Exhibit 56

RESPONSIVE REPORT

in the matter of

ABDIQAFAR WAGAFE, et al.

v.

DONALD TRUMP, President of the United States, et al.

By

Bernard R. Siskin, Ph.D.
Director

BLDS, LLC
Philadelphia, PA

October 13, 2020

I. INTRODUCTION AND OVERVIEW OF ANALYSIS

In this supplement to my amended report submitted on July 17, 2020, I assess and respond to the July 17, 2020 supplemental report and the September 21, 2020 second supplemental report of Sean M. Kruskol, Plaintiffs' accounting expert, including his conclusions, and present my analyses related to that assessment. I also address and respond to various statements, conclusions and opinions contained within the reports of several other of Plaintiffs' experts (Marc Sageman including his responsive expert report, Thomas K. Ragland, Jay Gairson, and Nermeen Arastu), including those that purport to rely upon statistical data analyses or warrant statistical analyses they fail to provide.[1]

II. RESPONSE TO MR. KRUSKOL'S SUPPLEMENTAL EXPERT REPORT

A. Overview of my Analysis and Response to Mr. Kruskol's Report and Conclusions

Mr. Kruskol's analysis uses the updated USCIS data tables provided to Plaintiffs in June 2020, for fiscal year (FY) 2013 through FY 2019 supplied in tabular form, which he refers to as the USCIS data, and the underlying data set USCIS supplied, which was used to produce the tables supplied. I relied upon the same data[2] for my report. He also summarized seven class lists

[1] In my amended report, and with statistical analyses, I address and refute the unsupported opinions expressed by several of Plaintiffs' designated "experts" (*e.g.*, Mr. Ragland, Ms. Arastu, Yliana Johansen-Mendez, Narges Bajoghli, and Mr. Kruskol) that USCIS operates CARRP with an anti-Muslim animus or effect.

[2] I was supplied with the same tabular data and underlying data set. Also, USCIS conducted a reliability study at my request, on randomly selected applications subject to CARRP, and provided the results I reference here and in my amended report. In addition to the statistical data and other sources of information cited or referred to in this report, I relied upon the July 17, 2020 supplemental report and the September 21, 2020 second supplemental report of Sean M. Kruskol, the reports of other Plaintiff experts Marc Sageman (including his responsive expert report), and the reports of Thoams K. Ragland, Jay Gairson, and Nermeen Arastu. I also reviewed the 9/30/2020 30b6 deposition of Kevin Quinn, the 1/30/2020 deposition of Mr. Quinn, the 12/12/2019 deposition of Christopher Heffron, the 8/31/2020 deposition of Kevin

(collectively referred to as the "Class Lists") prepared by USCIS. I did not rely on this data in my amended report but have reviewed the class lists for preparing this responsive report to address Mr. Kruskol's analyses of them. In his second supplemental report he also relies upon the August 31, 2020 deposition of Kevin Shinaberry. I have also reviewed the deposition of Mr. Shinaberry in preparing this report.

Based on his analysis of this data in his July 17, 2020 report, Mr. Kruskol draws four types of conclusions.[3] His first type of conclusions is based on his simple tabulations of the USCIS data by form type. He computes and compares outcomes of applications received from FY 2013 through FY 2019, comparing outcomes for applications referred to CARRP to those not referred to CARRP. Specifically he compares: (i) the rates at which they were still pending as of the end of the time period (Kruskol supplemental report, paragraph 7a) and also controls for the fiscal year the applicant applied (*Id*. at paragraph 8c), (ii) the rate at which they were denied (*Id*. at paragraph 7b), and (iii) for those adjudicated and applied in the same fiscal year, the speed at which they were adjudicated (*Id*. at paragraph 8a).

He also computes and compares the change in the percent of applications referred to CARRP separately by form type, (*Id*. at paragraphs 7c and 7e) between FY 2013 and FY 2017, rather than the whole time period. He also computes the year by year increase in adjudications separately by form type and finds the largest year to year change between FY 2014 and FY 2019 by form type (*Id*. at paragraphs 7d and 7f); he states that the increase was not due to shortening the adjudication process, but was due to "adjudicating applications received in previous fiscal

Shinaberry, the 9/9/2020 deposition of Nermeen Arastu, and the 9/18/2020 deposition of Thomas Ragland.

[3] Mr. Kruskol's conclusions are set forth in his paragraphs 7, 8, and 9, and listed by a letter designation. I refer to each of his specific conclusions with a numerical designation indicating the paragraph and the letter designation he used.

years" (*Id*. at paragraph 8b). Finally, he computes and compares applications that were adjudicated after more than 180 days by whether the application was or was not referred to CARRP and the percent of those applications that were adjudicated within the same fiscal year as they were received (*Id*. at paragraph 8b).

Mr. Kruskol's second type of conclusions is based on his classifying applicants' countries of birth as majority Muslim or non-majority Muslim and comparing the percent of applications referred to CARRP received from FY 2013 through FY 2019 based on majority Muslim status and form type (*Id*. at paragraphs 7g and 7h).

His third type of conclusions simply consists of his calculated average number of days a class member waited from USCIS' receipt of his/her application to adjudication, by form type (*Id*. at paragraphs 8b and 8c).

His fourth type of conclusions, which presumably question the accuracy of the data, focuses on purported discrepancies between the different data sources in mean processing times for FY 2015 through FY 2019 (the USCIS data supplied compared to data published on USCIS' website) (*Id*. at paragraph 9a) and in the number of I-485 and N-400 forms reported as pending at the end of FY 2018 and FY 2019 (the USCIS tabular data, and their underlying data set) (*Id*. at paragraph 9d).

In his September 21, 2020 second supplemental report he expands on his conclusions concerning the accuracy of the USCIS data. Specifically, he concludes that the USCIS detailed data (i) overstates the number of applications subject to CARRP,[4] (ii) contains duplicate records,[5] and (iii) must be incorrect in some cases because the time to adjudication in some cases

[4] See paragraph 6 of Mr. Kruskol's September 21, 2020 second supplemental report.

[5] *Id*. at paragraph 7.

occurs within 60 days of the receipt of the application, which he suggests cannot be correct and must reflect data errors, according to counsel for Plaintiffs.[6]

Mr. Kruskol's conclusions are basically statements of the results of his simple calculations. His analyses would be described as descriptive statistics rather than inferential statistics in that he rarely offers any detailed analysis or explanation of any relevance of his findings to the allegations in this litigation. Hence, my analysis first focuses on whether his calculations match those presented in my July 17, 2020 amended report, and if not, why not? Where the Kruskol calculations do not match the parallel calculations presented in my amended report, I examine whether any such differences were material. Finally, if Mr. Kruskol has conducted an analysis which I did not, I assess whether his new calculations have any relation to the issues in this case and, if appropriate, I conduct a new analysis to properly address any issue his calculations raise.

To prepare this report and any new analysis, I used the data and CARRP-related information referred to in my July 17, 2020 amended report, the most recent class list, information on historic processing times from the USCIS website, Mr. Kruskol's amended report, the reliability study discussed in my July 17, 2020 amended report, and discussions with and information obtained from USCIS personnel associated with the agency's data sources, and the USCIS data analyst who compiled the tabular data which was based on the underlying data. The underlying data was drawn from a variety of database searches conducted by several USCIS analysts.[7]

B. Conclusions Concerning Mr. Kruskol's Amended Expert Report

In reviewing Mr. Kruskol's computations and analyses, I conclude that:

[6] *Id*. at paragraphs 7 and 26.

[7] The specific information I rely upon is noted *infra* when I rely upon it.

1 In a few computations there are trivial differences between what he, USCIS or I report due to how he and USCIS, or I, define the relevant population or events. Hence, the conclusions that I derive from the data would not change[8] if I adopted Mr. Kruskol's definitions.

2 Many of Mr. Kruskol's calculations are contained in my July 17, 2020 amended report, frequently as part of a more complete analysis related to the issue associated with his simpler calculations. With one exception, where similar analyses are not included in my July 17, 2020 amended report, it is because his analysis is too cursory to be of any value in addressing the relevant issues. In those instances, my analysis is much more in-depth and responsive to the relevant issues in this case.

3 The one exception where Mr. Kruskol's analysis may be of value in addressing issues before the Court is his examination of the number of days awaiting adjudication based on the class list. If the class list represents a meaningful unique population of interest apart from or in addition to the population contained in the USCIS data compilations, then this new calculation may be of interest. However, as detailed in my July 17, 2020 amended report,[9] average time to adjudication (such as that presented in Table 9 of my July 17, 2020 report) must be viewed with caution, and a more in-depth analysis is needed to properly

[8] To the extent that Mr. Kruskol derives conclusions from his descriptive statistics other than simply reporting the arithmetic results of his computations, I believe his conclusion would not change if he adopted my definitions, as his arithmetic results would not materially differ from those now.

[9] See pages 53-56 of my July 17, 2020 amended report.

address the extent to which that difference between groups in processing time exists and what it implies about the process being studied.

4 Every one of Mr. Kruskol's summary descriptive statistics exhibits[10] (but for six of them[11]), although arithmetically correct, has serious statistical conceptual problems which make them meaningless or misleading; they fail to account for obvious factors which may totally change the results if accounted for in the analysis. My July 17, 2020 amended report discusses these problems.[12]

5 First, any superficial comparisons by CARRP status of the change in the frequency of case adjudication, pending cases, application approvals, or application denial between two fiscal years or by each fiscal year are statistically meaningless or misleading. Such comparisons are meaningless because they fail to account for the fact that the amount of adjudications or CARRP referrals in any fiscal year depends on the age of cases[13] that are being assessed. Failure to account for the age of the cases makes such a summary statistic of no probative value. Mr. Kruskol seems to understand this, since he does conduct one study[14]

[10] All of the Kruskol exhibits except for Exhibits AA, AR, BE, and Z, which concern data issues, are simply summary descriptive statistics.

[11] Kruskol's Exhibits AB, AD, and AH properly compare the difference in approvals and denial rates between CARRP and non-CARRP applicants overall. Exhibits BB, BC, and BD, which compare by CARRP status the percent of applications received in the same fiscal year and are still pending at the end of the period, provide a proper statistical comparison. Only the parts of Exhibits AC, AE, and AI, which are the basis for Mr. Kruskol's Exhibits AB AD and AH, make proper statistical comparisons.

[12] See discussion on pages 53-56 of my July 17, 2020 amended report.

[13] The age of the application is important, since the amount of time that the application is pending impacts the likelihood that it will be adjudicated (*i.e.*, approved or denied).

[14] See Kruskol exhibits BB (both I-400 and N-400 applications), BC (I-485 applications) and BD (N-400 applications).

of cases pending where he controls for the age of cases. Without controlling for the age of cases, there is no way of knowing if the change in the number or percent of adjudications in different fiscal years is due to changes in the speed of the adjudication process, or to changes in the ages of the applications being reviewed which impact the likelihood of an application being adjudicated. Thus, there is no informational value to simply knowing that there is a change in the number or percent of applications adjudicated. To be helpful in addressing any issues in this case, the change must relate to a known cause.

6 Second, when comparing the percent of outcomes, such as the percent adjudicated between fiscal years, one must not only control for the age of the applications (*e.g.*, how long the application has been pending when adjudication was being considered, *i.e.,* whether the application was received in the same fiscal year being reviewed or some earlier fiscal year), but the population studied must encompass all applications, not only adjudicated applications. As demonstrated below, failure to account for cases still pending leads to meaningless or misleading statistics. Failure to statistically define the correct population makes Mr. Kruskol's summary table of no probative value as to any difference between the results of CARRP and non-CARRP applications.

7 Third, in comparing the referrals to CARRP of applications received in a given fiscal year, simply comparing the numbers referred to CARRP between years is meaningless or misleading. This is the censored data problem (the truncating of data falling outside specified date parameters) that I discuss in my July 17, 2020

amended report.[15] That is, one would expect the number of applications referred to CARRP to be reduced compared to earlier years because the time period for possible referral is shortened since the data is truncated beyond September 30, 2019. Thus, the data analyses must be adjusted statistically for the censored data in order for the analyses to fit the FY 2013-2019 period.

8 Lastly, when comparing times to a decision, such as mean or median time to adjudication by CARRP status, one must also consider the impact of the cases not decided if the percent of cases not decided is different by CARRP referral status. Any computation or comparison of mean and median times without consideration of pending applications is statistically incorrect and misleading.

9 Mr. Kruskol never explains the relevance of his summary statistics to the Plaintiffs' allegations in this matter. While some are obvious, others are not, and his failure to relate his findings to Plaintiffs' allegations leaves his descriptive statistics open to misuse or misinterpretation.

10 A comparison of my calculations to Mr. Kruskol's calculations concerning the outcomes between applications from majority Muslim and non-majority Muslim countries results in trivial disparities because of some differences in our definitions of a whether a country is a majority Muslim country. These differences are trivial, irrespective of whether using my definitions, which I believe are correct, or Mr. Kruskol's,[16] since the number of cases involved is inconsequential. Regardless of which definition is selected, the analyses Mr.

[15] See discussion on pages 34 and 35 of my July 17, 2020 amended report.
[16] Or another expert's judgment in the cases in which we disagree on the classification of a country as to its majority Muslim population status.

Kruskol and I provide on the effects of majority Muslim status would remain essentially the same as presented in our respective reports submitted July 17, 2020.

11 Mr. Kruskol's conclusion that the USCIS data is inconsistent with data published on USCIS' website is meaningless. He is essentially implying that the FY 2013-2019 data compilations supplied by USCIS are incorrect. I spoke with and reviewed comments by a USCIS data analyst, who explained that these two data sets (the FY 2013-2019 data set produced in this litigation and the USCIS website data) are not measuring the same populations and, therefore, differences in numbers between the different data populations would be expected. Obviously, differences between data sets are informative about possible errors in the data only if they are supposed to be measuring the same populations (and are not differently truncated based on date parameters for various events such as dates of application receipt for adjudication data). Thus, any inference that one data set is inaccurate because it differs in the number of cases or average values is unjustified.

12 Mr. Kruskol misperceives that there are inconsistencies in the USCIS data's specification of which applications are CARRP referred.

13 Mr. Kruskol's claim that the USCIS data contains duplicate applications is not supported by the data he cites. Without having personal identification information, it is impossible to determine from the USCIS data supplied to Mr. Kruskol and me if applications are duplicates. For example, the applications from twins or triplets may look identical in the database but with the full personal

identification information, which was available to the USCIS data analyst who prepared the data base, it would be obvious the applications are not duplicates. Even if one incorrectly accepted as accurate his count of alleged duplicate applications, it does not show any meaningful duplication of applications.

14 Mr. Kruskol argues that the USCIS database receipt date and current status date are not accurate. This is based on his acceptance of Plaintiffs' counsel's belief, with no supporting data identified, that claims cannot be resolved within 60 days and that some cases were adjudicated within 60 days according to the USCIS data receipt date (when the application is received) and current status date (when adjudicated). Putting aside the issue of whether Plaintiffs' counsel is correct that no cases among the 10.6 million applications could have been adjudicated within 60 days, the degree of possible data error indicated by this is so small as not to raise any concerns about the reliability of the USCIS data insofar as its usefulness to conduct valid statistical analysis. Moreover, even if all of these cases adjudicated within 60 days of application receipt were deleted, the disparity between CARRP and non-CARRP applications in the time to adjudication would narrow, not widen, although not by any meaningful amount.

15 Mr. Kruskol claims that the USCIS over-reports the number of applications processed in CARRP[17] because it counts applications that were initially determined to possibly be national security (NS) concerns but were ultimately found not to be NS concerns. Mr. Kruskol assumes these should not be counted

[17] See paragraphs 6, and 8 through 20 of Mr. Kruskol's September 21, 2020 second supplemental report.

as referred to CARRP and processed in CARRP. The proper role of a statistician is to determine data reliability and analyze data produced. I am not an expert in how USCIS defines and interprets its internal CARRP policy, nor, to my understanding, is Mr. Kruskol. I can determine that the June 2020 data set designates cases referred to the CARRP policy and processed at some point according to the CARRP procedures as CARRP cases. I understand that this is what the data set was intended to show. Thus, there is no overreporting of what USCIS defines as CARRP cases.

C. Assessment of Mr. Kruskol's Four Types of Conclusions

1. Type 1: Mr. Kruskol's Conclusions[18] Concerning Simple Calculations of USCIS Data Unrelated to Majority Muslim status.

Mr. Kruskol presents nine conclusions[19] reporting the analyses he performed on the USCIS data irrespective of majority Muslim status of the applicants' country of birth or citizenship. Mr. Kruskol examines the rate of application referrals to CARRP, the differences between those processed in CARRP and those processed routinely (non-CARRP) in percent pending, percent denied, speed of adjudication, and year by year changes in adjudications.

He reports three conclusions based on comparing the difference in outcomes over the entire time period (FY 2013-2019) for applications in CARRP versus applications not in CARRP. One conclusion (Kruskol supplemental report at paragraph 7a) compares the rate of applications still pending at the end of the period by CARRP status. A second conclusion (*Id*. at paragraph 8a) compares the percent of applications that were adjudicated by CARRP status, and the third

[18] Mr. Kruskol's conclusions are delineated on pages 3 through 5 of his July 17, 2020 supplemental report.

[19] Kruskol conclusions 7a, 7b, 7c, 7d, 7e, 7f, 8a, 8b, and 8c.

conclusion (*Id*. at paragraph 7b) compares the denial rate for applications by CARRP status. In these analyses, Mr. Kruskol inexplicably does not separate the data by form type,[20] though his exhibits present the results by form type.

Basically, Mr. Kruskol's three conclusions report the obvious: that applications referred to CARRP are more likely to be denied than those not referred to CARRP (though most are approved), and that they are processed for a longer time on average, so the average time to adjudication is longer and the percentage still pending is higher. This cluster of Mr. Kruskol's general conclusion is not in dispute (and are the same as mine) and not at issue. But what his conclusions fail to note is that referral to CARRP is a rare event. While Mr. Kruskol notes in his supplemental report that "[o]f the 10,621,174 Aggregate Applications, the Updated USCIS Summary Data shows that 28,214, [only] 0.3% were subject to CARRP,"[21] he omits this from his conclusions. While I agree generally with this particular cluster of his conclusions, I do not believe that most of the statistics he offers for his conclusions constitute a correct statistical analysis (although they are arithmetically correct). Most are meaningless or misleading in addressing the relevant question of the extent of differences in outcomes caused by being processed by CARRP. Mr. Kruskol also fails to review changes, if any, over time and whether that supports the allegation that after the Trump Administration issued its Executive Orders in early 2017, the degree of vetting by USCIS became "extreme," causing alleged delays in adjudications and application denials to increase significantly. Mr. Kruskol never attempts to relate his year over year data comparisons to the issues raised in Plaintiffs' Second Amended Complaint.

[20] I understand that the Court has certified two separate classes based on the two different sets of applications – I-485 adjustment of status applicants and N-400 naturalization applicants.

[21] Kruskol supplemental report at paragraph 22.

Mr. Kruskol's first conclusion (Kruskol supplemental report at paragraph 7a), that applicants subject to CARRP were pending as of September 30, 2019 (the end of FY 2019) at approximately 1½ times the rate of applicants not subject to CARRP, is arithmetically accurate but statistically incorrect, as explained below. His second conclusion (*Id*. at paragraph 7b), that the denial rate of those in CARRP was two times greater than the denial rate of those not in CARRP, is again arithmetically accurate but statistically incorrect. His third conclusion (*Id*. at paragraph 8a), that the time applicants were pending in CARRP was 2½ times longer than the time pending of those not in CARRP, is also arithmetically accurate and yet statistically incorrect.

The problem with the analysis Mr. Kruskol offers for his first conclusion (see his Exhibit AB) is, as my July 17, 2020 amended report explains,[22] that a simple focus on the difference in rates pending confounds two effects: (1) the difference in the percent of cases submitted in a fiscal year which were referred to CARRP versus the routine (non-CARRP) channel, and (2) the difference in the percent of applications referred to CARRP versus the routine (non-CARRP) channel over time. Without separating the two effects, we cannot measure the impact of the difference between CARRP and the routine (non-CARRP) channel in the percent of cases pending at the end of FY 2019. Mr. Kruskol's presentation of this result[23] conflates the effects. One must control for the fiscal year in which an applicant applies. The fiscal year of application receipt is a critical variable if one wants to properly measure the difference in the percent of applications still pending for CARRP-referred applications versus applications in the routine (non-CARRP) channel. This is true whether we compare the number of applications denied,

[22] See discussion on pages 53-56 of my July 17, 2020 amended report.

[23] See Mr. Kruskol's Exhibits AB, AD and AH.

approved, or pending, either overall or for different years. Mr. Kruskol seems to realize this because in his Exhibit BB, BC and BD, he controls for the fiscal year in which the applications were received. But he never tells readers of his supplemental report that his cited analysis is statistically incorrect. The proper analysis is within his Exhibits BB, BC and BD, which are equivalent to Tables 10 and 11 in my amended report, which are explained beginning at page 59.

Mr. Kruskol's second conclusion (*Id*. at paragraph 7b), that those in CARRP are denied at 2½ times the rate of applicants routinely processed,[24] is consistent with Table 8 in my July 17, 2020 amended report, but his second conclusion suffers from the same problem as his first conclusion. Unlike Mr. Kruskol, I stated in my amended report after Table 8 that "one must be cautious in interpreting the data presented in Table 8 and 9," then explained why[25] and pointed to the correct statistical analysis in my Tables 12, 13, 14 and 15.

Mr. Kruskol's third conclusion (Kruskol supplemental report at paragraph 8a) is that the average time to adjudication for applications adjudicated in CARRP is 2½ times longer that the time for adjudication for applications not referred to CARRP.[26] This is consistent with my Table 9. His third conclusion suffers from the same problem as his first two conclusions, a problem explained in my amended report Table 9. One reason the data must be reviewed cautiously is because one should consider when the application is received. Mr. Kruskol seems to partially recognize this issue since his Exhibit AV (using the all-year result as the basis for his conclusions) presents the results separately by the fiscal year in which the applications were received. Despite this step in the right direction, he fails to properly account for the fact that the

[24] See his Exhibit AC, which supports this conclusion.

[25] See discussion on pages 53-56 of my July 17, 2020 amended report.

[26] This is supported by Kruskol Exhibit AV and is replicated by form type in his Exhibits AW and AX.

percent pending among the applications processed through CARRP versus the normal routine (non-CARRP) is different, making his comparison of times to adjudications an "apples and oranges" comparison because the data is censored (truncated) beyond FY 2019. This is referred to as a censored data set since the number of applications received that will be referred to CARRP is censored by the data truncation as of September 30, 2019. That is, all applications that are still pending are ignored as to their later disposition, even though we know that many such applications would be resolved after a longer pending time since most applications are adjudicated (and most adjudicated applications are approved).

To understand these problems, consider the following hypothetical:

Five applications received in FY 2017 are processed through CARRP, and 600 applications received that same year are processed through the routine (non-CARRP) process. Illustration 1 lists outcomes for each case in each fiscal year though FY 2019.

ILLUSTRATION 1

OUTCOMES

	CARRP Applications			Non CARRP Applications		
	Approved	Denied	Pending	Approved	Denied	Pending
FY 2017	1	0	4	200	0	400
FY 2018	0	1	3	200	0	200
FY 2019	1	0	2	0	200	0
End of FY 2019	2	1	2	400	200	0

In this hypothetical, three of the five referrals to CARRP of the applications received in between FY 2017 and FY 2019 (60% of them) were adjudicated by the end of the period (last day of FY 2019), and all applications processed through the routine process between FY2017 and FY2019 were adjudicated by the end of the period. If we restrict the data to studying only

the cases adjudicated (see Illustration 2 below), we see that one-third of both CARRP processed referrals and routine processed referrals are adjudicated in each fiscal year, and the median time to processing is the same for both (FY 2018 is the median year, when half of the applications are adjudicated).

ILLUSTRATION 2

COMPARISON OF MEDIAN TIME TO ADJUDICATION

	Outcomes Restricted Only the Cases Adjudicated CARRP Adjudications		All Outcomes CARRP Adjudications	
	Percent Adjudicated	Cumulative Percent Adjudicated	Percent Adjudicated	Cumulative Percent Adjudicated
FY 2017	33.33%	33.33%	20.00%	20.00%
FY 2018	33.33%	66.67%	20.00%	40.00%
FY 2019	33.33%	100.00%	20.00%	60.00%
End of FY 2019	100%		60%	

	Non-CARRP Adjudications		Non-CARRP Adjudications	
	Percent Adjudicated	Cumulative Percent Adjudicated	Percent Adjudicated	Cumulative Percent Adjudicated
FY 2017	33.33%	33.33%	33.33%	33.33%
FY 2018	33.33%	66.67%	33.33%	66.67%
FY 2019	33.33%	100.00%	33.33%	100.00%
End of FY 2019	100%		100%	

Median time to adjudication (the FY when 50% of all applications being considered have been adjudicated) is indicated by yellow highlight.

However, the result looking only at the adjudicated cases is misleading and makes no sense if examined in isolation as delineated in Illustration 2. It shows that a third of all CARRP and non-CARRP adjudicated applications were adjudicated in each fiscal year, and hence the median

time to adjudication measured as the fiscal year when half of all the adjudicated applications are adjudicated is the same by CARRP status. However, as Illustration 2 shows, if we consider all applications rather than only those adjudicated by the end of FY 2019, we see a different picture. While a third of all applications processed through the routine process (non-CARRP) were adjudicated each year, only 20% of the applications processed through CARRP were adjudicated, with two applications of the five, or 40% remaining unadjudicated by the end of FY 2019. The median time to adjudication[27] is longer for applications referred to CARRP (FY 2019 versus FY 2018).

Moreover, looking at the adjudicated cases only, we see that one-third of all cases (one-third of cases referred to CARRP, and also one-third of all cases not referred to CARRP) were denied. In this hypothetical, all cases routinely processed have been adjudicated, but two of the five CARRP cases (40%) have not been adjudicated. To determine the difference in final denial rates, we must consider what the rates might look like if the remaining two pending cases are later adjudicated (post-FY 2019). Unless both were approved, the approval rate for those referred to CARRP will be lower than the approval rate of the routinely processed applications; but if both are approved, it would be higher. Hence, comparing denial (or approval) rates for adjudicated applications is not accurate when that analysis omits data outside the narrow time period examined (here FY 2017-2019). Unless the percent pending and the likelihood of final outcome of the pending cases is identical, such an analysis is statistically improper; one should

[27] Technically, the data is censored (not all the final outcomes are known). To estimate the true median, one must look at the distribution of all the data, not just the non-censored data. The statistical methodology used to estimate the true median and the difference in the true medians between two groups is a survival analysis. My analyses of the difference in time to adjudication based on the Muslim status of the country of birth presented in Tables 25, 26, and 27 in my July 17, 2020 amended report are survival analyses. It should be noted that a survival analysis can estimate only the true median. It cannot estimate the true mean.

instead compare rates based on all applications as I did in my Tables 10 through 15 in my July 17, 2020 amended report.

Four[28] of Mr. Kruskol's remaining six conclusions focus on the changes between fiscal years in the percent of applications referred to CARRP, and also in the number of applications adjudicated in CARRP. Here, he conducted analyses separately by form type.

In addition, he draws a conclusion[29] about the difference between those processed routinely and those processed in CARRP in regard to the percent of applications taking longer than 180 days to adjudicate, and those adjudicated in the year in which the applicants applied. Mr. Kruskol did not conduct this examination by form type.

His final conclusion[30] is about the difference in the percent of cases still pending at the end of the time period, by the fiscal year in which the application was received among those processed routinely and those processed in CARRP. Turning specifically to what his conclusions were, he concludes that the percent of I-485 applications that were referred for processing in CARRP declined by 13% (Kruskol supplemental report at paragraph 7c), while there was a 15% decline in the percentage of N-400 applications that were referred for CARRP processing (*Id*. at paragraph7e). He gives no reasons why he focuses on the change between FY 2013 and FY 2017 and how it relates to Plaintiffs' allegations. Presumably, this is supposedly relevant to the Plaintiffs' "extreme vetting" allegation, or perhaps an added theme in Plaintiffs' experts' reports (not alleged in Plaintiffs' Second Amended Complaint) about USCIS supposedly altering its handling of CARRP cases in response to the litigation. These conclusions are based

[28] Kruskol supplemental report at paragraphs 7c, 7d, 7e, and 7f.

[29] *Id*. at paragraph 8b.

[30] *Id*. at paragraph 8c.

on analyses (*Id*. at Kruskol Exhibits AF and AJ) which are statistically incorrect and misleading, even while arithmetically accurate. As explained in my July 17, 2020 amended report,[31] one could expect the numbers of applications referred to CARRP to be somewhat reduced compared to earlier years because the time period for possible referral is shortened (since the data is truncated to September 30, 2019, the end of FY 2019). Analyses presented in Tables 1.1 and 1.2 of my amended report statistically adjust the data to remove the impact of censoring data.

Mr. Kruskol also concludes that the largest year to year increase in the adjudications by USCIS of I-485 applications referred to CARRP was a 36% increase from FY 2017 to FY 2018 (Kruskol supplemental report at paragraph 7d), and that the largest year to year increase in adjudications by USCIS of N-400 applications referred to CARRP was a 27% rise between FY 2018 and FY 2019 (*Id*. at paragraph 7f). These conclusions are based on his analyses (Kruskol Exhibits AG and AK) which are arithmetically accurate, but again statistically incorrect and misleading. As discussed above, and detailed in my July 17, 2020 amended report,[32] to properly compare results of the adjudication decision process between fiscal years, the analysis must make an "apples to apples" comparisons, controlling the age of cases pending adjudications during the fiscal year, since the age of the applications adjudicated (which depends on the application flow over time and the prior fiscal year decisions) significantly impacts the likelihood of adjudication. Without controlling for the age of the applications being adjudicated, Mr. Kruskol's analysis remains statistically incorrect and misleading, even if accurate arithmetically. He does not correctly measure the effect of the decision process and therefore cannot measure any change in the actual adjudication decision process over time. The analysis

[31] See Pages 34 and 35 of my July 17, 2020 amended report.
[32] See my discussion on pages 53 through 56 of my July 17, 2020 amended report.

presented in my amended report focused on the change between the adjudication rates immediately before and after the inauguration of the Trump Administration. That analysis is presented in Table 10.1, and the methodology is explained on pages 59-60 of my amended report.

Mr. Kruskol is aware that his analysis which simplistically compares the adjudication rates over time (Kruskol supplemental report Exhibits AG and AK) and his conclusions about the change in adjudications do not accurately address whether this is due to USCIS shortening the time period or a change in the mix of the current and backlogged applications. Hence, Mr. Kruskol does a study of adjudications where he controls for the year in which the applicant applied (*Id*. at Exhibit AY), which led him to conclude that his simple studies presented in his Exhibits AG and AK were misleading and that the apparent increase in adjudications was not due to shorter adjudication processing times but due to changes in the mix of current and backlogged applications being reviewed (*Id*. at paragraph 8b). This conclusion by Mr. Kruskol is consistent with my analysis.

Kruskol's final conclusion of this type (*Id*. at paragraph 8c) is that the percent of each fiscal year's applications still pending increased each fiscal year since FY 2014. While arithmetically correct, it is statistically incorrect and misleading. Given the problem of censorship of the data, this result would be expected. That is, the later the application is received, the shorter the time until September 30, 2019 it has to be adjudicated. That less applications received in FY 2018 are still pending adjudication than applications received in FY 2015 is expected and of no probative value to any issues in this matter.

2. Type 2: Mr. Kruskol's Conclusions Based on Classifying the Birth Country of Applicants by the Population's Majority Muslim Status, and Computing and Comparing Application Referral Rates to CARRP for Applicants Born in a Majority Muslim Country to Applicants Born in Countries Without a Majority Muslim Population.

Mr. Kruskol conducts separate analyses by form type for the FY 2013-2019 period. His conclusion (*Id*. at paragraph 7g) is that I-485 applications submitted by applicants from countries with a majority Muslim population were referred to CARRP at 10.8 times the rate of those from countries without a majority Muslim population,[33] which is comparable to my calculated rate of 10.9 times. His conclusion (*Id*. at paragraph 7h), like mine, is that N-400 applications submitted by applicants from countries with a majority Muslim population were referred to CARRP at 12.2 times the rate of those from countries without a majority Muslim population.[34] See Tables 16 and 17 in my July 17, 2020 amended report. The trivial difference in our results for I-485 applications is due to the slight difference in how we classify countries as having a majority Muslim population. However, while Mr. Kruskol analyzes the data separately by each fiscal year,[35] he reports no conclusions about the patterns over time, though he admits that "[t]hese results are similar for Fiscal Year 2013 through Fiscal Year 2019."[36] This contradicts Plaintiffs' claim that "extreme vetting" has led to an anti-Muslim bias in referring applications to CARRP.

3. Type 3: Mr. Kruskol's Conclusions Concerning the Average Number of Days an Applicant on the Class List Had to Wait for Adjudication.

I did not review the class lists for preparing my July 17, 2020 amended report and focused instead on the FY 2013-2019 data that was also provided to Plaintiffs. However, because Mr.

[33] Calculated from Kruskol's Exhibit AL.

[34] Calculated from Kruskol's Exhibit AN.

[35] See Kruskol's Exhibits Al and AN.

[36] See Kruskol supplemental report paragraphs 45 and 48.

Kruskol's reports included discussion or analyses of the class lists, I did review them to replicate and verify Mr. Kruskol's calculations and analyses. While his calculations are correct, the USCIS data for applications from FY 2013 through FY 2019 remains the proper analysis population for comparing the average wait time to adjudication by CARRP status and/or majority Muslim country status to the larger population of applications. That set of data allows a more complete analysis in terms of the time period studied; and unlike the class lists, it contains information on all applications received since FY 2013 and is not censored (truncated) on the front end. The class lists are censored (truncated) on the front end because they do not include applications referred to CARRP prior to June 21, 2017 if the national security concern was completed before that date. The class lists are limited also because they include only cases that are pending at least six months as of the date that the information for the list was obtained from the databases.

The USCIS data set I analyzed includes data for applications received and referred to CARRP since the start of FY 2013, and their complete history to the end of FY 2019. Hence, it allows one to compute the median time to adjudication of applications submitted in any year and referred to CARRP, and to compare it to the median time for applications submitted in the same year that were not processed through CARRP. More importantly, Mr. Kruskol's analysis does not address Plaintiffs' general averments concerning CARRP and its alleged anti-Muslim animus or impact. The average wait time for those on the class list represents the average wait time for a different population than the population in the USCIS data set. Therefore, a difference only reflects that the wait times of the two different populations are different. The average wait time for the class list may have some informational value as to an individual class member's

individual circumstances, but not for statistical analysis of the issues presented in Plaintiffs' class action complaints.

4. Type 4: Mr. Kruskol's Conclusions Questioning the Accuracy or Completeness of the USCIS Detailed Data Allege that the FY 2013-2019 USCIS Tabular Data is Inconsistent with other USCIS Published Data, and that one USCIS Table is Inconsistent with the FY 2013-2019 USCIS Data Set. He also Raises Questions about the Updated CARRP Indicator.

Mr. Kruskol reaches various conclusions questioning the completeness and accuracy of the USCIS data. In his July 17, 2020 supplemental report, he alleges that the FY 2013-2019 USCIS tabular data is inconsistent with other USCIS published data, and that one USCIS table is inconsistent with the FY 2013-2019 USCIS Data Set. He also raises questions about the flag which identifies which applications should be considered CARRP applications ("CARRP indicator"). In his September 21, 2020 second supplemental report, he claims that the USCIS data overestimates the number of CARRP cases, contains duplicates, and shows cases that are adjudicated within 60 days, which he claims must be in error because cases cannot be adjudicated that quickly according to counsel for Plaintiffs. I assessed each of these conclusions and present my findings below.

Mr. Kruskol's concludes (*Id*. at paragraph 9a) that the mean processing times of Forms I-485 and N-400 are inconsistent with what was reported on the USCIS website.[37] Mr. Kruskol compares his calculation from the USCIS data of the mean processing time separately for each fiscal year from FY 2015 to FY 2018 to that reported on the USCIS website. Mr. Kruskol's cited inconsistency is not actually an inconsistency, but the result of data computations from two sets of data where the populations studied are different.

[37] This conclusion is based on Mr. Kruskol's analyses presented in his Exhibits AP and AQ.

I have learned from a USCIS data analyst familiar with the data that the historic processing time published on the web reflects USCIS' estimated average active case cycle times. This web site data is self-reported from USCIS field offices and service centers. The FY 2013-2019 data set is derived from databases used and maintained in the regular course of business for USCIS operations, including specifically its adjudicative databases and its separate FDNS-DS database.

The different data sets do not represent the same population of adjudications. For example, the web site data excludes visa regression cases, any applications awaiting responses to requests for evidence or notices of intent to deny, and N-400 applications where the beneficiary has a pending N-400 application under re-examination. Additionally, the estimated average active case cycle times on the web site reflect all cases adjudicated regardless of when the application was received, while the USCIS tabular data that I have analyzed and Mr. Kruskol has reviewed is restricted to only applications filed during fiscal years since the start of FY 2013 through the end of FY 2019. The underlying data set used to generate the FY 2013 – FY 2019 tabular data and the web site data do not represent the same set of adjudicated applications. Thus, he is conducting an "apples and oranges" comparison.

Mr. Kruskol also questions whether the CARRP designation for applications included in the USCIS data set is accurate, based on his examination of the data. First, he notes that the update of the data with respect to CARRP designation led to changing the identification of some applications from CARRP to non-CARRP (*Id*. at paragraphs 14 and 15 and Exhibit Z). Since the update was intended to correct a coding error which had incorrectly classified as non-CARRP some applications that were referred to CARRP, Mr. Kruskol questions the reliability of the updated data because a small number of applications previously classified as CARRP-referred applications are now shown as non-CARRP. However, USCIS' data analyst has explained that

USCIS data generally is constantly updated. The updated data set reflected the databases as of when the update took place. For example, data that was updated or corrected for missing or inconsistent data resulted in some applications switching from the CARRP to non-CARRP category, would cause some changes from CARRP to non-CARRP in the updated USCIS data set and tabulations. Such updating of data is common to almost every data system I am familiar with, and the fact that a few data points change is expected and not a cause for concern about the reliability of the data. Second, Mr. Kruskol tabulates the determination of CARRP status based on the date(s) on which an applicant was referred to CARRP and the date(s), if any, on which the national security concern (NS concern) was closed (complete data if any indicated) (*see* his Exhibit AA). Applicants may be referred to CARRP more than once, and each time such applicants were referred to CARRP the date they were referred to CARRP would be reported (CME start date). If the NS concern was closed, the date the NS concern was closed (the CME completed date) would also be reported. Mr. Kruskol maintains that his Exhibit AA raises question as to USCIS' classification to CARRP. In reality, his Exhibit AA and the underlying data demonstrate the logic and correctness of the classification process.

Every application of the 10.6 million in the USCIS FY 2013-2019 data set includes the date an application was received (receipt date), and the date the application was adjudicated or administratively closed (the status date is the date the application was adjudicated or administratively closed or if still pending 9/30/2019). For simplicity in this discussion, the status date is referred to here as the date closed, although for pending cases it is the end of the study period. We also know for each application, for any and all times the applicant was referred to CARRP, the date the national security concern was opened and also the date the NS concern was completed, if completed. Based on this information, we can determine if an application was

processed through CARRP. If a national security concern was open on or after the date an application was received, and on or before the date the application was closed, the application should be identified as a CARRP-referred application. If a national security concern was already opened on the applicant, related to an earlier application[38] from that same applicant, for example, a new application involving the same applicant would be referred for processing through CARRP if the national security concern from the earlier application remained open. Conversely, if no national security concern was open for the applicant while the relevant application was pending, the application should properly be classified as not being processed in CARRP. For example, if the national security concern was closed before the relevant application was received,[39] or the national security concern related to the applicant was opened after the relevant application was adjudicated or administratively closed,[40] the application should properly be classified as not being referred to CARRP. The USCIS designation of whether an application was processed in CARRP is consistent with the logic above. The underlying data on all applicants' CARRP history shows that USCIS' designation of whether an application was processed through CARRP is properly based on the data.

Mr. Kruskol raises two specific questions.[41] First, he asks why a CARRP referred application would have a NS concern opened but not completed, and then asks why a national

[38] This would be indicated in the data by the applicant having a CME Start date before the current application receipt date and either no complete date entry or a complete date after the receipt date of the application. I asked USCIS to verify this. I received an email from Kevin Quinn verifying that this is correct and is the standard practice of USCIS.

[39] This would mean the complete date of the NS concern was before the receipt date of the application.

[40] This would mean the status date (the date the application was adjudicated or administratively closed or if still pending 9/30/2019) was before the opening of the national security concern for the applicant (the CME start date) triggered by another application.

[41] See Mr. Kruskol's supplemental report at paragraph 20.

security concern would be completed after the application's status date. It is possible that if the status date was September 30, 2019, the case and the NS concern were still pending. Also, if the national security concern was completed after September 30, 2019 but before the amended data set was compiled, the completed date would be after the status date. Moreover, the data for national security concerns is person-specific and is not restricted to the specific application being studied. Thus, the NS concern could be opened and completed after the status date of the relevant application because of a different application submitted by the applicant.

Mr. Kruskol claims that the USCIS data appears to contain duplicate applications. However, one cannot determine whether there are duplicate applications from examining only the USCIS data produced. Two records which match on all the variables in the USCIS data may actually still be from different applicants. And Mr. Kruskol ignores the prospect that a data set of 10.6 million applicants is likely to include some different applicants who may apply on the same day who have the same characteristics reported in the USCIS data. For instance, the rate of birth of twins in the U.S. was 1.9% in 1980 and 3.3% in 2010.[42] Among 10,621,174 applications (the number of I-485 and N-400 applications that USCIS received during FY 2013-2019), one might expect approximately 200,000 to 350,000 to be from twins, which would likely match on the 24 factors reported by Mr. Kruskol, or on all 44 factors in the USCIS datafile. Only by examining the personal identifier data as the USCIS data analyst did can one determine if the records are actually from the same applicant. Conferring with the USCIS data analyst, I know that he has examined the purported duplicates that Mr. Kruskol lists in his report and confirmed that none of them involve the same application. Thus, Mr. Kruskol's analysis of so-called

[42] "Three Decades of Twin Births in the United States, 1980-2009," National Center for Health Statistics, NCHS Data Brief, no. 8, January 2012, Centers for Disease Control. www.cdc.gov/nchs/products/databriefs/db80.htm

duplicate applications cannot show that there are any actual duplicate applications in the USCIS data. Moreover, even if one incorrectly accepted the so-called duplicate applications as actual duplicate applications, his analysis does not show any meaningful duplications of applications that would impact his analyses or conclusions, or mine.

The USCIS data contains 44 data fields on 10,621,174 applications. None of the fields in the USCIS data supplied to Mr. Kruskol (or me) contained personal information such as name, birthdate, address, or the USCIS Alien number ("A-number'). Of course, in constructing the data set to be used by Plaintiffs' and Defendants' experts, the USCIS data analyst had access to and used available personal identifying data.[43] Mr. Kruskol incorrectly assumed that if records of applications in the USCIS data supplied to him (and me) had the same characteristic values, they must have come from the same application. That assumption is not reasonable and is confirmed to be wrong based upon the USCIS data analyst's review. It is my experience that it is not uncommon in large databases with many characteristics being reported to see that some small percent of records representing different people may have the same values on all non-personal identifying characteristics. That is why a personal identifying data point or a unique identifier is needed.

Mr. Kruskol claims that there are 213,647 duplicate records in the USCIS databases.[44] This is based on matching records on 27 of the 44 variables[45] in the USCIS database.[46] Mr. Kruskol's analysis of so-called duplicates does not raise any meaningful issue as to the reliability of the database, even if it were a correct measure of duplicate applications (which it is not).

[43] See Shinaberry August 30, 2020 deposition, page 93 lines 1-16.

[44] See paragraphs 24 and 25 of Mr. Kruskol's second supplemental report.

[45] It is not clear whether he matched on all characteristics; his exhibit only listed 27 of the 44.

[46] *Id.* at paragraph 25.

In judging the accuracy of the USCIS database provided to him, which has almost 11 million records, Mr. Kruskol considers any errors no matter how small as making the USCIS database provided unreliable. However, in my experience. every large database contains some errors, especially a database such as the database produced in this matter, which was developed for statistical purposes from the various USCIS databases which are used regularly in business operations where errors are more likely to be detected and corrected. When errors are found, the first question is how frequently the errors occur. The second question is whether there is any reason to believe correcting the errors could change any of the conclusions that are being drawn from the data. For example, all banks are required to report certain mortgage data to the government under the Home Mortgage Disclosure Act (HMDA). This data referred to as HMDA data is widely used to assess trends and fairness in the mortgage market and by specific institutions. The government audits it for accuracy. A sample of each bank's data submission is drawn, and key data elements are tested for accuracy to assure the HMDA data is sufficiently reliable to be used and does not need to be corrected. For large financial institutions, a sample is tested, and key data fields are audited for accuracy; and if the error rate is found to be 2.5% or more, the HMDA data must be corrected and resubmitted.[47] An error rate of less than 2.5% is considered acceptable and does not need to be corrected, since such errors are not considered to make the HMDA data unreliable for analysis. Even assuming that Mr. Kruskol is correct that 213,647 of the 10,621,174 applications are in error (*i.e.*, all duplicates), the error rate would be 2.0%, which would be an acceptable error rate using a government standard.

[47] OCC Bulletin 2017-31/August 25, 2017 Home Mortgage Disclosure Act: updated FFIEC examiner transaction testing guidelines.

However, Mr. Kruskol could only define a duplicate based on the data in the USCIS databases being the same except for the unique identifier. This is inadequate to determine the matching records are really duplicates. I tried to duplicate Mr. Kruskol's results and could not. Matching on the 27 fields listed by Mr. Kruskol in his Exhibit BH, I find only 146,407 potential duplicates, and matching on all the 44 fields in the data except the unique identifier, there are 145,358 instances of potential duplicates. The difference is due to in 815 cases in which the ethnicity was different, 128 cases in which the CME dates were different, and 106 cases in which both were different. Even if we erroneously accept that these are actual duplicates, the rate of duplications is only 1.4%, which is not large enough to raise any meaningful questions about whether the USCIS database is sufficiently accurate to support statistical analysis.

Mr. Kruskol further argues that the USCIS data set receipt date and current status date are not accurate. He bases this on the fact that the data shows some cases where adjudication takes place within 60 days of the receipt of the application, and his presumption (based only upon what Plaintiffs' counsel has told him) that adjudications must take at least 60 days.[48] He presents a tabulation of his findings of adjudications by categories of processing days broken out by CARRP and non-CARRP applications by form type in Exhibit BI of his July 17, 2020 supplemental expert report. I have been told by the USCIS data analyst that some cases are expedited and may be processed within 60 days, but it is rare. Further, the USCIS data analyst has expressed that from his review, it appears that at least a large number of the cases that were processed in under 60 days are recorded accurately, as further review of those cases indicated that they were expedited. I have no further information for determining whether Plaintiffs' counsel or the USCIS data analyst is correct on this point.

[48] Mr. Kruskol's September 21, 2020 second supplemental report at paragraphs 26 and 27.

However, the data shows that overall, among the Form I-485 adjudicated applications, only 0.2% were processed within 30 days, 2% were adjudicated between 31-60 days, and only 3.75% were processed within 60 days. The percentages for Form N-400 applicants are smaller, none within 30 days, 0.2% between 31-60 days, and 0.46% within 60 days. Overall, the percent of I-485 and N-400 cases processed within 60 days is 2.0%. Even if we assume that all such cases are due to errors in the receipt date and/or current status date, they are so infrequent among N-400 cases that they do not raise any meaningful concern as to the reliability of the USCIS data to support the statistical analysis presented in this matter concerning N-400 cases. However, if we assume that no cases are expedited, the error rate using the strict HMDA standards would raise a concern that the adjudication times for a small but possibly meaningful number of I-485 applications is underestimated and could impact a study of adjudication times. But even if the data for cases shown as adjudicated within 60 days of application receipt were in error, almost all of the so-called errors occur numerically and disproportionately among non-CARRP applications. Among Form I-485 CARRP applications, only 25 or 0.36% (well below the 2.5% HMDA standard) occurred within 60 days compared to the 82,958 or 3.70% (above the 2.5% of non-CARRP applications).[49] Hence, if these cases that Mr. Kruskol suspect to be in error were deleted or corrected, the disparity in adjudication times between CARRP applications and non-CARRP applications would only narrow as the mean and median processing times for non-CARRP applications become longer. Hence, researching and correcting any such errors would be to the benefit of the Defendants and not support Plaintiffs' claims.

[49] Among Form N-400 CARRP applications, only one or 0.007% occurred within 60 days compared to 13,612 or 0.459% of non-CARRP applications.

Finally, Mr. Kruskol claims that the USCIS over-reports the number of applications processed in CARRP.[50] This statement is false. Mr. Kruskol appears to be engaging in a policy debate as to how USCIS should define CARRP, rather than conducting a statistical analysis of whether the data USCIS presented is showing what it intended for it to show. As I stated in my July 17, 2020 amended report, the data indicates if an application was referred to CARRP for vetting regardless of whether it was determined as a result of the vetting that the application was not a national security concern and referred back into routine processing.[51] While I agree with Mr. Kruskol that there is no indication in the USCIS data which would allow one to identify and distinguish such CARRP cases from other CARRP cases, the question of the whether the definition of a "CARRP case" should be reclassified as any cases adjudicated while in CARRP is appropriate for experts on USCIS policy in light of the issues in this case and not by a statistical expert. As a statistical expert I can determine if the USCIS definition of a CARRP application is consistently applied (it is) and what the impacts of an application being defined as a CARRP application were.

My review of deposition testimony indicates that the approach taken by the USCIS data analysts who compiled the data is consistent with what USCIS intended to identify: the CARRP cases in the USCIS data set that were referred to CARRP and processed at least in part under CARRP until it was determined via CARRP vetting procedures that the application did not have a link to a national security concern or that the concern had been resolved. The data set produced to Mr. Kruskol and me properly indicates whether a case was processed according to

[50] See paragraphs 6, and 8 through 20 of Mr. Kruskol's September 21, 2020 second supplemental expert report.

[51] See pages 11 and 12 of my July 17, 2020 amended report.

the CARRP policy at all. It did not purport to show how long a CARRP case was processed in CARRP or details about the type of CARRP processing it received.

D. Differences in How Mr. Kruskol, USCIS, and I Defined Certain Variables and the Impact

There are four situations where there is a slight and not meaningful difference in how USCIS, Mr. Kruskol, and I defined certain variables.

First, for determining whether a country has a majority Muslim population, Mr. Kruskol and I used the same sources but handled a few cases differently. These variations caused a trivial difference in our calculations and did not impact our conclusions at all. If I were to use Mr. Kruskol's designations or he used mine, there would be no meaningful difference from our reported results, or in the strength of our conclusions. Specifically, the following table lists all differences in dispute, and my assessment of which is correct. Regardless of the classification used, the number of cases involved is so small that the impact of the choice will not matter.

TABLE R1

ANALYSIS OF THE DIFFERENCE IN HOW COUNTRIES OF BIRTH WERE CLASSIFIED BY SISKIN AND KRUSKOL

Differences in Classification of the Entry for Country of Birth				Number of Cases in Dispute	
Country of Birth Entry	Siskin	Kruskol	Assessment of Difference	I-485	N-400
Stateless	Excluded	Included as non-Muslim	Kruskol Error (1)	34	13
Trust Territory	Excluded	Included as non-Muslim	Kruskol Error (2)	[redacted]	[redacted]
Unknown	Excluded	Included as non-Muslim	Kruskol Error (3)	8,219	10,134
Europe	Included as non-Muslim	Excluded	Kruskol Error (4)	2	0
Eritrea	Included as non-Muslim	Included as Muslim	Debatable	[redacted]	[redacted]
Mayotte	Included as Muslim	Included as non-Muslim	Kruskol Error (5)	[redacted]	[redacted]

Notes

Errors

Kruskol

1 Stateless is not a birth country

2 Trust territory is not a birth country, nor is a country which is under known control

3 Unknown is not a birth country

4 Of the 45 comprising of Europe only Albania, Bosnia and Kosovo are majority Muslim, and they comprise a tiny percent to Europe's population

5 Mayotte is Muslim according to both Pew Research Center and CIA

Debatable: Eritrea is listed by Pew Research Center as 36.5 percent Muslim.
It is listed by Wikipedia as between 36 percent and 51.6 percent Muslim.
CIA does not give Eritrea's percent Muslim.

Second, with respect to adjudications, both the USCIS tables and my July 17 amended report treated any decision (except an administrative closure) as an adjudication. Mr. Kruskol also excluded withdrawn applications when studying adjudications, since he attributes a withdrawal decision to the applicant and hence that case is not adjudicated by USCIS. However, to be consistent with USCIS' general reporting practice of considering such cases as adjudicated, I did not exclude these cases. Since the number of withdrawals is so small (less than 0.1%), inclusion or exclusion makes only a trivial difference in the calculations.

Third, to be conservative in defining denials, USCIS' tabular analysis included revoked, [redacted], and withdrawn cases, which is USCIS' normal practice. Mr. Kruskol considered only adjudications labeled denials as denied. When using the tabular results, I used the USCIS definition; but when the study of denials was based on the underlying data set (analyses of denials in my Tables 13 and 15), I used only the cases labeled as denied. Since the number of cases that were revoked, [redacted] or withdrawn is so small (less than 0.1%), it will make only a trivial difference in the calculations, regardless of whether they are considered denials.

Lastly, in computing the number of applications still pending[52] at the end of the FY 2013-2019 period by applications received in a fiscal year, both of us counted the cases that were marked pending in the USCIS data set and subtracted cases flagged in the USCIS data as indicating the application may have been adjudicated. I deleted these cases from the number of applications still pending, and also from the count of applications received. Since I assumed we do not know the correct outcome, I deleted these cases from the number of applications still pending and from the count of applications received. But Mr. Kruskol did not make this deletion, presumably because he assumed that all were not pending. An additional difference is that I considered all applications received, but Mr. Kruskol did not count the applications received on September 30, 2019, the last day of the period. The number of cases received on the last day, and the number of cases in conflict regarding pending status, are both so small that regardless of whether the percent pending computation is based on Mr. Kruskol's method or mine, the difference is trivial in all fiscal years but FY 2019; at most, it affects the percent in the

[52] See my Tables 10 and 11, and Mr. Kruskol's Exhibit BC and BD.

first decimal place. Even for FY 2019, the difference would be small (a few percentage points) and not meaningful.

III. RESPONSE TO DR. SAGEMAN'S EXPERT REPORT

A. Overview of Analysis of Dr. Sageman's Expert Report

I have been asked to review Dr. Sageman's July 1, 2020 report and his August 7, 2020 responsive report. I first review his July 1, 2020 report and then his August 7, 2020 responsive report.

In reviewing Dr. Sageman's July 1, 2020 report, I have restricted my assessment to his various statements and conclusions discussed below that warrant a statistical analysis. Dr. Sageman makes various assumptions about the CARRP process. To the extent these assumptions are critical to his conclusions, I note them but express no opinion as to whether they are accurate.

B. Conclusions Concerning Dr. Sageman's July 1, 2020 Expert Report

1. Dr. Sageman argues that almost no one is a national security "threat" or will participate in terrorism, so almost all those referred to CARRP are not national security "threats," and hence the CARRP program has a very high false positive rate. He argues that the purpose of CARRP is to identify national security "threats," and that since such threats are rare, CARRP should be eliminated. From a statistical perspective, this is not correct if one assumes that those referred to CARRP are more likely to be a national security "threat."
2. Dr. Sageman argues that an unknown error rate in CARRP referrals justifies abandoning CARRP. There is no statistical basis for this argument. The error rate of those screened is very high in many statistically rational and valid screening practices.

3. Dr. Sageman argues that the "near-total elimination of the threat of terrorism is an impossible scientific or law enforcement standard to achieve, and results in a system of incentives that encourages the generation of false positives." There is no statistical evidence to support the degree to which if any of this impacts the CARRP policy. Very few applications are referred to CARRP. If the goal were to eliminate the threat of terrorism at all costs, and if a more stringent CARRP process would be more likely than the routine process to identify national security threats, then it is the routine process that should be eliminated, not CARRP.

C. Basis for Conclusions Concerning Dr. Sageman's July 1, 2020 Report

Dr. Sageman assumes that the sole purpose of CARRP is to "predict" which immigration benefit applicants are likely to commit acts of terrorism currently or in the future.[53] He labels such applicants as national security "threats" in his report.[54] He further assumes that USCIS commits an "error" when it refers applicants "who pose no risk to national security" to CARRP, and he characterizes such referrals as "false positives."[55] He concludes that almost all applicants referred to CARRP are incorrectly referred because virtually none of them are national security "threats."[56] Hence, he suggests that CARRP is an unreliable process that should not be used because of what he characterizes as a high "false positive" rate.[57] His reasoning can be summarized as follows: (1) the probability (or "base rate") that a randomly-selected individual is

[53] Sageman July 1, 2020 report at paragraphs 11, 84, 90, 93, and 98

[54] *Id.* at paragraphs 45, 47, 51, 62, 98.

[55] *Id.* at paragraphs 89 and 93.

[56] *Id*. at paragraphs 56-59 and 63-65.

[57] *Id.* at paragraph 104.

a national security "threat" is "extremely low;"[58] (2) an individual's placement in the United States Government's Terrorist Screening Database (TSDB) is not a reliable indication that a person is a national security "threat,"[59] and similarly a USCIS officer's identification of someone as a national security concern is not a reliable indication that a person is a national security "threat;"[60] and (3) the process for referral to CARRP will therefore have a very high "false positive" rate.[61] (*Id*. at paragraphs 83-88, 93-94, 99-101, and 104)

Even assuming that Dr. Sageman's concept of "error" is correct, two types of error can occur when USCIS determines whether or not to refer an application to CARRP: (1) USCIS refers an applicant to CARRP who is not a national security "threat" (false positive), or (2) USCIS fails to refer an applicant to CARRP who is a national security "threat" (using Dr. Sageman's terminology) (false negative). In statistical terms, reducing one type of error will necessarily increase the other when all other variables remain constant. That is, the two errors are inversely related. A perfect screening process would eliminate both types of errors. However, if a perfect process is not possible, USCIS has to weigh the costs posed by false positives against the costs posed by false negatives in deciding whether or not to refer an application to CARRP.

Dr. Sageman implicitly acknowledges that he cannot accurately measure what he describes as the "false positive" error rate in CARRP because virtually no baseline exists for determining

[58] *Id*. at paragraphs 62-63.

[59] *Id*. at paragraph 84.

[60] *Id*. at paragraphs 90-91.

[61] Dr. Sageman spends much of his report challenging the reliability of the TSDB and USCIS's separate assessment of whether an applicant who is not on the TSDB might be a national security "threat" (using Dr. Sageman's terminology). However, he never points to a more reliable source or method for USCIS to use in identifying applicants who may be national security "threats."

whether someone is a national security "threat."[62] Although Dr. Sageman acknowledges that he cannot calculate the false positive rate in CARRP, he nevertheless assumes that the false positive rate is very high, and he then leaps to the conclusion that the high false positive rate justifies abandoning the CARRP process.[63]

With respect to Dr. Sageman's argument that an unknown error rate in CARRP referrals justifies abandoning CARRP, there are many situations where people make preliminary judgments based on information with an unknown error rate. One common approach in such cases is to look at the characteristics of persons of interest and focus attention on persons with those characteristics.

For example, police investigating a crime will ask witnesses to provide a set of characteristics describing the perpetrator (*e.g.*, white male, over six feet tall, dark hair, about 200 pounds). Police then narrow their search to persons who fit the description (or perhaps most of the description, to allow for data error) and investigate persons fitting the description. Clearly, just meeting the description does not mean that a specific individual is the perpetrator. Nevertheless, it is reasonable to assume that limiting the investigation to these individuals rather than investigating random persons will increase the likelihood of finding and arresting the perpetrator. However, it will also lead to interviewing many persons who are not the perpetrator

[62] See Dr. Sageman's statements criticizing USCIS as using an "extremely low threshold … for identifying 'national security concerns'", in paragraph 12 of his report, "By any measure, the base rate of violent terrorist attacks is extremely low." in paragraph 62 of his report , and "[T]he relevant base rate is exceptionally low. … [T]he base rate of acts of political violence in the United States is so low that any attempts to predict who is likely to carry out such violence would have to rely on extremely accurate indicators – which for political or terrorist violence simply do not exist – in order to avoid a flood of false positives." in paragraph 88 of his report.

[63] *Id*. at paragraphs 83, 87, 93 and 98-100.

(*i.e.*, the process will have a high false positive rate), though far less than if a random process were used.

Although logical and appropriate for investigation purposes, this strategy is not scientifically valid or reliable for the later task of making final determinations for a prosecution or conviction – as distinguished from shifting the investigatory focus from random persons to those more likely to be the perpetrator based on the description provided. We have no estimated error rate because we may not know the percent of persons in the area who meet the description of the perpetrator. However, moving beyond investigation to prosecute and convict someone based solely on meeting the description would be inappropriate. Dr. Sageman illustrates this type of error in paragraphs 49-59 of his report.[64] But it is logically and statistically correct to use such a strategy to reduce the population of applications needing a more stringent vetting process, *i.e.*, the investigation process rather than the ultimate adjudication of an application.

[64] There is a famous law case where the perpetrators of a crime in Los Angeles were described as a black male and a blonde white female riding in a yellow convertible. The police found such a couple riding in a yellow convertible and, based almost solely on matching the description, the couple were arrested and tried for the crime. The evidence put to the jury was the probability of finding a couple that matched the description of the perpetrators driving a car that matched the description of the suspect vehicle. A statistician testified that the probability that one could find a car with a couple in it fitting the perpetrators' description was one in a million. That is, out of a million cars in Los Angeles, only one would be a yellow convertible with a black male and a blonde white female in it. The jury convicted, but the verdict was overturned on appeal when a statistician testified that there are more than five million cars in the Los Angeles area, so we would expect there to be five cars in that metropolitan area that would be expected to fit the description. Thus, the probability that the defendant was actually the perpetrator would be 20%, since only one of the five cars contained the perpetrators. Note that stopping the car was the correct decision for the police, but bringing criminal charges and trying the people without further evidence that they were the perpetrators was the wrong decision. Similarly, USCIS does not adjudicate an application simply based on an applicant having been referred to CARRP. The ultimate decision to approve or deny is based on the actual totality of evidence from the vetting process (as is indicated by the fact that the immigration benefits of approximately 80% of all persons whose applications are referred to CARRP are approved).

Now, let us turn to Dr. Sageman's argument that CARRP should be stopped because it allegedly has a high false positive rate, and that USCIS cannot show statistically that referring an applicant to CARRP is a reliable and valid predictor that the applicant is a national security "threat" (Dr. Sageman's substitution for CARRP's focus on national security "concerns"). For Dr. Sageman to be correct, one must make the unwarranted assumption that those referred to CARRP, as a group, are no more likely to be national security "threats" than those not referred, despite USCIS' receipt of information indicating that the referred applicants are national security concerns. However, if we assume, based on the criteria for referral to CARRP (such as being identified as a known or suspected terrorist in the TSDB), that those referred to CARRP are more likely to be national security "threats" (using Dr. Sageman's terminology) than persons not referred to CARRP, then Dr. Sageman is incorrect. Ending CARRP would eliminate what Dr. Sageman has defined as false positives at the expense of maximizing false negatives. If one accepts that the cost of false negatives is greater than the cost of false positives, then choosing to eliminate false positives at the cost of increasing false negatives is foolish. That is, using the identification of a "national security concern" to address the prospect that one might pose what Dr. Sageman characterizes as a national security "threat," and winnowing the population of applicants needing a greater degree of scrutiny, is reasonable and statistically appropriate.

To illustrate this argument, we know that approximately three in every 100,000 persons in the United States have tuberculosis (TB). Detecting TB is very important since early treatment can be very effective and eliminate the spread of the disease. The cost of not identifying someone who has TB, which could lead to death and an epidemic, vastly outweighs the cost of incorrectly flagging someone as possibly having TB. One way to precisely determine if a person has TB is to X-ray the individual. Therefore, one could argue that we should X-ray all people to

determine who has undetected TB and needs treatment. However, this project would have large economic and logistical costs and may not be possible. Moreover, X-rays are not benign, and irradiating large numbers of persons without TB would cause collateral harms to public health.

Alternatively, there is an inexpensive and easy skin patch test available which, let us assume, has the following error characteristics. If you have TB, it will show positive (true positive) 98% of the time, so the false negative effect is 2%. If you do not have TB, it will show positive (false positive) 10% of the time. We give the test to an entire population of 1,000,000 people, including 30 who have TB and 999,970 who do not. Out of the 30 with TB, we get 29 true positives based on the expectation of 98% true positives. Out of the 999,970 without TB, we get 99,997 false positives, based on the 10% false positive rate. Thus, for the 100,026 people testing positive for TB (29 who had TB and another 99,997 who did not have TB), the false positive rate is 99,997/100,026, or 99.97%.

Clearly, this test with a 10% false positive rate produces a huge number of false positives when administered to a very large population in which having TB is rare, and it would be incorrect to assume that everyone with a positive test result has TB since only 0.03% among those testing positive actually have TB. For the 100,026 cases showing a positive result, one possible procedure would be to follow up and do better testing (such as an X-ray). Although the patch test is not a reliable predictor of who actually has TB, it does dramatically reduce the search process from one million down to 100,026, thus reducing by 90% the cases needing more intensive testing, at a cost of missing only one TB case (*i.e.*, one false negative) among 30 in a population of one million.

Similar to the patch test, USCIS uses the CARRP policy to reduce the number of persons they need to subject to greater scrutiny for national-security-related concerns – though with a

much sharper reduction, by about 99.73%. Thus, even accepting Dr. Sageman's premise that the purpose of CARRP is to predict which applicants are likely to commit terrorist attacks, if the probability of being a national security "threat" (using Dr. Sageman's terminology) is higher among those in CARRP than among those not in CARRP, and if the expected cost of false negatives outweighs the cost of false positives, then the CARRP program is statistically valid.[65]

Dr. Sageman argues that the "near-total elimination of the threat of terrorism is an impossible scientific or law enforcement standard to achieve, and results in a system of incentives that encourages the generation of false positives." (*Id*. at paragraph 84.) He may be correct that eliminating terrorism is a goal that is impossible to meet. But the data does not support his premise that the goal of eliminating terrorism has resulted in generating a large number of false positives. If one's goal were near total elimination of the threat of terrorism at all costs, then all cases would be processed through CARRP. However, making the CARRP process the normal process may not be economically or logistically feasible, just like X-raying all Americans to eradicate TB is not economically or logistically feasible. Given the small percent of applications referred to CARRP (one in every 375), there is little empirical support for the allegation that the desire to totally eliminate terrorism is generating a huge number of false positives.

D. Overview of Analysis of Dr. Sageman's Responsive Report

Dr. Sageman's responsive report dated August 7, 2020 presents his response to my study of the relationship between the level of terrorist activity in a country measured by the Global Terrorism Database (GTD), whether a country is a state sponsor of terrorism, the number of

[65] That however does not mean that another screening process may not be better, but I am not aware that Plaintiffs have offered a better screening approach or any screening approach.

applications from a country, and the percent of the population of a country that is Muslim, with the number of referrals to CARRP. He criticizes the analysis because it relies on the GTD which he claims is not a reliable source. He claims that the GTD (i) conflates terrorism acts with insurgency or civil war, (ii) is not a consistent methodology for characterizing events, and (iii) is infected with political bias. He further argues that the designation of a country as a "state sponsor of terrorism" is political, and refers only to acts by the state, and holds no predictive or probabilistic value in assessing the propensity of individuals to be a terror threat.

E. Conclusions Concerning Dr. Sageman's August 7, 2020 Responsive Report

1 Dr. Sageman argues that the purpose of CARRP is to "predict" which immigration benefit applicants are likely to commit acts of terrorism currently or in the future, and he further argues that the GTD is incapable of doing that. As discussed in Section C above, I disagree with his definition of the purpose of CARRP, which appears to differ from CARRP policy documents.

2 Dr. Sageman argues that my analysis should not have considered whether a country is designated as a "state sponsor of terrorism" because of political bias in the designation. While not accepting Dr. Sageman's view nevertheless, I have removed the designation from my analysis. Its removal only strengthens my findings.

3 Dr. Sageman argues that the GTD is unreliable and politically biased in its count of terrorist events (at least against countries with a high Muslim population percentage) and, hence, should not be used. As explained in my amended expert report, no database is perfectly accurate, and random error only results in underestimating the correlation between the GTD data and referrals to CARRP.

If one accepted Dr. Sageman's premise that the GTD is politically biased so it overcounts terrorist events in countries with a high Muslim population percentage, such a bias could confound the correlation between the number of terrorist attacks reported in the GTD, the percent of a country's population which is Muslim and referral to CARRP. My analysis suggests that any political bias does not have a very significant impact, and that a perfectly accurate GTD would likely confirm my findings. That is, the data does not support the conclusion that the regression results based on GTD are the result of political bias resulting in inflating the count of terrorist events in countries whose population is highly Muslim.

F. Basis for Conclusions Concerning Dr. Sageman's August 7, 2020 Responsive Expert Report

In my July 17, 2020 amended report, I addressed two problems Dr. Sageman raises with the GTD[66]: (i) that the GTD may conflate terrorist acts with insurgency or civil war, and (ii) that the GTD may be incomplete. I pointed out that random errors do not create correlations; they mask them. Moreover, I noted that to the extent inaccuracies are not random, one would expect the correlation between the GTD data and referrals to CARRP to be underestimated, since the countries with the largest numbers of referrals tend to be more authoritarian and underdeveloped, which would limit the amount of reported data on incidents of interest.

However, in his responsive report, Dr. Sageman raises three new issues. One is an extension of his argument that the purpose of CARRP is to "predict" which immigration benefit applicants are likely to commit acts of terrorism currently or in the future, and that the GTD is

[66] See pages 114 -117 of my July 17, 2020 amended report.

incapable of doing that. As discussed in Section C above, his definition and understanding of the purpose of CARRP appears to be at odds with CARRP policy documents. However, the other two issues he raises are more interesting. One is that the designation of a "state sponsor of terrorism"[67] is political and does not relate to terrorist activities within the country and therefore should not be used and may unduly influence the regression. The second is that there is political bias in the GTD decisions of what data to collect and how to classify it.

His first concern can be directly addressed. To test if the inclusion of the factor flagging whether a country is a "state sponsor of terrorism" impacts my regression findings, we can simply remove the variable "state sponsor of terrorism" and see what happens to the regression results. Table R2 shows that removing the "state sponsor of terrorism" variable does not alter the unstandardized coefficients[68] on the impact of GTD and the percent Muslim of population of the country. If the countries[69] that were designated as a "state sponsor of terrorism" are not differentiated by the variable "state sponsor of terrorism," then the model does not fit as well, and more significantly, the impact of the number of terroristic events on referrals to CARRP increases (although not by a statistically significant amount) and the impact of the percent Muslim of the population of the country decreases (although not by a statistically significant

[67] Dr. Sageman criticizes me for not citing my source of the countries which are state sponsors of terrorism, but notes that the list of "state sponsors of terrorism" is compiled and maintained by the U.S. Department of State. It is that same list that I relied upon. See https://www.state.gov/state-sponsors-of-terrorism/.

[68] I present the unstandardized coefficients rather than the standardized coefficients because I am focused on the effect of the removing variables or observations on the coefficient, not on the effects between coefficients. When comparing coefficients of different variables, one must standardize to adjust for the difference in the scale of the different variables. However, if one is comparing coefficients of the same variable, then unadjusted variables are appropriate.

[69] Four countries (North Korea, Iran, Syria and Sudan) are currently considered 'state sponsors of terrorism." However, the data contains no applications from North Korea, so the designation for only three countries is removed from the regression model.

amount). Hence, Dr. Sageman's concern that accounting for the designation of a country being a "state sponsor of terrorism" affects my analysis is factually unfounded.

TABLE R2

EFFECT ON REGRESSION COEFFICIENTS OF REMOVING VARIABLE DESIGNATING A COUNTRY AS "STATE SPONSOR OF TERRORISM"

Variable	Full Model Unstandardized Coefficient	Without State Sponsor of Terrorism Variable
Number of terroristic events associated with country	0.177**	0.181**
Percent Muslim of population of the country	1.211	1.031

*Excludes data from Afghanistan, Iraq, Syria and Somalia
** Statistically significant at 0.05 level

Addressing his second question is more difficult. To the extent that there are random errors in the GTD data, such as underreporting from authoritarian states or underdeveloped countries, and to the extent that these characteristics are correlated with the percent Muslim of a country, the correlation I found between GTD and referral to CARRP would be understated. While Dr. Sageman notes that the data reporting may have random errors, he also contends that the GTD data is politically biased such that countries that have a higher Muslim population percentage will have a higher number of terrorist incidents reported. This would confound the correlation between the number of terrorist incidents and the percent Muslim population with CARRP referrals. The true extent of these factors cannot be precisely measured, since that would require having a perfect terrorist activity database and measuring the difference in the regression results with the correct data and the biased data.

To illustrate such bias, Dr. Sageman claims that the GTD incorrectly counts as terrorist attacks all attacks carried out during an insurgency or civil war such as those in Afghanistan, Iraq, Syria or Somalia, all of which have a very high Muslim population percentage. Hence, he suggests that "terrorist events" reported in these countries are incorrectly significantly inflated due to political bias (or inability to separate insurgency or civil war events from terrorist events). While we cannot determine which events should be counted, we can see what effect it has on the regression results simply by removing these countries from our analysis. This will give us some insight into how these potential political biases might actually impact the regression findings.

Therefore, I removed these four countries from the analysis[70] and found that both the coefficient associated with the number of terroristic events and the coefficient associated with the percent Muslim of the population of the country decrease, although neither to a statistically significant extent (see Table R3 below). Thus, we see that the impact of political bias appears to lightly inflate the correlations of both the coefficients. It is interesting to note that the net effect of adjusting for political bias in the data and removing the designation of whether a country is a "state sponsor of terrorism" had no effect on the estimate of the impact of being referred to CARRP of the number of terroristic events reported in the GTD, but it did reduce the estimate of the impact of the percent Muslim of the population of the country by approximately 25%. While one cannot say with certainty, these analyses indicate that the conclusions would not be different if a country's designation as a "state sponsor of terrorism" was not considered and the GTD counts of terrorist events were totally accurate.

[70] I also omitted the variable "state sponsor of terrorism" for continuity.

TABLE R3

EFFECT ON REGRESSION COEFFICIENTS OF REMOVING VARIABLE DESIGNATING A COUNTRY AS "STATE SPONSOR OF TERRORISM" AND REMOVING THE FOUR COUNTRIES FOR WHICH DR. SAGEMAN CLAIMS THE DATA IS BIASED

Variable	Full Model Unstandardized Coefficient	Without State Sponsor of Terrorism Variable	Excluding Four Countries Data* and Without State Sponsor of Terrorism Variable
Number of terroristic events associated with country	0.177**	0.181**	0.177**
Percent Muslim of population of the country	1.211	1.031	0.914

*Excludes data from Afghanistan, Iraq, Syria and Somalia
** Statistically significant at 0.05 level

Finally, it should be noted that an underlying premise of Dr. Sageman's criticism of the Global Terrorism Database, and by extension my analysis, is that incidents that might involve insurgency or civil war should never be included as incidents of terrorism, and inferentially would never warrant investigation for a national security concern. But USCIS, and not Dr. Sageman, has responsibility for CARRP and determining whether an incident that might ultimately be recharacterized as involving insurgency or civil war ever warrants investigation for an Immigration and Nationality Act (INA) related national security concern.

IV. RESPONSE TO MR. RAGLAND'S EXPERT REPORT

A. Overview of Analysis of Mr. Ragland's Expert Report

I have been asked to address certain statistical aspects of the June 30, 2020 report of attorney Thomas Ragland as Plaintiffs' designated expert. Specifically, I have been asked to address the statistical issues relating to Mr. Ragland's opinions that the CARRP adjudication rates were very low prior to Plaintiffs filling this lawsuit in early 2017, and that the increase in CARRP adjudication rates is due to the filing of this lawsuit (Ragland Supplemental Expert Report paragraphs 105-109) and his method of estimating the number of his clients which were processed in CARRP. In his report, Mr. Ragland states that he has handled more than 50 CARRP cases.[71] However, he does not actually know which of his clients, if any, were adjudicated via CARRP. He says he estimates whether someone was adjudicated in CARRP by "tell-tale" signs.[72]

B. Conclusions Concerning Mr. Ragland's Expert Report

1 Mr. Ragland is simply wrong in claiming that there is any valid statistical evidence that supports an inference or conclusion that the increase in the number and rate of applications adjudicated after the Plaintiffs filed this lawsuit occurred in response to the filing of this lawsuit. He is correct that the number and rate of adjudications increased. However, there is no statistical evidence that USCIS shortened the adjudication processing time as a result of the lawsuit, which is the only way USCIS could cause the simple increase in adjudications and

[71] In his deposition he estimates this is about 5% of all his clients. Ragland September 18, 2020 Deposition p. 233, ln. 2-12.

[72] *Id.* at p. 81, ln. 16-22; p. 82, ln. 1; p.114, ln. 8-22; p. 115, ln. 1-7.

adjudication rates. In fact, both Mr. Kruskol and I concluded that the increase in the number and percent of cases adjudicated was not due to a change in USCIS' processing time of CARRP cases, but was due to the change in the number and age (time from application receipt) of the applications being reviewed.

2 Mr. Ragland is presumably basing his identification of CARRP cases on his observation of Muslim clients who were denied, and categorizing them as having been adjudicated via the CARRP process based on the presence or absence of certain "tell-tale" signs. There is a significant problem with the reliability of Mr. Ragland's "tell-tale" sign estimation of CARRP cases. First, as Mr. Ragland acknowledged, the "tell-tale" signs are not unique to persons in CARRP.[73] Second, the overwhelming number of applicants from Muslim majority countries are not processed in CARRP. Therefore, the estimation is very likely to have a high false positive rate. Thus, Mr. Ragland's reliance on "tell-tale" signs will likely significantly overestimate the number of his clients that were processed in CARRP.

C. <u>Basis for Conclusions</u>

Mr. Ragland's assertion (at paragraphs 105 and 108) that CARRP adjudication rates rose sharply (*i.e.*, more than doubled) as a result of Plaintiffs filing this lawsuit is simply wrong. While the rates rose sharply, the rise was not the result of Plaintiffs filing this lawsuit. As I discuss in detail above in my review of Mr. Kruskol's amended report, any assessment of purported changes in the adjudication process in FY 2017 must consider the significant

[73] *Id.* at p. 71, ln. 5-11; p. 80, ln. 3-22; p. 81, ln. 1-6; p. 133, ln. 1-8; p. 147, ln. 1-12; p. 157, ln. 18-22; p. 215, ln. 2-22; p. 216, ln. 1-2.

increasing trend in applications referred to CARRP in earlier fiscal years (pre-FY 2017), and the fact that the adjudication decision process in a fiscal year is impacted by how long the application has been in processing. I discussed this problem in my amended report at pages 59-60. Moreover, I statistically assessed whether or not the number of adjudications actually changed in the two year period FY 2017 and FY 2018, or FY 2018 alone (which is the first full fiscal year after the filing of this lawsuit), from the number of adjudications in FY 2016 (which is the last full FY before the filing of this lawsuit). My analysis showed that the number of adjudications that occurred in either the two-year period or in FY 2018 was less than would have occurred if the decisions to adjudicate pending applications in CARRP were identical to those decisions in FY 2016, the last full FY prior to the filing of this lawsuit (see Table 10.1 of my amended report).

Moreover, Exhibits AG and AK of Mr. Kruskol's supplemental report are misleading (as explained in depth above in my assessment of Mr. Kruskol's supplemental report) in that they do no indicate whether any of the change was caused by USCIS changing the processing time of adjudicating cases. It is evident that Mr. Ragland was misled by these exhibits. These Kruskol exhibits present simple descriptive statistics which, taken on their face with no statistical assessment, led Mr. Kruskol to conclude that the largest year over year increase in I-485 applications occurred between FY 2017 and FY 2018 (Kruskol's supplemental report paragraph 7d) and the largest year over year increase in N-400 applications occurred between FY 2018 and FY 2019 (*Id*. at Paragraph 7f). Mr. Ragland similarly based his conclusions on the simple descriptive statistics of adjudications and adjudication rates by fiscal year. However, Mr. Kruskol, unlike Mr. Ragland, was aware that these simple descriptive statistics do not show why these increases were taking place. Based on another analysis of adjudications controlling for the

fiscal year of the application being reviewed (*Id*. at Exhibits AZ and BA), Mr. Kruskol concluded that the increases between years in the amount of adjudications was not due to USCIS shortening the adjudication processing times, but rather due to changes in the number and age of applications (*Id*. at Paragraph 8b). Thus, looking at the same data as Mr. Ragland, Mr. Kruskol came to the exact opposite conclusion as Mr. Ragland. Mr. Ragland's conclusion that the data shows that USCIS shortened the processing time of CARRP applications in response to the filing of this lawsuit is simply wrong.

Mr. Ragland is presumably basing his identification of CARRP cases on his observation of Muslim clients who were denied, and categorizing them as having been adjudicated via the CARRP process based on the presence or absence of certain "tell-tale" signs. There is a significant problem with the reliability of Mr. Ragland's "tell-tale" sign estimation of CARRP cases. First, Mr. Ragland acknowledges that his "tell-tale" signs are not unique to persons in CARRP.[74] Second, the overwhelming majority of applicants from Muslim majority countries are not processed in CARRP. Therefore, the Ragland estimation is very likely to have a high false positive rate. Thus, Mr. Ragland's reliance on "tell-tale" signs will likely significantly overestimate the number of his clients who were processed in CARRP.

I have no actual data to determine the extent to which these "tell-tale" signs actually occur among those not processed via CARRP, or the likelihood of such "tell-tale" signs occurring among those actually referred to CARRP. However, while it is probable that they were significantly less likely to occur among applicants not in CARRP because of the very small number of applicants who were processed in CARRP, it is my opinion that his methodology is

[74] *Id.* at p. 71, ln. 5-11; p. 80, ln. 3-22; p. 81, ln. 1-6; p. 133, ln. 1-8; p. 147, ln. 1-12; p. 157, ln. 18-22; p. 215, ln. 2-22; p. 216, ln. 1-2.

not a reliable predictor that a specific applicant actually was processed in CARRP and will markedly overestimate the number of his clients processed via CARRP.

To illustrate the problem, let us consider the "length of time until denial," one of his "tell-tale" signs. I looked at the data related to length of time until denial. I cannot specifically measure delay to adjudication from the date of interview, but can study how long a denied applicant had to wait from their application date to the adjudication of the application. Thus, I computed the percent and number of applications denied whose adjudication was delayed more than one year, two years, or three years by form type and by whether the application was a CARRP or non- CARRP case. In doing so, I restricted the data to applications received before FY 2017, so they could have been delayed for at as much as three years.

ILLUSTRATION 3
EXAMPLE OF THE USE OF A "TELL-TALE SIGN" TO IDENTIFY CARRP APPLICANTS WOULD BE MISLEADING

Form I-485

Tell Tale Days to Deny Applications	Percent of all Denied Apps CARRP	Percent of all Denied Apps non-CARRP	Denied Apps CARRP	Denied Apps non-CARRP	Percent Denied Apps in CARRP
at least 1 day	100%	100%	1,087	158,762	0.7%
at least 365 days	84.9%	43.4%	923	68,970	1.3%
at least 730 days	54.0%	17.7%	587	28,051	2.0%
at least 1095 days	24.3%	7.5%	264	11,872	2.2%

Form N-400

Tell Tale Days to Deny Applications	Percent of all Denied Apps CARRP	Percent of all Denied Apps non-CARRP	Denied Apps CARRP	Denied Apps non-CARRP	Percent Denied Apps in CARRP
at least 1 day	100%	100%	2,169	275,576	0.8%
at least 365 days	83.2%	20.7%	1,804	57,022	3.1%
at least 730 days	45.9%	5.5%	996	15,293	6.1%
at least 1095 days	20.2%	2.5%	439	6,981	5.9%

The data presented in Illustration 3 above. shows that the percent of CARRP cases delayed is markedly higher than the percent of non-CARRP cases. Hence, a longer time to adjudication is a sign that a person's application was more likely subject to CARRP than that of a person whose application had a shorter time to adjudication, to some extent fitting Ragland's notion of a "tell-tale" sign. For example, more than 80% of all CARRP applicants whose applications for naturalization were denied had to wait 365 days before adjudication. However, because the overwhelming number of denied applicants are non-CARRP (99.2%), the likelihood of being a CARRP application among those taking more than two years, to be denied is still very small. This is because a large percent of a small number is often much less than a small percent of a large number. Thus, such cases with this or any "tell-tale" sign which occur among both CARRP and non-CARRP applicants cannot reasonably indicate that an application is actually a CARRP application unless the occurrence of such a "tell-tale" sign among non-CARRP applicants is extremely rare. Furthermore, use of such estimation would grossly overstate the number of applicants processed actually referred to CARRP.

V. RESPONSE TO MR. GAIRSON'S EXPERT REPORT

A. Overview of Analysis of Mr. Gairson's Expert Report

At Defense counsel's request, I have reviewed and respond here to statistical issues raised in paragraph 104 of the supplemental report by Plaintiffs' designated expert, immigration attorney Jay Gairson. Mr. Gairson opines that the estimated relative difference between an individual listed in the TSDB and any other individual in the world is only 1.43 persons, relying solely on the article "How Many People Do You Know?: Efficiently Estimating Person Network Size." by McCormick, Salganik, and Zheng. Mr. Gairson concludes that there is a near mathematical certainty that one can link any applicant to someone on the TSDB. Using an estimated person network size of 472, Mr. Gairson concludes that "USCIS will be able to find an 'articulable link' to a purported national security concern for virtually any person on Earth" because CARRP uses the TSDB (Gairson paragraph 104). I analyzed Mr. Gairson's opinions from a statistical perspective.

B. Conclusions Concerning Mr. Gairson's Expert Report

1 Based on his 1.43 persons estimate of the distance between any person in the world and someone on the TSBD, Mr. Gairson concludes that "USCIS will be able to find an 'articulable link' to a purported national security concern for virtually any person on Earth," because CARRP uses the TSDB (Gairson paragraph 104). Mr. Gairson's opinion is statistically unsound because it relies upon two flawed assumptions which completely invalidate his estimate and argument.

2 Even if his assumptions were incorrectly considered not to be flawed, Mr. Gairson's conclusion or finding does not provide support that Plaintiffs or other

Plaintiffs' experts could use for a claim that the CARRP program is intended to deny immigration benefits to otherwise eligible applicants, and that pretextual links to the TSDB have provided the mechanism for denying benefits. Such claims are refuted by the empirical evidence.

C. Basis for Conclusions

The presumption that the TSDB can be easily used as a tool to find an "articulable link" to a purported national security concern for virtually anyone and therefore can be used as a pretext for discrimination is incorrect.

Mr. Gairson's opinions are statistically unsound because they rely upon two obviously flawed assumptions: (1) that personal network size does not vary among individuals; and (2) that all persons are equally likely to be connected to another person in the world.

A person's network size is the number of persons the individual knows. Based on some research in the article by McCormick, Salganik and Aheng which is not relevant to this matter, Mr. Gairson assumes that the median network size is 472 associations. He then assumes this median number is the network size for every person in the world. Based on this network size, Mr. Gairson further assumes each person in the world is equally likely to know someone else, and then uses the formula reported in the article he cites to compute the median degree of separation between any person in the world to a person in the TSDB, based on the population size of the TSDB and the world. This type of measure (degree of separation between people) has been popularized by the party game Six Degrees of Separation from Kevin Bacon.

Mr. Gairson's assertion that the estimated person network size and the size of the TSDB establishes with near mathematical certainty that any specific person can be linked to someone on the TSDB is wrong, because the assumptions upon which his assertion is based are clearly

flawed. The assumption that the median network size is the network size for every individual in the word is untenable. Network sizes vary widely by individual and someone with a large personal network will be more likely to have a smaller distance than someone with a small personal network. The median network sizes may not be a good estimate for any individual person, meaning different persons could have a markedly different network sizes and markedly different degrees of separation.

More significantly, Mr. Gairson's conclusion is based on a flawed assumption of "random mixing." The random mixing concept (as explained in the article he cites) unrealistically assumes that any individual is equally likely to know (or not know) anyone else in the population. The likelihood that any two members of the population know one another is presumed to be constant, without regard to other factors (referred to as barrier effects) such as their geographical distance or proximity, or their sharing a family, social, educational, professional or other relationship. A barrier effect occurs whenever some individuals systematically know more (or fewer) members of a specific sub-population than would be expected under random mixing. The assumption that everyone in the world is equally likely to know someone else, which is critical to the validity of Mr. Gairson's conclusion, has no relationship to reality. Random mixing is virtually always incorrect. It is almost a certainty that certain applicants are more (or less) likely than other applicants to know or be linked to someone on the TSDB. The failure to account for barrier effects makes Mr. Gairson's estimated distance between a person in the population and anyone on the TSDB false, useless and misleading. Mr. Gairson's assumption of a near mathematical certainty that one can link any applicant to someone on the TSDB is simply wrong.

Mr. Gairson's theory that USCIS could use the TSDB to find an "articulable link" to a national security concern and any adjustment-of-status or naturalization applicant is not only fundamentally flawed because it is premised on assumptions which do not reflect reality, the data USCIS produced refutes his apparent suggestion that USCIS as a common practice uses the TSDB in this fashion as a tool to arbitrarily, subjectively, and discriminatorily place applications into CARRP.[75] The USCIS data for FY 2013-2019 shows that only about one in every 375 applications for I-485 or N-400 benefits are referred to CARRP, making referral to CARRP rare. Thus, the data refutes any notion that USCIS has adopted a widespread practice of referring applicants to CARRP based on tenuous links between applicants and any person on the TSDB.

Even if his assumptions were correct, and they are not, as discussed in my July 17, 2010 report, the data does not support any presumption that USCIS routinely uses these types of TSDB associations as a pretext for referring applicants to CARRP given that (1) only 0.44% of I-485 and N-400 applicants were referred to CARRP during this same period, and (2) approximately 80% of all referrals of applications referred to CARRP which were adjudicated

[75] While I cannot statistically conclude that a pretextual articulation does not exist for some individuals regardless of their true person network size, the data does not support its existence as a common practice. To understand why this is the case for a statistician, consider what statistical evidence can and cannot conclude. The following illustration is offered to demonstrate what statistical evidence is and what it is not. Suppose I am playing a coin flipping game (*i.e.*, heads I win a dollar, tails you win a dollar) and you flip a coin 100 times and report getting 50 heads in 100 flips. There is no statistical evidence to support any inference that you were cheating, since the outcome of getting half heads is exactly what one would statistically expect from a fairly flipped coin. The pattern of heads and tails is not consistent with a pattern of cheating. Does that mean you did not cheat on isolated flips? I cannot rule out that you may have gotten 52 heads, but cheated and only reported getting 50 heads. Statistics can look at the pattern of events and determine whether or not there is a pattern of cheating but cannot rule out rare isolated incidents of cheating. Hence, while we can determine that there is no pattern of pretextually finding a link to discriminate against individuals who know persons in the TSDB, it cannot be statistically determined that the TSDB was never used in that fashion in a few isolated cases. The converse holds true for Mr. Gairson's conclusion, though. The data does not support a finding that the TSDB is ever used pretextually to refer a case to CARRP

were approved. The very low percentage of applicants referred to CARRP and the high percentage approved when adjudicated after CARRP referral contradict any notion that the CARRP program is intended to deny immigration benefits to otherwise eligible applicants, and that pretextual links to the TSDB have provided the mechanism for denying benefits.[76]

[76] Similar statistics exist which would contradict any notion offered by several of Plaintiffs' designated "experts" that the CARRP program is intended to deny immigration benefits to otherwise eligible applicants, and that pretextual links to the TSDB have provided the mechanism for denying benefits to otherwise eligible applicants.

VI. RESPONSE TO MS. ARASTU'S EXPERT REPORT

A. Overview of My Analysis of Ms. Arastu's Expert Report

I was asked to review the July 1, 2020 "Expert Declaration" of Nermeen Arastu, her amended report, for statistical accuracy. Specifically, I was asked to examine her conclusion stated in paragraph 67 of her amended report and to review the academic research presented in Exhibit C of that report, consisting of her law review article relating to her examination of 158 published federal court cases brought by applicants whose applications for citizenship were denied based on a lack of good moral character due to false testimony. I was asked to opine on whether this academic research represents a reliable statistical analysis of the type that professionals normally conduct. That is, can it be relied upon to draw valid inferences about USCIS practices? I was also asked to opine on whether the reported results actually support the conclusions expressed in her amended report even if one were to accept her analysis.

B. Conclusions Concerning Ms. Arastu's Expert Report

1 Ms. Arastu states in paragraph 67 of her report that "[t]he fact that all of the nations that top this list [the 12 countries whose applications were subjected to CARRP in the highest numbers in the USCIS data] are majority-Muslim or home to large Muslim populations suggest that in practice, [national security] concerns often serve as a thinly veiled metonym for Muslim and that CARRP joins the corpus of immigration law and law enforcement policy that by design or effect applies almost exclusively to Arabs, Muslims and South Asians." (Arastu July 1, 2020 amended report at paragraph 67). However, in my opinion, the list she cites simply does not lead to or justify her conclusion.

2 In her report, Ms. Arastu relies on her study of 158 federal court cases she selected that were filed by applicants for citizenship whose applications were denied for lack of the requisite good moral character at least in part because of false testimony to draw conclusions about adjudicator and systematic bias in the naturalization process. Her conclusions are based on an unsupported and illogical assumption that the 158 cases she examined are a representative sample of all cases and particularly all CARRP cases denied citizenship. Her analysis and conclusions based on her study of the 158 federal court cases are not based on any valid statistical analysis or evidence, but are simply her conjecture of how the data might support her inferences. Even if one could presume that the 158 cases could be projected to the population of all USCIS cases and also to USCIS CARRP cases—which they cannot—they still would not statistically support Ms. Arastu's argument that CARRP was designed to allow Muslims to be disproportionately denied for false testimony. Furthermore, on its face, the data she presents is inconsistent with her argument.

C. Basis for Conclusions

Ms. Arastu presents her hypothesis of adjudicator and systematic bias, especially in CARRP, and then looks for data which would anecdotally support her hypothesis if one is already predisposed to believe her hypothesis. While this technique is useful for illustrating an argument, it does not provide statistical evidence to allow an inference that her hypothesis is true. In order to provide valid statistical evidence to support an hypothesis, one must state their hypothesis in the negative (*i.e.*, assume their hypothesis is not true) and its implications before collecting and/or analyzing the data. Then, one must collect data that is representative of the population of interest and statistically test the extent to which the data would be unlikely to exist if the hypothesis was not correct. For example, you believe a coin is biased in favor of heads. You set up the counter hypothesis that the coin is fair and then collect data on 20 flips of the coin and record the number of heads. Assume that the result of your test is 17 heads. Since the probability of getting at least 17 heads with a fair coin is only .0013, you can infer from the data that the coin is biased, and the likelihood you are wrong (*i.e.*, that the coin is not biased) is very small. Ms. Arustu does not conduct or refer to such a statistical analysis.

Ms. Arastu's amended report at paragraph 67 lists what she states are the twelve countries with the highest number of N-400 applications referred to CARRP, classified by the applicants' country of citizenship. She leverages this listing to suggest that the vast majority of referrals to CARRP are Arabs, Muslims and South Asians, since the population of nine of the countries is majority Muslim, another of the 12 countries (India) has the world's second largest Muslim population, and the two remaining countries (China and Russia) have sizeable Muslim minorities. She proposes that this demonstrates that "NS concern often serves as a thinly veiled metonym for Muslim." This is a vast overreach. Although applications from some countries

with majority Muslim populations are more likely to be referred to CARRP, her table does not support the argument that the vast majority of referrals to CARRP are Arabs, Muslims and South Asians.

It is notable that Ms. Arastu stopped at just the top 12 countries without any justification for looking only at those 12. If she had looked at the top 15 countries, her statement that all the top countries are countries where the population is majority Muslim or have a sizeable Muslim population would no longer hold. Table R3 below lists the top 15 countries and presents the number and percent of referrals to CARRP along with the number of denials of applications broken out by whether they were processed in CARRP.

TABLE R4

DATA ON TOP 12 AND 15 COUNTRIES WITH THE HIGHEST REFERRALS TO CARRP BASED ON CITIZENSHIP OF APPLICANT

Country of Citizenship	Referred to CARRP		Denied Via		Percent CARRP of Denials
	Number	Percent	CARRP	Routine	
[redacted]	2,711	2.61%	358	8,080	4.2%
[redacted]	1,785	1.88%	239	6,460	3.6%
[redacted]	1,220	2.81%	263	4,421	5.6%
[redacted]	1,167	0.41%	83	23,231	0.4%
[redacted]	1,159	1.53%	147	6,162	2.3%
[redacted]	753	4.05%	100	1,442	6.5%
[redacted]	745	2.51%	164	3,705	4.2%
[redacted]	712	0.19%	72	14,999	0.5%
[redacted]	675	3.39%	143	1,985	6.7%
[redacted]	560	3.54%	145	2,629	5.2%
[redacted]	535	0.81%	40	2,907	1.4%
[redacted]	459	0.88%	87	4,249	2.0%
TOP 12	12,481	1.05%	1,841	80,270	2.2%
[redacted]	362	1.05%	70	1,998	3.4%
[redacted]	259	1.05%	72	20,648	0.3%
[redacted]	323	1.05%	17	2,466	0.7%
TOP 15	13,425	1.05%	2,000	105,382	1.9%

Further, Ms. Arastu's conclusion that her top 12 list consists of countries with a majority Muslim population or countries with a large Muslim (but not majority Muslim) population is an *ad hoc* justification for her opinion that Muslims are victims of discrimination by USCIS and is unfounded. First, denial of an immigrant benefit does not mean one was discriminated against. Second, Indonesia is the country with the largest Muslim population in the world, and its population is majority Muslim, but it is not in Ms. Arastu's top 12 or the top 15 countries CARRP referral countries; Nigeria and Bangladesh are also among the four largest majority Muslim countries, but they too are not among the 15 countries with the most referrals to CARRP.

Russia and China have large Muslim populations, but have much larger Christian populations. In fact, Russia has the fourth largest, and China has the eighth largest, Christian population in the world. Notably though, Ms. Arastu does not suggest from this data that Christians were discriminated against in CARRP referrals. Expanding the list to 15 would include one country (Syria) which is majority Muslim, and Cuba and Canada, neither of which are majority Muslim nor have a sizeable Muslim population. The list simply does not prove what Ms. Arastu contends. Moreover, looking at all the data, only 58% of all referrals to CARRP were citizen of countries whose population is majority Muslim. While a referral is disproportionately more likely to be for a citizen from a majority Muslim country, referrals are far from exclusively made for applicants from countries with a majority Muslim population.

More critically, Ms. Arastu ignores the low rate of referrals to CARRP for applicants from these 12 countries. Clearly, being a citizen of one of Ms. Arastu's selected 12 countries is not a major factor in being sent to CARRP; referral to CARRP is a rare event for any application, regardless of the country from which the applicant originated. This means that some factor(s) other than country of birth or citizenship is the dominant factor determining whether one's application is referred to CARRP.

Moreover, Ms. Arastu seems to assume or intimates that those referred from these 12 countries were all Muslim. For example, she assumes that the [redacted] CARRP referrals from Russia during the 7-year period examined (FY 2013-2019) were all Muslims (or predominantly Muslim). However, the data she analyzed does not include information on an applicant's religion, so there is no basis for determining what proportion of applicants who were citizens of each of Ms. Arastu's selected twelve countries, including the three countries without a Muslim majority (India, Russia and China), are Muslim. Although one can argue that without such

information, one can assume that applicants who are citizens of countries whose population is majority Muslim are more likely than not to be Muslim, and conversely applicants who are citizens of countries whose population is not majority Muslim are more likely to not be Muslim, Ms. Arastu is not consistent on this point because she assumes that all from a majority non-Muslim country are Muslim, so long as it has a sizeable Muslim population. That is, if you apply from a majority Muslim country with a sizeable non-Muslim population, she assumes you are Muslim; but if you apply from a non-majority Muslim country with a sizeable Muslim population, you are still Muslim. As a result, Ms. Arastu tries to have it both ways. This type of classification system cherry picks the countries to create a desired result, and such a system is not a valid statistical methodology. Ms. Arastu provides no justification or basis for her speculations that referral to CARRP of applicants who are citizens of India, Russia and China (whose populations are respectively 13.4%, 14.7% and 1.6% Muslim) is due to anti-Muslim bias, other than her unsupportable belief that CARRP targets Muslims. Accordingly, Ms. Arastu fails to use a reliable methodology to reach these conclusions.

Additionally, the data shows that there is no difference in the likelihood of denial among those in CARRP from majority Muslim countries or non-majority Muslim countries, and only approximately 20% of CARRP cases overall are denied. Thus, if we believe that applicants in CARRP were denied because they were citizens of a majority Muslim country, we would expect a large percent of all denials to be citizens of countries with a majority Muslim population. Yet, we see no difference in the denial rates within CARRP between those who are citizens of a country with a majority Muslim population and those who are not.

As a result, the data refutes Ms. Arastu's contention that CARRP serves as a "metonym for Muslim" and the role of CARRP is to deny naturalization to Muslims.

I now turn to Ms. Arastu's article ("Aspiring Americans," attached as Exhibit C to her amended report) which she says gives statistical support for her opinion that CARRP uses the "false testimony" criterion to disproportionately reject CARRP processed applicants who are Muslim. She cites the article's discussion of the good moral character requirement of the Immigration and Nationality Act (INA) and its purported potential to enable individual and institutional bias through a subsection that requires USCIS to deny naturalization petitions when USCIS finds that the applicant offered "false testimony." Ms. Arastu's presumption that USCIS and its personnel apply the good moral character requirement in a manner to effect individual and institutional bias against Muslims, particularly those in CARRP, with the resulting denial of naturalization petitions based upon a biased finding of "false testimony," is total conjecture for which she offers no valid statistical or other support.

The contention that the use of the good moral character requirement and the belief that a finding of false testimony can be abused to engender denials based upon individual or institutional bias against Muslims is not evidence that it has actually occurred, and that is occurred more frequently in CARRP decisions. Much of what Ms. Arastu claims is simply supposition or theory.

Ms. Arastu states that her conclusions are based in part on her examination of 158 published court cases where an applicant's use of false testimony was a basis for denial. Based on her analysis of this data, she surmises about the overall practices of USCIS, especially with regard to CARRP. Her reliance on her review of the 158 cases as evidence to prove her suppositions is statistically incorrect. First, her claim that the sample is a representative sample of the population of applicants for citizenship who were denied is obviously incorrect. Hence her review of the 158 cases cannot be the basis for any valid statistical assessment of the overall

practices of USCIS, and especially for those in CARRP, since she does not know which, if any, of the 158 cases where adjudicated in CARRP.

Ms. Arastu states that her "data set offers a comprehensive and representative sample of published federal cases." This statement is irrelevant to the issue of the statistical representation of the sample. The question is not whether it is representative of the few published federal cases, but rather whether it is representative of the population of denials made by USCIS and specifically those in CARRP. To conduct a valid statistical study that is based on a sample and allows the results to be projected to a population, the sample must be a scientifically drawn probability sample (often referred to as a random sample) or a judgment sample that can be demonstrated to be representative of the population of interest. In the latter case, it is the responsibility of the user of the sample to justify why they believe the judgment sample is representative. This, Ms. Arastu fails to do.

Clearly, neither is the case here. The sample is not a random sample. There is no reason to believe that those few individuals whose applications were denied for false testimony are representative of all denials. The factors which influence a person to seek redress in court are many, and obviously are not common, since few denials result in published federal court cases. Either denials for false testimony are extremely rare (in which case the issue of incorrect denials for false testimony is not significant) or the number of such cases which result in published federal court cases is very small (so the 158 are not representative of the population of denials for false testimony).

To assume the factors affecting whether a case involving false testimony are random among the population of applicants denied for false testimony is highly speculative and unlikely to be correct. Without clear and persuasive evidence that the sample is either a scientifically

drawn random sample or selected based on the supportable judgment that those who sue in federal court are representative of all the relevant denied applications, the use of the sample to project to the population of denials especially in CARRP would not be reliable. In my opinion, no competent statistician would rely on the sample of 158 published federal cases to project the characteristics of all cases denied for false testimony. Ms. Arastu did not draw a random sample, nor did she choose a judgment sample, nor did she present any evidence to support that her sample would be representative of the population. Instead, she took a "convenience sample," because it was the information that was available. However, such data cannot be used to reliably make any statements about the population of cases denied for false testimony. Such data at best simply provides anecdotal cases to illustrate a narrative being offered. Yet, it cannot be used as statistical support for any narrative being offered.

Ms. Arastu states as a fact that among some 158 cases where false testimony was alleged (although not necessarily proven), 130 cases occurred after 9/11 (September 11, 2001), the vast majority of recent cases were presumably Muslim because they were from Muslim countries or had a Muslim name, and that in some cases (32 or 24.4% out of the 131 cases decided) the denials due to false testimony were found by the courts to be questionable and inappropriate.

Even if one could presume that all of the foregoing statements by Ms. Arastu were true, the data she cites (approximately 40% of which is pre-CARRP) hardly proves that CARRP denials are significantly based on USCIS inappropriately using the "false testimony" bar. First, it cannot be presumed that any of the cases were even referred to CARRP, as there is no evidence that any of these 93 possible CARRP cases (or the lower number of possible CARRP cases for which a court overturned a denial) involved applications processed through CARRP. During FY 2013 through FY 2019, among N-400 applications only 2.8% of applicants who were born in a

Muslim country and were denied were processed at least in part through CARRP. The other 97.2% of denied N-400 applicants from majority Muslim countries were processed via the normal (non-CARRP) process. Thus, there is no factual basis to support Ms. Arastu's suggestion that the published cases she points to as CARRP cases actually involve CARRP.

Ms. Arastu surmises that the increase in published cases where an applicant's false testimony was used as a basis for denying naturalization after 9/11, especially after the introduction of CARRP in 2008, is due to the introduction of CARRP. If as she alleges CARRP was formed to be a "metonym for Muslim," the data does not support that the increase is due to the introduction of CARRP. If we look at the data for the 158 cases, we find that while the number of applicants increases from nine pre-9/11 to 37 in the period from 9/11 to the start of the CARRP program, to 93 after the start of the CARRP program; but the percent of cases that she identified as Muslim remained essentially the same: 44.4%, 45.9%, and 46.2%, respectively. For the sake of argument, if we incorrectly accepted the 158 cases as an appropriate sample from which to draw inferences about the population, the correct inference would be that 9/11 and the introduction of CARRP had no impact on the likelihood that a contested denial for false testimony would be from an applicant of a Muslim country. Again, therefore, the data is completely inconsistent with Ms. Arastu's narrative about the impact of CARRP.

Moreover, the statistical data that shows referrals to CARRP are rare also shows that the denial rate in CARRP did not differ by what country the applicant came from, and that 97.2% of the denials of applications for applicants from majority Muslim countries occurred during routine processing and adjudication (outside of CARRP). Thus, one can only reasonably conclude that almost all the 158 cases on which Mr. Arastu focuses her opinion concerning denials based on

the applicants' "false testimony" were non-CARRP cases, denied through the normal (non-CARRP) processing of applications.

Finally, if one were to accept Ms. Arastu's hypothesis that the denial of N-400 applications processed in CARRP made false testimony easier and were significantly based on anti-Muslim bias and the pretextual finding of "false testimony" as the mechanism for that bias, we would expect the percent of cases from applications from Muslim countries would increase with the introduction of CARRP—it did not—and we would expect that applicants from majority Muslim countries would be more likely than applicants from non-Muslim countries to suffer a rejection of their application once submitted to CARRP, but, that is not the case. Specifically, there is not a higher rate of denials of N-400 applications from majority Muslim applicants whose applications are submitted to CARRP compared to applicants from non-Muslim countries whose applications are submitted to CARRP.

Bernard R. Siskin
Dated 10/13/20