to a Court Order and to the Federal Rules of Civil Procedure, held at the New York City Law Department, 100 Church Street, New York, New York 10007, before Geraldine Naber, a Notary Public of the State of New York.

have had to then change his -- the disability preference in his -- in the data set. He'd have to have changed it to either HV or mobility. But for the processing that we're doing here, it wouldn't have mattered because then it would have been properly marked as a disability preference. Let's say he put it on down as HV, which is like a -- which goes after mobility. If he had done that, it would have been a preference and this erroneously bypass would not have occurred. But there would have required modifying the data and that's the point.

So the program -- the bug that we found in the program, you know, created I thing 85,000 erroneous bypasses. But these cases are data cases where the status sheets, which is whatever -- what both the Defendant and the Plaintiffs kind of agreed was a ground truth or what is called, you know, like, the source of truth, don't tell you that. But it could have been fixed just using what we know -- if it's a DUNS, it's a disability unspecified.

Q. Is there a reason that you didn't make that fix?

MR. GURIAN: Objection. You may answer that question.

A. Well, the answer to it I think is fairly simple. We made the one fix. We were finding other issues. These are not -- these are not exhaustive. So someone, and I think it's really Mr. Siskin -- Dr. Siskin's business to go

through and carefully review this. 'Cause it was not very hard to run the SAS program looking for what I would call outliers in the NP level. Because that's where the erroneous bypasses come from. They come mostly in the NPs, some in ME, but mostly in NP. ME and NP are two preference categories.

Q. So another problem that you identify and do not account for in your calculations of considered applications that you set forth in Table 2 are the NYCHA selections that you reference and discuss in paragraphs 55 and 57.

Can one tell from the data who had the NYCHA preference?

MR. GURIAN: Objection as to form and as to foundation, lack of foundation.

A. From the data that was shared with Dr. Siskin, the answer actually is yes. There's both a code for NYCHA preference and then if you go look -- because the issue here are the NYCHA preferences that are effectively community based -- community preferences. So you could tell there because they tested for each of the -- the Housing Connect people tested to see if they're in the CD. If they were in the community district preference area and if they were there is a code in CD num.

MR. GURIAN: For the reporter, the word was tested, the City tested.

Is this the type of concern that you're looking at now?

A. I think it's possible.

Q. And in what specific program or code are you concerned that there may be some coding errors?

A. It appears that there may be some problems in the I guess it's called -- in the sections that he -- where he analyses perpetuation. But we're not finished with the review.

Q. Can you identify with any further specificity of where in the code regarding perpetuation of segregation you have concerns?

A. Yes, I think so. It's -- but see, this is one of these nerdy things. You know, it's hard -- it's kind of, like, hard to get it, you know, verbally on the records. So perhaps the best way to do it would be to look first in his report.

Q. Exhibit DD?

A. Yes, the table. I think there's two -- there's five or there are several tables that relate to this. You know, the Table 5, which is basically drawn from Table 6, and he talks about using -- basically doing a comparative dissimilarity analysis where he uses the underlying fractions that are used to calculate the similarity for the applicant and he does it by tract, you know. But for the applicant, for the project and for the City and -- let's

see if he describes it. (Looking in report.) Delineating account and he looks at whether or not, or at least he says he looks at whether or not, the award decreases and dissimilarity increases -- dissimilarity index. Dissimilarity is dis with similarity after just for the reporter's benefit. Or had no impact on the dissimilarity index.

And I guess the first point I'd make is that he has six -- he has basically four -- there are four races, race groups, race Hispanic groups, that he looks at, which are non-Hispanic white, non-Hispanic black, non-Hispanic Asian and Hispanic. And actually there is a fifth race group in the analysis that's not discussed in his report, which is other. Which is actually kind of like a catchall category.

So if you look at the table -- so you have segregate -- let's take -- just look at the top -- we'll look at, just to give and example for a problem, of what one of the issues is. It says white versus African American.

Q. Are you looking at Table 6?

A. Yeah, excuse me, Table 6, first row. Go to Table 6, first row. And so it says white versus African American and then the total is 8224 and, as you know, 10,245 units were awarded. But because some of the units are split by

tract and some, as you know, have more than one CD preference area, so they basically combine -- so some cross CD boundaries as well. So he eliminates those, which I think is reasonable, and then it also turns out that it's somewhat diminished because with the geocoding, and this is also very reasonable, you never get everyone, you know, when you geocode. Some addresses don't geocode properly. So anyway, so that's the first thing he does.

So then he takes the tract -- well, he basically marks -- you know, he takes -- so, for example, whites versus African American. The way that works is you get a ratio of whites versus African Americans in various ways and then you -- you compare it. Well, if you look across and so this -- I just want to make this point, he's got 8224 and then he has segregate 213 no effect, 7435 integrate 576. Well, the problem with the 7435 is that it actually combines two sorts of data. It combines data based upon the other pairings. So he looks at every potential pairing and he says, okay, which pairings -- which pairings are integrative, which pairings are not integrative and which pairings have no effect.

Well, the first point that has to be made is the -- a pairing for whites versus African Americans would, if you say compare whites versus Asians, you know, the fact is the black -- the white/black pairing will, you know, or

the -- or other pairings -- like, so say the white/Asian pairing would have no effect on the white versus African American pairing. So he has no effect of all of the cases where there is no way there would be an effect, because the relevant groups are not there. So it gives kind of a misleading impression of what he's actually testing.

So for one thing that needs to be fixed on this chart would be to parcel out not applicable from no effect. So that's the first point. And I did, you know, we did notice that and probably should have said something about it.

Then the second thing is that looking at the actual code, he has the three ratios, like, it would be white -- it would be blacks versus white/blacks for tracts, projects, which is the tract that the project's in, and City and see if he could -- as a basic way dissimilarity works, the basic interpretation and the actual thing about dissimilarity is it's an evenness measure. So it measures how even are groups spread around. So what he's testing here is whether or not the moves would make them more even or less even in effect. So if it makes it more even, that would be integrative, if it makes them less even, that would be segregated.

Well, the problem is that from a review of the codes, and I'm not, you know, I'm not certain if this is

true, but from a review of the code it appears that he didn't take into account the direction of the percentage to look at. So, in other words, if you're looking at blacks and you want to see if they're more even or less even, it's either higher or lower than the black tract -- black over black plus -- well, black, and when I say black here I mean non-Hispanic black. So it's a higher or lower of the percent of non-Hispanic blacks are of non-Hispanic whites plus non-Hispanic blacks. And so if that -- if that -- if you're -- in effect if you're moving from a tract that's lower than that to a tract that's higher than that, then that would be -- that would actually be a segregative move and visa versa. If you're moving from a tract that is above the average for the City with that ratio and you move to a tract that's below the average for that City with that ratio, then that would be an integrated move. So that's the way it's set up.

So the thing that appears to have happened is Dr. Siskin doesn't seen to have conditioned the test of the -- of the ratio based upon which race he's testing. And so we've got, you know, we're going to look -- review this further, but that, you know, since you asked, that is one issue or actually two issues, so.

Q. Before I follow up on that, are there any other issues that you have identified and are exploring?

at the same time I argue that you don't really -- when you have a correlation of minus .0016, that is such a small correlation and you're going to find many -- if you have thousands and thousands of cases which actually, as you know, it's very possible in this situation because there's so many applicants that you're going to have a lot of applicants. So there's kind of a rubric in statistics and I's sure Dr. Siskin knows this, because he's -- he is a very well known statistician, he was chair of the Statistics Department at Temple, that if you have a large enough number of cases all differences become statistically significant, virtually all differences.

So he has what I would call a very meager correlation, you know, meager, it's so small it's hard to see it and then your have, you know -- then you're asking me should he put in other results. I mean if you're going to see what the decision pattern is for applying, the answer is yes.

Q. So I'm going to go through the factors that you note in paragraphs 90 and 91 and I think that the answer to the question could be a relatively quick, yes or no answer. And let's see how that goes.

One factor that you mention is that a person may no longer liver in New York City. Do you expect that the probability of a person moving out of the City is related

to how close they live to a project?

A. No, it's a question of whether or not they'll apply. I mean I guess the fundamental problem that we found, he did not actually screen out time here. So, in other words, we don't even know if the -- if you move out -- let's say you move to Florida, you're not going to apply for a unit in, you know, New York. I mean you left New York. And we also know that the priority -- if you're outside of New York the priority -- you're at the lowest priority level. So it's not that it's going to effect -- it's going effect whether you apply.

Q. So let's go on to another factor that you mentioned that a person's household composition or income might change.

Do you expect the probability of one's income or household changing to impact their interest in a project so that it would impact their interest in a project if that's related to one how close they live to a project?

MR. GURIAN: Objection as to the form of the question. Objection as to it being a compound question, but you may answer the question.

A. I think you have to repeat that, because I'm thinking about it.

Q. Well, one of the comments that you note is that the household income also the household composition may

change.

Do you expect the probability of a household's composition changing to impact how close the probability of how close they live to a project?

A. No, but it could -- see, once again, it could effect whether they apply. Because let's say they're -- the -- their -- first off it wouldn't necessarily have to change. If their income is different than the income specified in the ads, the income ranges specified in the ads, why would they apply.

And Dr. Siskin didn't test for that, he didn't test for whether or not they're still in New York, he didn't really screen out whether or not they were, in effect, still eligible. I mean if you -- he basically took people that had applied once and he said, oh, well, they can apply to all 168 projects. And I -- and I mean that's kind of a -- sort of an interesting assumption.

Q. So let's talk about that. Would restricting the applications studied for a project which an applicant didn't apply for to only those who applied for another project which was open at the same time resolve your concern of not knowing the proper address of the non-applicant?

MR. GURIAN: Objection as to form.

A. Well, that would be another issue. We don't

once they've applied they're not going to get, you know -- they're going to have a much less chance of getting into the unit. And -- and so -- so the real comparison I think is not that this project, you know -- that there are more African Americans searching for, in effect, affordable housing.

The real question here is, you know, if you apply should you be treated equally. And -- and the City's project -- the City's process assures that you will not be treated equally. That it will be treated based upon the community district that you live in for each and every application you make.

Q. Does the community preference policy alter the mix of people who apply for a project?

A. No, in fact, it doesn't. As far as -- well, we don't know. We don't know. Actually I'll take that back because -- because it hasn't been tried.

Q. Again, shifting topics a bit. Dr. Beveridge, did you find any problems with Dr. Siskin's lottery simulation, other than that he reported the results overall and citywide and not by CD typology?

A. I guess I don't know what you mean with problems.

Q. Well, you've criticized the simulation for not doing its analysis by CD typology and you then undertake your own analysis of some of the simulation data and you do

A. Okay.

Q. Page 2, of Exhibit CC.

A. Well, I guess that way is unnecessary. I don't really believe that generally units would be fungible. They might be fungible in a specific project. In other words, if you have a one bedroom room on the third floor, that might be more or less fungible with a one bedroom apartment on the seventh floor in a given unit, with a given neighborhood, with certain neighborhood characteristics. And that would kind of assume that all else being equal in the unit, but let's say you like a view. Seventh floor is better than the third floor or on the first floor, stuff like that. All of those things effect the units and that's just within the project.

Now if you get outside the project, you have the neighborhood, you have the transportation, you've got the fact that people may want to be in a certain location because of jobs, you have the schools, you have the crime rate, you have all these things that go toward neighborhood characteristics.

And so assuming that a unit, and I don't know if there's any projects in Staten Island, so assuming that there's a unit in the Brooklyn and a unit in the Bronx, that those are kind of in some way equivalent, I think is a very -- it goes against, like, the history of real estate I

would say.

Q. So your analysis in each of your reports, your Preliminary Report, the June 1, 2017 report, your April 1, 2019 Report and your September 19th, 2019 Report, treat apartments as fungible though, don't they?

MR. GURIAN: Objection. You may answer the question.

A. Not in the way that Dr. Siskin does.

Q. In what way do you treat the apartments as fungible?

MR. GURIAN: Objection. Foundation --

Q. In those reports?

MR. GURIAN: -- and to form. You can answer.

A. I guess I would argue -- I would say something slightly differently. I think I don't actually treat the apartments as fungible. So I don't treat that -- but we look at one aspect, which is clearly not, you know, not -- is distinctive among neighborhoods and the one aspect is the racial composition. And so, you know -- so I think that that is, like, a very, very important point and Dr. Siskin sort of assumes not just that all apartments are fungible, but he assumes that all neighborhoods are equal.

Q. So are you saying that in a one bedroom affordable unit that goes for the same price in a black

So please answer that question.

Q. Is it your opinion, I think this is a yes or no answer, and you if need to explain you can, but I still haven't heard.

Is it your opinion that an apartment that is a one bedroom apartment in a majority black neighborhood and a one bedroom apartment in a majority white neighborhood that are both affordable units, same size, same rent, is it your opinion that those two apartments are not fungible?

MR. GURIAN: Objection. You may answer.

A. It is my opinion because it depends upon the preferences of the people who are quote unquote bidding or entering the lottery to get the units. And I actually went through some of those things. So neighborhood characteristics, the person's job, the characteristics of, you know, the crime rate, all of those things which, you know, are wrapped up in real estate. I mean the old adage about real estate, real estate is -- there are three things about real estate location, location and location. And what you're trying to say I think is that location doesn't matter for real estate. Even though we all know sitting here that location is the number -- one of the number one things and if you live in the Bronx and you get -- and you need to go to Brooklyn to get an affordable apartment, but your kid goes to school in the Bronx, your job's in the

Bronx, et cetera, et cetera, et cetera, you're not -- that's not fungible with an apartment that's maybe near you in the Bronx or maybe one that's near you in Manhattan.

Q. So where in your analysis did you take into consideration any of these real estate type factors that you just described?

MR. GURIAN: Objection. You may answer that question.

A. By showing that for a -- by showing that -- well, basically all I did was I assumed that they were fungible and I looked to see where people were -- preferred to get units and where people were dis-preferring and what impact that had on racial composition. So that's the long and the short. So the whole issue of fungibility is kind of a canard.

Q. I'm sorry, I'm just not sure if I heard you correctly. You said -- can you read that back? I really don't know if I heard, that you assumed that they were or were not fungible? I couldn't hear, were or weren't fungible?

A. They're not fungible.

MS. SADOK: Can you read that back? I really couldn't hear.

(Whereupon, the referred to answer was read back by the Reporter.)

have and so -- and then I finished that by saying that I really truly believe the whole notion that you have fungibility of apartments or of housing is really a canard. Because people buy, they buy the house -- you know, they rent the housing, but they rent the neighborhood, they rent the school and they rent all that stuff. So I don't -- you know, so it's not fungible and I think it's -- and why I said it's a canard is it's kind of, like, really? Fungible?

Q. So how did your analysis take into consideration the fungibility of the apartments?

A. It didn't. It assumed they weren't.

Q. So how did your analysis take into consideration the fact that the apartments are not fungible?

MR. GURIAN: Objection. Asked and answered.

A. The fundamental thing is that I assumed that people have their preferences for units and we don't have any direct measure of those exactly. And so those -- and then they apply and then the City puts them into two groups and then the one group is the preferred group, the CD preference group and some of the other preferences, and then the rest of them are, like -- we'll call them here for this to answer this question, the NP. And so that's how -- that's how they get divided up in terms of their ability to, in effect, have their preferences honored. But their

D E C L A R A T I O N

I hereby certify that having been first duly sworn to testify to the truth, I gave the above testimony.

I FURTHER CERTIFY that the foregoing transcript is a true and correct transcript of the testimony given by me at the time and place specified hereinbefore.

See errata.

ANDREW A. BEVERIDGE

Subscribed and sworn to before me this 1st day of NOV 20 19

NOTARY PUBLIC

MARK F. SHKRELI
Notary Public, State of New York
No. 03-4996849
Qualified in Bronx County
Cert. Filed in Westchester County
Commission Expires 8/26/22

ERRATA

6:25	"effect" should read "affect" REASON: Reporter transcribed incorrectly.
8:1-2	"to compare them" should read "a comparison" REASON: Reporter transcribed incorrectly.
8:7	"comparite" should read "redline" REASON: Reporter transcribed incorrectly.
11:1	"wake-able" should read "linkable" REASON: Reporter transcribed incorrectly.
12:9	"Lottery Project Applications Submitted History Sequence Number" should read "LTTRY_PRJ_APP_SBMTD_HST_SEQ_NO" REASON: Clarifies intent of response.
12:12	Delete "so" REASON: Clarifies intent of response.
12:14-15	"lottery project number random sequence number" should read "LTTRY_PROJ_APP_RANDOM_SEQ_NO" REASON: Clarifies intent of response.
12:21-22	"lottery project applicant number" should read "LTTY_APPLNT_SEQ_NO" REASON: Clarifies intent of response.
17:1	"numbers" should read "applicants" REASON: Reporter transcribed incorrectly.
19:8	"file" should read "final" REASON: Reporter transcribed incorrectly.
21:9	"there" should read "that" REASON: Reporter transcribed incorrectly.
21:12	"thing" should read "think" REASON: Reporter transcribed incorrectly.
24:9	Add an additional sentence as follows. "'NYCHA unspecified' applicants are processed well before ''NP' applicants." REASON: Clarifies intent of response.
27:14	Add an additional sentence as follows: "As for statistical reports, paragraphs 61 and 62 of my reply report make clear the variation between Dr. Siskin's calculation of

"considered" applicants and the statistical reports of some developers showing many fewer applicants having been considered."
REASON: I was confused by counsel's describing paragraph 61of my reply report as dealing with the "disconnect between Dr. Siskin's assumed processing order and the actual processing order as shown in the award unit type file," when in fact that initial sentence of paragraph 61 represented a transition from what had just been discussed to the actual subject of paragraph 61: the disconnect between Dr. Siskin's universe of "considered" and the "considered" counts found in the statistical reports of some developers.

32:20-22 "we do not think that this is effectively the separate but equal disparate impact analysis assuming that, you know, if" should read "we think that this is part of Dr. Siskin's separate-but-equal analysis. If"
REASON: Clarifies intent of response.

35:23 "similarity" should read "dissimilarity"
REASON: Reporter transcribed incorrectly.

36:2 "account" should read "the count"
REASON: Reporter transcribed incorrectly.

38:23 "segregated" should read "segregative"
REASON: Reporter transcribed incorrectly.

39:7-8 "of the percent of non-Hispanic blacks are" should read "of non-Hispanic blacks as a percentage"
REASON: Clarifies intent of response.

39:19 "seen" should read "seem"
REASON: Reporter transcribed incorrectly.

39:22-23 Replace these lines as follows: "further. Note: We have confirmed that Dr. Siskin made the error described, as acknowledged when he recently amended page 56 (Table 6) of his already amended September 4th opposition report. His amendments to the awarded and apparent sections of the table are correct overall, as shown both by his method and an alternative method, the programming for which is separately being provided. However, he ran the "considered" section without updating it to reflect what he treats now as his "best estimate" of "considered." The addendum attached shows the results for all three sections of the table, using for "considered" Dr. Siskin's current best estimate, reporting the intermediate (not scaled) results for "considered" and apparent that Dr. Siskin chose not to include in his report, and distinguishing between those who are CP beneficiaries and those who are not. Across the sections of the tables and across the six different pairings, the results for CP beneficiaries – by number, net number, percentage, and net percentage – are significantly less integrative (*i.e.,* perpetuate segregation more) than the results for non-beneficiaries."
REASON: Clarifies and updates response.

70:13 "a par" should read "apar"
REASON: Reporter transcribed incorrectly.

74:9 "testing" should read "Dr. Siskin is testing"
REASON: Clarifies intent of response.

74:11-13 "words, the white" should read "words, 'the white" and "balance out," should read "balance out' –
REASON: Transcription failed to capture the fact that I was giving my interpretation of Dr. Siskin's position and thus the portion of the sentence that begins with "the white neighborhoods" and ends in "balance out" should be rendered within quotation marks.

75:8 "unit" should read "project"
REASON: Clarifies intent of response.

80:10 "were" should read "were not"
REASON: Reporter transcribed incorrectly.

80:10-11 "preferred to get units and where people were dis-preferring" should read "preferred (by the City) to get units and where people were dis-preferred (by the City)"
REASON: I do not believe that the reporter captured by statement correctly, and the correction in any event clarifies my intent, as I indicated at 82:18-83:9.

80:14 "short" should read "short: looking at the application choices people were making and how the City's policy affected those choices."
REASON: Clarifies intent of response.

91:7 "entrance" should read "entrants"
REASON: Reporter transcribed incorrectly.

92:14 "by" should read "but"
REASON: Reporter transcribed incorrectly.

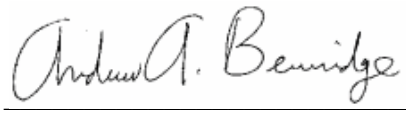
92:17 "Brown" should read "segregation"
REASON: Clarifies intent of response.

93:6 "ability" should read "effect"
REASON: Clarifies intent of response.

94:4 "and" should read "of"
REASON: Reporter transcribed incorrectly.

95:7 and 17 "and" should read "of"
REASON: Reporter transcribed incorrectly.

97:7	"ism as Dr. siskin has shown" should read "is as Dr. Siskin has shown in his September 4th report" REASON: Reporter typo and clarifying response
97:10	After "perpetuation." add the following sentence: [Note: In the amendment to his September 4th report, Dr. Siskin shows perpetuation for all six racial pairings.] REASON: Updating response to comport with Dr. Siskin's corrected and modified findings.
101:8	"that" should read "than" REASON: Reporter transcribed incorrectly.
101:10	Strike the word "can" REASON: Clarifies intent of response.
103:12	"apply" should read "apply, and, without the policy, would have competed on a level playing field." REASON: Clarifies intent of response.
104:5 and 7	"entry" should read "entrant" REASON: Reporter transcribed incorrectly.
110:18	"entrance" should read "entrant" REASON: Reporter transcribed incorrectly.
114:11	"preferences" should read "preference areas" REASON: Clarifies intent of response.



ANDREW A. BEVERIDGE

A P P E N D I X J

UNITED STATES DISTRICT COURT
SOUTHERN DISTRICT OF NEW YORK

----------x

SHAUNA NOEL and EMMANUELA SENAT,

Plaintiffs,

-against-

Civil Action No.: 15-CV-5236

CITY OF NEW YORK,

Defendant.

----------x

August 26, 2019
9:59 a.m.

VIDEOTAPED DEPOSITION of PROFESSOR BERNARD R. SISKIN, taken by Plaintiffs, pursuant to Notice, held at the offices of Veritext Legal Solutions, 1250 Broadway, New York, New York, before Judith Castore, a Certified Livenote Reporter and Notary Public of the State of New York.

SISKIN

A We ran the table, which is Table 2, which is in this report which uses eligibility. Okay. And we supplied it to you. We supplied you the data that underlies that. Okay. Came up with discussion, clearly you were raising the question that that model doesn't convert. Okay.

I realized then that still going with the model, being the appropriate statistical model, but that we had done some studies to determine that that was the appropriate statistical model.

So I supplied you with those studies and the backup for those studies which included one new variable which was a way of trying to get rid of the colinearity in the data set to get it to converge.

Q So at the time of the filing of the report, it didn't converge?

MS. SADOK: Objection.

A It still doesn't converge.

SISKIN

And it still is a valid study of what it purported to do, of what it produces.

Q So did any model converge?

A Yes. Actually, two models converged and get to converge with the base study and get to converge by eliminating the bedroom apartments. You can get a partially converged linear probability model, okay. There are options. All those options, by the way, showed greater disparities than the model that we produced.

They're the type of things that you look at just trying to do away with that, and the tradeoff always is, what are you doing with the model. And when you start to combine it or get rid of some variables in the process, you lose some information. But you gain a model which converges. The question then becomes, I've been taught, you're looking to see whether or not it affects the variables that you're

estimating.

The variable we're estimating, okay, which were race and preference status, really not affected by the convergence problem. And that's -- that's indicated by the -- running the ordinary lease squares model which indicates which variables, estimates are tenuous because of the correlation, you have to do it restricted, and which ones aren't, okay.

And we know that a linear probability model is not as good as the logistic model because the average it's to averaging -- it's actually as good as the logistic model, okay. That's an option to use.

If you use that option -- if you look at the back up, you'll see that it actually increases the data, increases the findings, makes it even stronger.

The other two methodologies I

couldn't get rid of the variable, which is the least preferred because you're giving up a lot of information, but you get a convergent model. And that, again, gets a result which is worse from the plaintiff's viewpoint, strengthens the conclusions.

The third way is to try and build a model which gets rid of combine some. We did that through the bedroom model which loses some information but gets you a convergence. Again, the disparities were larger in terms of the relationships and correlations between race and preferences.

Q All of these things you did prior to filing the report?

A Yeah, we did this initially because I felt -- yeah, this is the standard thing we do to get a non-convergent model.

Q So the -- was there a note in the report that I missed that stated that the model didn't converge?

A C K N O W L E D G E M E N T

STATE OF NEW YORK)
) ss.:
COUNTY OF NEW YORK)

I, BERNARD R. SISKIN, certify, I have read the transcript of my testimony taken under oath in my deposition of August 26, 2019; that the transcript is a true, complete and correct record of what was asked, answered and said during this deposition, and that the answers on the record as given by me are true and correct.

BERNARD R. SISKIN

Sworn and subscribed to before me

this 11th day of October, 2019.

Notary Public

Commonwealth of Pennsylvania - Notary Seal
ERIC SISKIN - Notary Public
Philadelphia County
My Commission Expires May 10, 2023
Commission Number 1262372

Errata -- Testimony of Bernard R. Siskin, dated August 26, 2019				
Case: Noel and Senat v City of New York				
Page	**Line**	**Now reads**	**Should read**	**Reason for change**
181	8	to you in you	to you in	misspoke; clarity
181	15	But the A using	But a suffix is used, I believe, then	To clarify the intent of the sentence.
191	18	effects	affects	typo
193	5	effect	affect	typo
193	6	effect	affect	typo
194	24	disagreement	argument	typo
194	25	Because you can't determine who.	Because you can't determine who was actually impacted by the practice at	clarity
199	24	policies	policy's	typo
200	5	to -- for blacks	too few blacks	typo
202	20	in terms at what	in terms of what	Transcription error
203	20-21	you're going to challenge it you can't do that, then you move it to the next	you're going to challenge that analysis that it can't be done accurately, then you move the analysis to the next	To clarify the intent of the sentence.
203	10	wind as	wind up as	Transcription error
208	21	How	What	misspoke
214	15-16	where you do not make it.	where you make it without it.	clarity
219	23	process	preference	misspoke
219	25	process	preference	misspoke
220	14	overprotected	a protected	typo
223	5	counts	accounts	typo
223	8-9	CP awards	awarded apartments through the CP policy	clarity
235	20	hired	awarded	misspoke
241	8	lease	least	typo
241	16-17	logistic model because the average it's to averaging	logistic model, but estimating the average effect	To clarify the intent of the sentence.
242	10-11	gets rid of combines some.	gets rid of or combines some variables.	clarity
243	23	catenation	concatenation	typo
251	16	catenation	concatenation	typo

Errata -- Testimony of Bernard R. Siskin, dated August 26, 2019				
Case: Noel and Senat v City of New York				
Page	**Line**	**Now reads**	**Should read**	**Reason for change**
254	21-24	I get -- apartment, I get an apartment. Okay? And if I bid -- if I don't get an apartment, I get an apartment	I get one apartment, I get an apartment. Okay? And if I don't get any apartment, I didn't get an apartment	To clarify the intent of the sentence.
255	13-14	Most people -- there are some people.	Most people would not	clarity
262	3	bias	biased	typo
262	22	MAGNA	MAGA	misspoke
265	9	your	you're	spelling error
270	4	early state	earliest date	clarity
282	9	six month	six mile	misspoke
286	4-5	which is preference	which is a preference	clarity
287	2-4	and, of course, getting the preference is -- has to be designed as with the outcome of getting it is.	and, of course, getting the preference, on average, is good. It is designed to be an advantage with respect to the outcome of getting an award.	To clarify the intent of the sentence.
288	12	in illustration	in the illustration	To clarify the intent of the sentence.
		By R Sisk	10/11/19	
		Bernard R. Siskin	Date	
		Subscribed and Sword to Before Me this ____ Day of _____, 2019		
		SEE Attached Certificating		
		Notary Public	Commission Expires	