**IN THE UNITED STATES DISTRICT COURT
FOR THE SOUTHERN DISTRICT OF NEW YORK**

CENTRAL RABBINICAL CONGRESS OF THE USA & CANADA, *et al.*,

Plaintiffs,

vs.

NEW YORK CITY DEPARTMENT OF HEALTH & MENTAL HYGIENE, *et al.*,

Defendants.

Case No. 12-Civ.-7590

Judge Naomi Reice Buchwald

SUPPLEMENTAL AFFIDAVIT OF DR. AWI FEDERGRUEN, D.SC.

1. In my initial affidavit in support of the plaintiffs' motion for a preliminary injunction in this case, dated October 10, 2012 ("Federgruen Aff."), I identified and addressed the statistical deficiencies in the study published by the CDC's non-peer reviewed *Morbidity and Mortality Weekly Report* ("MMWR"), which was co-authored by several staff members of the New York City Department of Health and Mental Hygiene ("the Department"), including the current Health Commissioner, Dr. Farley. (*See* Farley Decl., Exh. K.)

2. That study had concluded that the observed incidence rate of HSV-1 infections among the MBP population is higher than that in the general population of male infants, in a way which is *statistically significant*; it formed the principal (if not exclusive) justification for the newly adopted Health Code § 181.21. In my affidavit, I explained why that conclusion was not supported by the data. In fact, after correcting for any one of several of the MMWR study's erroneous and unsupported assumptions, the data show that the incidence rate of HSV-1 in the MBP population is not higher than the baseline incidence rate by any statistically significant margin.

3. I have now carefully reviewed the declaration submitted by Dr. Andrew Gelman in support of the defendants ("Gelman Decl."), which defends the MMWR study and attempts to respond to my criticisms thereof. I have also reviewed the exhibits that were submitted by the defendants, and in particular the minutes of the Board of Health's hearing on § 181.21, from June 12, 2012. (*See* Goldberg-Cahn Decl., Exh. A.)

4. In light of these new materials, I have revisited my initial affidavit. That affidavit's overall conclusion—that the incidence rate in the MBP population fails to be significantly larger than the baseline rate—must, in fact, be amplified. Amplification of this conclusion is based on two corrections, which were discussed but not implemented in my initial affidavit. *First*, the new data submitted by the defendants now allows for a

plausible accounting of the incidence of MBP outside the Hasidic/Yeshiva communities (which the MMWR study assumed to be zero), and thus for a more complete correction of the study's critical assumption about the size of the MBP population segment. *Second*, employing an alternative—and, in this setting, a more accurate—way to construct the confidence interval for the relative risk ratio further weakens the Department's claim that a statistically significant link exists. In addition, while not as readily quantifiable, the accuracy of several *other* parameters of the MMWR study's analysis is at the very least in question, as discussed below.

5. Combining both corrections shows that, at the 95% confidence level that the MMWR study adopted, the actual risk of HSV-1 infection in the MBP population, while still potentially larger, may actually be *lower* than the risk among the general population. In other words, the reported or assumed data, *even if entirely accurate*, are consistent—again, at the 95% confidence level—with the possibility that the actual risk in the MBP population is 42% lower than in the general population, as opposed to the study's conclusion that it is at least 30% higher. These calculations are explained below, and their impacts are exhibited in Table 1:

STARTING VALUE 95% CONFIDENCE INTERVAL	MMWR Study's Confidence Interval	Alternative Confidence Interval
No parameter corrections	1.3	1.02
Correcting MBP pop. within Hasidic / Yeshiva communities	0.9	0.71
Fully correcting MBP pop. assumption	0.72	0.58

TABLE 1[1]

<u>The Study's Assumption About the Size of the MBP Population</u>

6. My initial affidavit corrected the MMWR study's assumption about the number of MBP circumcisions in New York City during the study period by employing the precise enrollment data at Hasidic and Yeshiva kindergartens in New York City (rather than the K-12 data used by the MMWR study). (Federgruen Aff., ¶¶ 8-9.) As Dr. Gelman admits, that adjustment alone eliminated the statistically significant association between HSV-1 and MBP. (Gelman Decl., ¶ 19.) In particular, as shown in Table 1, making that correction alone results in the conclusion that the HSV incidence rate among the MBP population could be as low as 10% *lower* than among the general population (*i.e.*, a 0.9 relative risk ratio).

[1] The complete 95% confidence intervals are reported in Appendix 1.

7. Dr. Gelman notes that I lack expertise in identifying which schools fall into the Hasidic, Yeshiva, or Other categories, or in estimating the percentages of the students in these groups who were likely to have had MBP. (*Id.* ¶ 18.) But I relied, in categorizing the schools, on designations provided by experts on Jewish schools in New York City. More specifically, the categorization of the schools was made by Deborah Zachai, the Director of Education Affairs of the Agudath Israel of America and her staff. Ms. Zachai employed the official 2011 New York State Department of Education enrollment spreadsheet for all Jewish schools/kindergartens in New York City, and indicated, on this spreadsheet, which of them fall in each of the above three categories. A printout of that spreadsheet was attached to my affidavit. (Federgruen Aff., Exh. 1.)

8. Ms. Zachai now confirms this process and (with a few minor changes) affirms the characterizations of the schools. (*See* Decl. of Deborah Zachai, ¶¶ 2-4.)

9. The school designations, along with the MMWR study's assumed MBP practice rates, allowed me to estimate what percentage of the *2011* male kindergarten population had been exposed to MBP. I anchored my population estimates to the 2011 enrollment data because the boys attending kindergarten in that year may be assumed, almost exclusively, to have been born in 2006, the first of the 5.75 years in the MMWR study period. As described in my original affidavit, the numbers revealed that 69% of the boys belonged to the Hasidic community and 23% to the Yeshiva community. Applying these percentages to each of the 5.75 years in the study period actually underestimates the size of the MBP population in the 2007-2011 years since the demographic trends imply *growing* shares for the Hasidic community, in which, under the MMWR study's own assumptions, MBP is practiced without exception.

10. Independent verification of the validity of the population shares in the above three communities is now provided by Dr. Marvin Schick, the author of the K-12 census report on which the MMWR study relied for its analysis. (*See* Aff. of Dr. Marvin Schick, ¶ 11.) Dr. Schick has based his estimated kindergarten population shares on enrollment data pertaining to the 2008 year, the year to which his census report related. His estimate for the Yeshiva community is virtually identical to the one that I obtained (22.5% vs. 23%); that pertaining to the Hasidic community is somewhat lower than mine ("in excess of 60%" vs. 69%) but that difference is fully consistent with the continued rapid growth of the relative population size of this community, mentioned above and confirmed by Dr. Schick (*see id.*, ¶ 8).

11. As to the percentages of the students in the three school types who are likely to have been exposed to MBP, I simply adopted the assumptions of the MMWR study—that 100% of the Hasidic community, 50% of the Yeshiva community, and 0% of the Other community undergo MBP. Notably, the 50% figure is most likely too *low*.[2]

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[2] The 50% assumption appears to have originated from an informal exchange with Rabbi Zwiebel of Agudath Israel of America. (*See* Farley Decl., Exh. L, at 1-2.) That figure was offered as an "unscientific" and "extremely conservative estimate" (*id.*), and most rabbinic authorities indicate that the true figure is much higher. However, since the precise figure can not be ascertained, I have, in my analyses continued to

12. Moreover, in my initial affidavit, I stated that MBP is practiced by many outside the Hasidic and Yeshiva communities. (Federgruen Aff., ¶11.) For example, it is practiced almost universally within the Sephardic Jewish community, which has orthodox and non-orthodox members, and is, in any case, not part of the established Yeshiva or Hasidic communities. Attached as Exhibit 1 is a letter signed by 14 prominent Sephardic Rabbis in New York City, attesting that MBP is practiced by approximately 90% of their communities. As another example, many families outside the Orthodox community use Chabad *mohelim*, who practice MBP almost without exception. Dr. Schick points at various reasons why reliance on NYC kindergarten enrollments results in too low an estimate of the MBP population (Schick Aff.,12.)

13. The MMWR study itself admitted that the MBP population might be underestimated by virtue of the assumption that MBP occurs only within the Hasidic and Yeshiva communities: "The findings in this report are subject to at least one limitation. ... [T]he relative risk depends, in part, on assumptions used to estimate the number of male infants who undergo circumcision with direct orogenital suction, and those assumptions might not be valid. For example, because not all of the cases were in ultra-Orthodox Jewish families, estimates of the exposed population might be underestimated." (Farley Decl., Exh. K, at 5.)

14. What the study did not mention—but what is now revealed by the minutes of the Board of Health hearing attached to the Goldberg-Cahn Declaration—is that, of the 11 cases of HSV following MBP between November 2000 and December 2011, *at least two* arose outside the Hasidic/Yeshiva communities.[3] (*See* Goldberg-Cahn Decl., Exh. A at 8, 13.) Beyond the considerations mentioned above, these numberssuggest that, outside the Hasidic and Yeshiva communities, the remainder of the MBP population is sizable, albeit hard to quantify exactly. If the 2:9 ratio among the 11 reported cases reflected the ratio of the actual populations exposed to MBP, then the size of the total MBP population would need to be further increased by approximately 22%=2/9. A plausible approach to correct for the MMWR study's complete omission of any MBP cases outside the Hasidic/Yeshiva communities, is therefore to increase the size of the total MBP population by 22%(or2/9).

15. This adjustment, on top of the initial adjustment based on the enrollment data, results in the conclusion shown in Table 1: that, at a 95% confidence level, one can conclude only that the true rate of HSV-1 among the MBP population could be as low as 28% *lower* than the baseline rate (*i.e.*, a relative risk ratio of 0.72), rather than at least 30% *higher*, as the MMWR study had concluded.

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adhere to the 50% assumption, recognizing that this results in an underestimate of the size of the MBP population, and, hence, an inflated magnitude of the risk ratio..

[3] It is unclear why the breakdown is cast in ambiguous terms such as "at least two", as opposed to providing an exact breakdown.

Validity of the Formula Employed To Construct the Confidence Intervals

16. Setting aside the erroneous estimates of the MMWR study's parameters, there are important problems associated with the formulae it employed to construct the confidence interval for the risk ratio. Constructing a confidence interval for the risk ratio is, indeed, key to determining whether the observed difference between the incidence rate in the MBP population and the general population is statistically significant or not. So, while the MMWR study, based on its assumed parameters, observed an incidence rate in the MBP population that is 3.4 times that in the general population, the question is whether this difference may be due to the randomness intrinsic to observing samples, or not. To this end, a statistical study typically constructs an *interval* that contains the *true* ratio of the infection likelihoods or "risks" in the two populations, with a pre-specified certainty of, say, 95%. This interval is referred to as the confidence interval associated with the pre-specified level of confidence. When the 95% confidence interval of the risk ratio contains the value 1, this means that the reported data are consistent with the possibility that the risk in the exposed population—in our case, the MBP population—is *equal to*, or even *lower than*, the rate in the general population; i.e., the observed difference in the rates in the two populations fails to be significant, at the 95% confidence level.

17. Thus, the MMWR study's claim of a statistically significant difference with respect to the risk within the MBP population, is entirely based on its computed 95% confidence interval starting at the value 1.3, *i.e.*, failing to contain the value 1. However, as explained below, in this setting the *exact* confidence interval cannot be computed, requiring use of an *approximate method*. There are several alternatives for such approximations, but, depending on the setting, some are more accurate than others. It is for this reason that, in Table 1, I have computed the starting values of the 95% confidence interval when computed with a different but classical method, which, as explained below, in this setting is considerably more accurate than the one employed by the MMWR study. I have done this both without any corrections to the assumed parameters, and with such corrections for the size of the MBP population. (The complete intervals are reported in Appendix 1.)

18. Since the exact interval cannot be computed with analytical formulae, constructing the appropriate confidence interval for a risk ratio is a task that requires care and judgment; different approaches are required under different circumstances. These are discussed in most classical textbooks in the area, such as Lehmann (1986), the classical work on statistical hypothesis testing; Fleiss et al.(2003), a standard reference work in biostatistics; and Rothman et al. (2008), generally considered the premier handbook in epidemiology.[4] The formula employed by the MMWR study—without any reference or discussion—is frequently used in public health studies. However, it is based on two important approximations:

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[4] Lehmann, E. (1986), *Testing statistical hypotheses,* John Wiley, New York, NY; Fleiss, J., B. Levin & M. Paik (2003, 3rd ed.), *Statistical methods for rates and proportions,* John Wiley, Hoboken, NJ; Rothman, K., S. Greenland & T. Lash (2008, 3rd ed.), *Modern Epidemiology*, Lippincott Raven.

(a) The number of diagnosed cases in each of the two populations (in this case, the MBP population versus the general population) follows a so-called Binomial (n, p) distribution. Here n denotes the number of potential cases and p the incidence rate. (Let Y denote the number of diagnosed cases in the MBP population; the study's estimates for the parameters of this Binomial distribution are n=20,493 and p=5/20,493). Since no method has been identified to construct the risk ratio confidence interval from this *exact* distribution, an *approximate* distribution needs to be used. The MMWR study's formula approximates the Binomial distribution with a Normal distribution, as opposed to a Poisson distribution. But the authoritative handbook on univariate distributions and their approximations concludes that the Poisson distribution is to be preferred, because of considerably greater accuracy, if (n^0.31)*p<0.47.[5] [6] In the MBP population, this test quantity amounts to only 0.0053, which, therefore, overwhelmingly favors the Poisson approximation. If the size of the MBP population is corrected, as above, the test quantity further reduces to 0.004 or below; the same observation applies to X, the number of diagnosed cases in the general, non-MBP population, which has a Binomial distribution with n=352,411 and p=25/352,411.

(b) The second approximation underlying the formula employed by the MMWR study is to replace the *logarithm* of X (shorthand notation: logX) and the logarithm of Y (logY) by a *linear* approximation around the mean: once again, this is an acceptably accurate approximation only if X and Y have relatively low volatility so that they are likely to stay close to their respective means. However, in this case, Y in particular, has considerable volatility, as measured for example by the coefficient of variation=standard deviation/mean=0.45.

19. Appendix 2 describes a classical, alternative method to derive the desired confidence interval. It can be found in each of the three classical textbooks mentioned above: § 4.5 in Lehmann; pp.344-345 in Fleiss et al.; and pp.254-255 in Rothman et al. This method approximates the Binomially distributed random variables X and Y by Poisson distributions, a far more appropriate approximation. Moreover, *no other* approximations are required. The resulting 95% confidence interval starts at the value of 1.02, instead of 1.3 when the MMWR study's assumed parameter values are employed. This value reduces, however, to 0.71 if only the size of the Hasidic/Yeshiva MBP population is corrected and 0.58, if a plausible, though conservative, accounting is made for the remainder of the MBP population, as set forth above.

20. In short, this means that a more accurate method of computing confidence intervals to assess the association between MBP and HSV shows that the incidence of HSV in the MBP population could be as low as 42% *below* the baseline rate, using the corrected assumptions about the MBP population size.

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[5] Johnson, N., A. Kemp & S. Kotz (2005, 3rd ed.) , *Univariate discrete distributions,* John Wiley, Hoboken, NJ.

[6] The notation a^b is used to denote the b-th power of the number a; for example 3^2 =9 (the square of 3); and 2^3 =8 (the number 2, raised to the power 3).

Other Apparent Errors in the MMWR Study's Analysis

21. The MMWR study used, as one of its parameters, that five cases of HSV following MBP had occurred during the 5.75-year study period. However, Defendants' brief in opposition to the preliminary injunction states that, of the 11 cases identified in the 2000-2011 period, "there was confirmation that the ritual circumcision included [MBP]" only "in six of those cases." (Dkt. No. 34, at 9.) That is, close to half of the claimed cases could *not* be confirmed as involving MBP at all. Of the five cases analyzed by the MMWR study, *three* were not confirmed to have involved MBP. (*See* Farley Decl., Exh. K, at 4 tbl. 1.)

22. Further, even the cases that the MMWR study treated as "confirmed" are dubious. For example, in the first of the six "confirmed" reported cases, the *mohel* denied performing MBP—a denial that withstood a polygraph test. (Aff. of Robert Simins ("Simins Aff."), ¶ 20; Aff. of Dr. Daniel S. Berman ("Berman Aff."), ¶ 12.)

23. In other words, by the study's own criteria of "confirmed" cases, only 2 of the 5 cases identified during the study period are "confirmed," and the criterion for confirmation is apparently subject to error, rendering it possible that the actual number of MBP cases associated with an HSV-1 infection may even be lower than 2.

24. Notably, if *even one* of the five reported cases did not actually involve MBP, the risk ratio would show no statistically significant association between MBP and HSV, even absent any of the other required corrections described in my affidavits.

25. The MMWR study also used, as a parameter, 25 cases within the study period of HSV-1 among male infants *not* exposed to MBP. However, as explained by Dr. Daniel S. Berman, herpes infections do not present with a clear, much less unique, set of symptoms. (*See* Supp. Aff. of Dr. Daniel S. Berman, ¶ 44.) Given that the condition is extremely rare, far from all clinicians would consider testing for it when presented with a baby from the general population. In contrast, the association between HSV and the Orthodox community has become immediate and ubiquitous, creating a classical instance of a screening bias.

26. Notably, if the number of cases in the general population, not exposed to MBP, were significantly larger than 25, this adjustment would, *by itself*, lead to rejection of the hypothesis of a significantly higher risk in the MBP population.

27. An additional problem with the methodology in the CDC study is the fact that the duration of the surveillance study period was not specified at the outset. A properly designed study specifies this duration either in terms of a specific number of calendar years or in terms of a desired number of observed cases. It appears likely the study was terminated at the first instance where statistical significance of an increased risk among the MBP population could be claimed. That is improper, by itself calling the finding of statistical significance into question. (*See* Aff. of Dr. Brenda Breuer, ¶¶ 6-8.)

Responses to Dr. Gelman's Declaration

28. One of Dr. Gelman's principal responses to my affidavit is to claim that, even if the MBP-HSV association is not statistically significant based on the data from the MMWR study period of 5.75 years, the association regains statistical significance if the 6 cases that predate the study period are taken into account. (Gelman Decl., ¶ 10.) The problem, however, is that no data are available for the total number of infections among the general population before April 1, 2006, when HSV became a reportable condition in New York City. That is why the MMWR study begins its analysis on April 1, 2006. Without precise data on the number of cases in the preceding years, comparison of the relative risks among the MBP and general populations becomes purely speculative. Dr. Gelman attempts to resolve that issue by assuming that the annual incidence rate in the 6.25 preceding years was identical to that observed in the study period. (Gelman Decl., ¶ 22 n.1.) That is plainly improper, however, because it artificially inflates the reliability of actually observed differences over a smaller study period.

29. Dr. Gelman says that, even taking into account the adjustment made by my initial affidavit to the assumption of the MBP population size, the relative risk ratio remains "nearly statistically significant." (Gelman Decl., ¶ 24.) Statistical theory may be used to identify, with a given confidence level, whether an observed higher incidence rate of HSV-1 infection in population A, may arise due to mere randomness, even though the *actual* likelihood of an infection in population A is, in fact, *equal to or lower than* that in population B. (See the discussion above.) The concept of being "nearly statistically significant" is ill-defined and, therefore, is not used in statistical studies to arrive at public policy conclusions. Instead, scientists report the end points of the computed confidence interval. As discussed above, merely correcting the assumed size of the Hasidic/Yeshiva component of the MBP population results in the 95% confidence interval of the risk ratio starting at the value 0.9 (meaning that the incidence rate among the MBP population could be as low as 10% below the baseline). Adding a conservative estimate for the remainder of the MBP population, reduces this value further to 0.72 (thus suggesting that the incidence among the MBP population could be 28% below baseline).

30. Relatedly, Dr. Gelman contends that, regardless of whether a link between MBP and HSV is statistically significant, "the raw data are compelling." (Gelman Decl., ¶ 8.) The raw data reveal that 5 out of 30 HSV cases (*i.e.*, 1/6th) are associated with confirmed or so-called "probable" MBP. But both the numerator and denominator of that figure are disputed, as set forth above. And, even when these challenges are disregarded, how could a 1/6 ratio be "compelling" in a vacuum, without relating it to the sizes of the MBP and the total population, and without constructing rigorous confidence intervals for the true risk ratio? Statistical analysis exists to determine whether raw data are, in fact, "compelling," and that analysis here suggests that the data are not.

31. Dr. Gelman also argues that the link between MBP and HSV is bolstered by the existence of "clusters of cases connected with two people who perform ritual circumcisions." (Gelman Decl., ¶ 5.) In particular, the MMWR study associates Mohel A with three cases of HSV following MBP during a two-year period; and Mohel X with

two cases over three years. The suggestion is that these clusters are unlikely to have occurred by chance, indicating a link between these *mohelim* and the infections.

32. The first problem with this argument is that, of the three cases that are attributed to Mohel A, two are twins who underwent circumcisions minutes apart. As Dr. Berman has explained, the two ensuing infections are clearly not independent events, in the sense that infection of one twin will likely be accompanied by infection of the other one, by transmission from one infected baby to his brother, or through a caregiver or mother who changes the diaper and circumcision wound dressings. (Berman Aff., ¶ 13.) Consequently, for the purpose of statistical analyses, the claim that 3 cases were associated with Mohel A in a two-year period must be replaced by the claim of two infections in a two-year period, during the years 2000-2005.

33. Furthermore, it is doubtful whether the first of the cases associated with Mohel A, while treated as "confirmed" MBP by the Department, qualifies as an MBP case. The *mohel* has denied performing MBP; his denial withstood a polygraph test; and there are no eyewitnesses. (*See* Simins Aff., ¶ 20.) In other words, it is very possible that *no* cluster of two infections in a two-year period and associated with the same *mohel* occurred at all.

34. However, even if a cluster of the above type occurred, the odds of such a clustering event is not particularly small. Rather, it is on the order of 10% or higher. This is because one cannot consider Mohel A in isolation, but rather must take into account the 20 to 30 other *mohelim* practicing MBP in New York City during this time frame. To use an example, a front-page article in the *New York Times* (February 14, 1986) reported on a woman winning the New Jersey lottery twice, and describing it asa "1 in 17 trillion long shot." Yet it is almost a certainty that, over a 20-year period, *at least one* person in the United States wins the jackpot twice; the odds are better than even to have a double winner in seven years somewhere in the United States and better than 1 in 30 to have a double winner in a four month period.[7] The calculations in Appendix 3 show that the likelihood of *some* mohel experiencing two cases of HSV in the course of a two-year period within a 5-year window is 9% to 12%. In other words, this event has a very considerable likelihood of occurring.

35. As for Mohel X, to whom two "probable" cases of HSV following MBP are attributed over a *three-year* interval, the odds are even higher (because the cases occurred over three years rather than two).

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[7] *See, e.g.*, Diaconis, P.& F. Mosteller (1989), *Methods of studying coincidences*, JOURNAL OF THE AMERICAN STATISTICAL ASSOCIATION, vol.84, pp.853-861.

I declare under penalty of perjury under the laws of the State of New York that the foregoing is true and correct to the best of my knowledge.

Executed this 30th day of November, 2012, at New York, New York.

[signature]
Awi Federgruen

STATE OF NEW YORK
COUNTY OF New York

Subscribed and sworn before me this 30 day of November, 2012.

[signature] Florence J Grant
Notary Public
My commission expires on: 3/30/14.

FLORENCE J GRANT
Notary Public - State of New York
NO. 01GR4666482
Qualified in New York County
My Commission Expires Mar 30, 2014

APPENDIX 1.
Complete 95% Confidence Intervals.

Table 2 exhibits the complete 95% confidence intervals associated with Table 1.

95% CONFIDENCE INTERVAL	MMWR study's confidence interval	Alternative confidence interval
No parameter corrections	[1.3, 9.0]	[1.02, 9.14]
Correcting MBP pop. within Hasidic / Yeshiva communities	[0.9, 6.11]	[0.71, 6.38]
Fully correcting MBP pop. assumption	[0.72, 4.90]	[0.58, 5.22]

TABLE 2

APPENDIX 2.Confidence Interval for the Risk Ratio, based on Poisson approximations.

In this Appendix, I describe a classical, alternative method to derive the confidence interval for the risk ratio. The method can be found in each of the three classical textbooks mentioned above: see section 4.5 in Lehmann(1986), pp.344-345 in Fleiss et al. (2003) and pp.254-255 in Rothman et al. (2008). It is this confidence interval which is discussed in points 14. and 15. ,above, and was used to obtain the numbers in the second column of Table 1, above.

Let
X= the number of HSV-1 babies identified among the general non-MBP population.
Y= the number of HSV-1 babies among the MBP population.
n1= the size of the general non-MBP population during the study period.
n2=the size of the MBP population, during the study period.

The CDC study claims the following values for these four quantities:
X=25
Y=5
n1=352,411
n2=20,493
(See, however, the discussion as to why the reported values of X and Y are dubious , and why the value of n2 needs to be corrected):

Let p1= π=true probability of HSV-1 infection among the general non-MBP population.
p2= $\vartheta\pi$= true probability of HSV-1 infection among the MBP population.
Note that ϑ denotes the true risk ratio.

As explained, the true distributions of X and Y are Binomial but these distributions are accurately approximated by Poisson distributions with mean n1*p1 and n2*p2, respectively. Since X and Y are independent random variables, the conditional distribution:
$Y \mid X+Y$ has a Binomial distribution with parameters n=X+Y=30 and incidence probability α, where $\alpha = n2*p2/(n1*p1+n2*p2)=\vartheta n2/(n1+\vartheta n2)$.

The exact 95% Clopper-Pearson confidence interval for the incidence probability α of a Binomial distribution, based on observing X=5 is [0.056, 0.347]. Since $\vartheta = n1*\alpha/(n2*(1-\alpha))$, *i.e.*, ϑ is an increasing function of α, it follows that the lower end point of the 95% confidence interval for the risk ratio ϑ is given by $\vartheta=1.02$; this is the first value reported in the second column of Table 1.

The second and third numbers in this column are obtained by replacing n2 by n2=29371 and n2=35818 respectively. As explained, these values represent, respectively, a corrected estimate for the size of the Hasidic/ Yeshiva MBP population, and a further correction to account for MBP cases in the remainder of the Jewish community.

APPENDIX 3.
The odds of at least one *mohel* experiencing a cluster of 2 or more cases in two consecutive years during a 5-year window.

In this Appendix, I explain how the odds of the above cluster event can be computed. As mentioned above, it is very questionable whether this cluster event actually took place during the years 2000-2005, and we know that it did not occur during the subsequent study period. However, the objective of this Appendix is to demonstrate that the odds of the above clustering event occurring is anywhere between 9-12%, so that no one should be surprised by the appearance of a cluster event even if it actually occurred.

As explained in the Affidavit, the total number of MBP circumcisions during the study period is unknown, one of several limitations of the MMWR statistical study. Some of the parameters in the calculations are therefore somewhat tentative. The purpose of this analysis is not to arrive at a precise calculation of the odds, but to demonstrate that its order of magnitude is 10% or more. The notation a^b is used to denote the b-th power of the number a; for example 3^2 =9, the square of 3 and 2^3 =8 the number 2, raised to the power 3.

This analysis uses 35818 as the number of MBP circumcisions during the 5.75-year study period, based on my initial affidavit's direct estimate of the size of the Hasidic and Yeshiva components of the MBP population and this affidavit's use of a 2:9 ratio to determine the incidence of MBP outside those communities. This implies an annual number of MBP circumcisions of 6229 (line 4 of the table).

The next important parameter is how many MBP circumcisions are performed by an average MBP *mohel*, and hence how many MBP *mohelim* practiced in New York City in the past 12 years. A professional *mohel* performs at least 200 circumcisions per year (roughly one every other day); the busiest among them may do as many as 450 per year. Given the uncertainty about this parameter, I have performed the analysis for three separate values: 200, 250, and 300, in the three columns of the Table (line 5). Given an assumed value of the average number of circumcisions per *mohel*, the number of *mohelim* (m) follows, on line 6, as the ratio of line 4 and 5. (The numbers are rounded down to the nearest integer.)

Assuming that the infection risk per baby is that of the general NYC population, *i.e.*, 8 per 100,000 (line 7), it follows that the total number of infected cases experienced by a single *mohel* in a single year is Binomially distributed with a mean given by the product of the infection rateper baby (line 7) and the number of circumcisions performed (line 5). This mean μ is exhibited in line 8. Lines 9 and 10 exhibit the likelihood that this Binomially distributed random variable equals zero (p0) or one (p1), respectively. (It is well known that the Binomial distribution is very accurately approximated by a Poisson distribution with the same mean, when the number of trials (200-300) is relatively large and the incidence rate per trial very low (8*10^-5).) If X denotes a Poisson distributed random variable with mean μ, Prob[X=0]=exp(-μ) and Prob[X=1]=μ*exp(-μ).)

For a *specific mohel* to avoid two infections in two consecutive years within a 5-year window, one of the following 5 year, patterns must prevail, with 0 indicating a year with 0 infections, and 1 indicating a year with 1 infection:

(a) *All five years without any infection*
(00000): probability=p0^5

(b) *One year with one infection, all other years without any infection*:
(10000), (01000), (00100), (00010),(00001): probability of each of these 5 patterns = p1*p0^4

(c) *Two nonconsecutive years with one infection, all other years without any infection:*
(10100),(10010),(10001),(01010),(01001),(00101); probability of each of the 6 patterns=(p1^2)*(p0^3)

(d) Threeyears with one infection, no two of which are consecutive; *no other infections.*
(10101); the probability of this single pattern equals (p0^2)*(p1^3)

The combined likelihood of these various patterns is computed using the numbers in lines 9 and 10, and added up to arrive at the number π in line 11. Thus, any *specific mohel* faces the possibility of this tragic event with the complementary likelihood (1-π) varying from 0.3% to 0.7%. However, the likelihood (L) of at least one *mohel* being faced with this event is the complement to 1 of the likelihood that all *mohelim* avoid it. This implies that L=1-π^m, the bottom line result in line 12.

line	**MOHEL LIKELIHOOD ANALYSIS**			
1	window k	5	5	5
2	# MBP cases	35818	35818	35818
3	# years	5.75	5.75	5.75
4	#MBP cases/yr	6229	6229	6229
5	# MBP per mohel	200	250	300
6	m=# *mohelim*	31	24	20
7	general incidence rate	0.00008	0.00008	0.00008
8	μ=poisson mean # cases/mhl	0.016	0.02	0.024
9	p0=prob of zero cases/mhl/yr	0.984127	0.980199	0.976286
10	p1=prob of one case/mhl/yr	0.015746	0.019604	0.023431
11	π=prob of no two consec years with at least 1 c in 5 yrs	0.996992	0.995372	0.993437
12	prob at least one *mohel* experiences 2+ cases in 2 yrs	0.089	0.105	0.123

TABLE 3