Exhibit 11

UNITED STATES DISTRICT COURT

NORTHERN DISTRICT OF CALIFORNIA

IN RE: ROUNDUP PRODUCTS LIABILITY LITIGATION	)	MDL No. 2741
THIS DOCUMENT RELATES TO ALL CASES	)	Case No. 16-md-02741-VC

TUESDAY, JANUARY 23, 2018

CONFIDENTIAL - PURSUANT TO PROTECTIVE ORDER

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VIDEOTAPED DEPOSITION of JENNIFER R. RIDER, ScD, held at the offices of Cetrulo LLP, 2 Seaport Lane, Boston, Massachusetts, commencing at 2:39 p.m., on the above date, before Maureen O'Connor Pollard, Registered Merit Reporter, Realtime Systems Administrator, Certified Shorthand Reporter.

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PROCEEDINGS

THE VIDEOGRAPHER: We are now on the record. My name is Chris Coughlin, and I'm a videographer for Golkow Technologies. Today's date is January 23, 2018, and the time is 2:39.

This video deposition is being held in Boston, Massachusetts in the matter of Roundup Products Liability Litigation, MDL Number 2741, United States District Court, Northern District of California, Case Number 16-md-02741-VC.

The deponent is Dr. Jennifer Rider.

Will counsel please identify yourselves and state whom you represent.

MR. WOOL: David Wool from Andrus Wagstaff for the plaintiffs.

MR. TRAVERS: Jeffrey Travers with The Miller Firm for the plaintiffs.

MR. LASKER: Eric Lasker, Hollingsworth LLP, for Monsanto.

MR. WOOL: The court reporter is Maureen O'Connor, and she will now swear in the witness.

JENNIFER R. RIDER, ScD,
having been first duly identified and sworn, was examined and testified as follows:

EXAMINATION

BY MR. WOOL:

Q. Good afternoon, Dr. Rider.
A. Hi.
Q. How are you doing?
A. Good. Thank you.
Q. So one, I know we haven't met before, and I know that you've been through this process before, but I can tell that you are somewhat eager to give your answers, so if you'll just give me a moment to finish my questions and let counsel get in his objections where he wants to, I think that will make the court reporter's job a little bit easier.
A. Okay.
Q. Fair enough?
A. Mm-hmm.
Q. So I have marked as Exhibit 1 a copy of your supplemental expert report.
A. Mm-hmm.
Q. Now, does that expert report, along with the original expert report that you

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authored in this litigation, include all of your opinions for the Andreotti study?
MR. LASKER: Objection to form.
A. I can't say that it includes all of my opinions, but I felt like it covered the most important issues.
BY MR. WOOL:
Q. Since you authored that report, have you read anything that changes any of the opinions that are described in that report?
A. No.
Q. Now, in preparing that report, did anybody assist you in summarizing literature?
A. No.
Q. You wrote the report by yourself without the assistance of, say, a grad student or a teaching assistant?
A. That is correct.
Q. All right. And have you read any of the plaintiff expert reports that were submitted pursuant to Pretrial Order 34, which is the Order that required the supplemental reports?
A. Yes, I have.
Q. Which expert reports have you read, if you recall?

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A. Yes. I have read Dr. Ritz's report, and Dr. Neugut's report, and I believe I also took a quicker look at Dr. Portier's report.
Q. All right. Do you believe that exposure is accurately assessed in the Andreotti study?
A. So I think that the authors did a good job of making sure that even if there would be some imperfect measurement of exposure like we have in all epidemiologic studies, that it had very little impact on the findings.
Q. So it's fair to say that all epidemiological studies have some degree of inaccuracy?
A. Yes. I don't think we could find an example of an epidemiologic study where there's absolutely perfect reporting when you're talking about a long-term follow-up of participants.
Q. Have you ever collected occupational exposure data for an epidemiological study?
A. No. I don't do occupational epidemiology, but I certainly do epidemiology that deals with questionnaire data and cancer outcomes.
Q. Now, have you ever designed a

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questionnaire for an occupational exposure study?
MR. LASKER: Objection to form.
A. Like I said, I don't do occupational epidemiology in my own research, so no, I wouldn't have designed those types of questions. But I have contributed to questions that have appeared on questionnaires for fairly high profile observational studies.
BY MR. WOOL:
Q. Just to make things easier for me, how does occupational exposure epidemiology differ from what you do?
MR. LASKER: Objection to form.
A. So in my view, I think occupational epidemiology is just looking specifically at exposures a person would encounter while they are working. But the -- how we handle these exposures in study design and analytics really isn't very different from looking at any other lifestyle exposure, which is something that I do in my own work.
BY MR. WOOL:
Q. And you've served as a peer review -- strike that.

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You have served as a peer reviewer for various epidemiological journals, correct?
A. That is correct.
Q. And in your capacity as a peer reviewer, have you ever peer-reviewed any occupational exposure studies?
A. I can't recall a specific example, but it wouldn't surprise me if I have.
Q. So it's possible?
A. It's very possible that I have. But I have peer-reviewed a lot of articles.
Q. Fair enough.
And the Andreotti study evaluated exposure data, correct?
A. Well, they related GBH use to various cancer outcomes, yes.
Q. Let's -- actually I'm just going to mark this as Exhibit 2, which is the Andreotti study.
MR. WOOL: Here is a copy of it, Counsel.
And I will mark as Exhibit 3 a copy of the enrollment questionnaire.

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(Whereupon, Exhibit Number 33-1, Supplemental Expert Report of Jennifer R. Rider, ScD, Number 33-2, Andreotti, et al article, Glyphosate Use and Cancer Incidence in the Agricultural Health Study, and Number 33-3, Agricultural Health Study, were marked for identification.)

MR. LASKER: Which one?

MR. WOOL: This is the private applicator questionnaire.

BY MR. WOOL:

Q. Now, have you seen this document before?

A. The private applicator questionnaire, or this study? Sorry, which one are we talking about?

Q. The private applicator questionnaire, Exhibit 33-3.

A. I have gone online to the Agricultural Health Study website, but I believe I have spent more time looking at the commercial applicator questionnaire.

Q. The commercial applicator enrollment questionnaire?

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A. Correct. Yes.

Q. And the Andreotti study evaluated both private applicators and commercial applicators, correct?

A. Yes. So they all were people who were enrolled while they were getting their pesticide license, their applicator license.

Q. All right. So if you look at Exhibit 3, Page 10, at the very top you will see questions about Roundup, Jury, or glyphosate.

Do you see that?

A. I do.

Q. And the questionnaire asked whether the cohort member has ever personally mixed or applied the herbicide, how many years they personally mixed or applied the herbicide, and an average how many days per year they used it along with when they first used the herbicide, correct?

A. Mm-hmm.

Q. All right. And this is the -- so the questions on Page 10 on, I believe, row H are the questions that form the baseline exposure assessment for the Andreotti study for private applicators, correct?

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MR. LASKER: Objection to form.

A. I would have to look more carefully at this document to confirm that.

(Witness reviewing document.)

A. Yeah, I mean, it seems reasonable that this is the enrollment questionnaire and not the take-home questionnaire, which they also did. But I can't be sure just from looking at this, no.

Q. Well, if you turn to the very first page, you will see --

A. Enrollment questionnaire, yes. Thank you.

Q. So we agree this is the enrollment questionnaire --

A. Yes.

Q. -- by private applicators.

Now, cohort members were also asked about the use of protective equipment, correct?

A. Correct.

Q. And if you turn to Page 15, Question 17 asks about the use of protective equipment, correct?

A. That is correct.

Q. And Question 17 is not specific to any

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herbicide, is it?

A. It just says, "What type of protective equipment do you generally wear when you personally handle pesticides?" So that is correct.

Q. And so if somebody used more than one pesticide and multiple types of protective equipment, this questionnaire would not distinguish which specific type of protective equipment applied to which pesticide, would it?

A. It doesn't appear this questionnaire would. I believe if we looked at the commercial pesticide applicator questionnaire, that one distinguishes at least by different classes of pesticides.

Q. Do you know approximately how many of the cohort members were commercial applicators versus private applicators?

A. I would need to look back. I don't recall offhand, no.

Q. I believe if you look at -- let's see. Well, we can go back to that actually.

A. Okay.

Q. Now, if you look at Question 16 still in the questionnaire, Page 15.

A. Okay.
Q. Question 16 asks, "How do you personally apply pesticides?" Correct?
A. Correct.
Q. And Question 16 does not ask cohort members to describe the application method specific to a pesticide, does it?
A. That is correct. Again, I think this is different than in the commercial questionnaire. But on this one, that is correct.
Q. So if somebody used multiple pesticides, again we would have no way of knowing which application method applied to which pesticide, would we?
MR. LASKER: Objection to form.
A. That is correct. I mean, we would know how frequently they used particular pesticides, but not the application method specifically for each one.
BY MR. WOOL:
Q. Okay. Now, if we look at the Andreotti study, Table 1, I think there's a breakdown of the percentage of private applicators versus commercial applicators, if

you go under "applicator type."
Do you see that?
A. Yes, I do.
Q. Okay. And at least for the kind of ever-never -- or not never side. Okay.
And so in the first column we are looking at -- sorry, strike that.
In the first column of Table 1 we are looking at never used glyphosate, correct?
A. Correct.
Q. And the total number of private applicators is 8,476?
MR. LASKER: Objection to form. Mischaracterizes the document.
A. So there are 91 percent of those who never used glyphosate were private applicators, meaning that's the majority of glyphosate users were commercial applicators.
BY MR. WOOL:
Q. Okay. And the cohort members were all restricted use pesticide applicators, correct?
A. They all had to have their license. That is how they were enrolled.
Q. What is a restricted use pesticide?
A. I'm not exactly sure what goes into

the licensing requirements. I just know that that is how they enrolled the cohort, so that in many ways the cohort would be more able to give good quality information on pesticide use.
Q. Is the use of personal protective equipment something that could impact actual exposure to glyphosate?
A. I mean, I think we have data from several biomonitoring studies that suggest that yes, it does.
Q. And in what way does the use of personal protective equipment impact actual exposure to glyphosate?
A. Well, I mean, I think it's reasonable to think that if you are wearing personal protective equipment, you might have a lower internal dose of glyphosate exposure.
Q. And how did the authors in Andreotti use the information about personal protective equipment?
A. They used it in their intensity measures. There's an algorithm for which they calculated intensity of use, and personal protective equipment was incorporated into that algorithm.

Q. Are you familiar with the term exposure misclassification?
A. Yes, I am.
Q. What does that term mean to you?
MR. LASKER: Objection to form.
A. It means the degree to which you are assigning a participant the wrong value for exposure, and it can be either differential or non-differential with respect to the outcome.
BY MR. WOOL:
Q. And what is the difference between differential and non-differential just so we're clear?
A. So non-differential -- if we're talking about exposure misclassification, non-differential exposure misclassification would mean that you're providing the wrong information on exposure equally as often in those who do and do not go on to develop the outcome of interest, whereas differential, there would be some difference in that misclassification according to disease status.
Q. And is non-differential exposure misclassification a type of systematic error that can occur in an epidemiological study?

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MR. LASKER: Objection to form.
A. I guess it is a type of error that could create a bias, if that's what you mean by systematic error.
BY MR. WOOL:
Q. Have you heard the term systematic error before as it relates to epidemiological studies?
A. Yes, some people use that term. But it's one of these terms that's used to mean very different things.
Q. Just so I'm clear, if I use the term, what does the term mean to you?
MR. LASKER: Objection to form.
A. So I wouldn't really talk about systematic error. I would talk about the specific type of bias that we're talking about.
BY MR. WOOL:
Q. Okay. Do you believe that exposure misclassification can bias the results of an epidemiological study towards the null?
MR. LASKER: Objection to form.
A. So you're asking if non-differential misclassification can bias the results toward the null?

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BY MR. WOOL:
Q. Yes.
A. On average, that tends to be what happens, especially when we're looking about dichotomous exposures, but there's also random error. So you can't always expect that a point estimate will be biased towards the null in a given study.
Q. And can exposure misclassification impact the relative risk of a study?
MR. LASKER: Objection to form.
A. So again, are you talking specifically about non-differential exposure?
BY MR. WOOL:
Q. Let me clarify my question.
Can non-differential exposure misclassification impact the relative risk estimate in an epidemiological study?
A. Yes, it can.
Q. Can it have a substantial impact upon relative risk estimates?
A. It depends on a number of factors. So again, if we're just talking about non-differential misclassification of exposure, you know, we need to know the degree to which

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the exposure is being misclassified. And then I also mentioned there's also this random error issue, and so, in general, that's less of a problem in larger studies.
Q. And accurate exposure assessments are important in large cohort studies, correct?
MR. LASKER: Objection to form.
A. Well, yeah. I mean, I think our goal is to measure the exposure that we're thinking to -- that we're hoping to relate to a particular outcome. And so in my own work I want to get as close as I can to measuring that exposure correctly.
BY MR. WOOL:
Q. And do you believe the exposure assessment in the Andreotti study is accurate?
A. So I think they did a number of things to ensure that they were getting very good exposure reporting. I think, you know, first of all, just including a cohort of licensed applicators, they were likely to get much better information than, say, in some of the case-control studies that had been conducted previously. And they also -- you know, they have the initial baseline exposure, they have

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follow-up exposure, they look at exposure classified in several different ways, so yeah, so I think they did a good job in measuring the exposure.
Q. And what is selection bias, just so we're clear on that term?
A. So the sort of structural definition of selection bias is when you're conditioning on an effect of both exposure and disease. So in other words, one example would be if -- you know, getting into the analysis of your study depends on both exposure and disease.
Q. Now, if we turn to Page 7 of Exhibit 2, which is the Andreotti study.
A. Okay.
Q. If you look in the left-hand column.
A. Sorry, we're on Page 7 you said?
Q. Correct.
A. Yes.
Q. I mean, I'm sorry, not the left-hand column, the right-hand column.
A. Okay.
Q. Okay. At the top of the paragraph, second to the bottom, the authors state that "This evaluation has some limitations that

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should be acknowledged. First, despite the specific information provided by the applicators about use of glyphosate, some misclassification of exposure undoubtedly occurred."

Do you agree with that statement?

A. Like I said, I think we would be very hard-pressed to find an epidemiologic study where there was absolutely no misclassification of exposure, especially when we're dealing with lifestyle or occupational behaviors. So in that way, yes, I would agree.

Q. And does misclassification always bias the results of a study in a particular direction; for example, towards the null or away from the null?

MR. LASKER: Objection to form. Asked and answered.

A. Yes, so I did -- this is what I stated previously. But in general, non-differential misclassification of exposure would bias the results towards the null, especially if you are just talking about a dichotomous exposure. But we have random error issues as well, and so the point estimate you obtain from a single study wouldn't always be necessarily closer to the

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null.

BY MR. WOOL:

Q. Now, if we go to -- back to your report --

A. Okay.

Q. -- to Page 4, I want to ask you about the last sentence of this top paragraph.

MR. LASKER: I'm sorry. I was on Page 4, but the wrong document. So Page 4, yes.

BY MR. WOOL:

Q. Okay?

A. Yes.

Q. The last sentence you state that while -- sorry, strike that. You state, "While this theoretically could lead to a shift in the relative risk for any individual category in either direction, because the reported relative risk in all categories in Andreotti, et al results are below 1.0, it is impossible for non-differential exposure misclassification to conceal any positive associations in that data," correct?

A. Mm-hmm.

Q. And so is it your opinion, as you sit here today, that it is impossible that exposure

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misclassification could have concealed a positive association in the Andreotti study?

A. So I think you're taking that sentence a bit out of context, because I'm talking in this whole paragraph about a specific situation where you're looking at exposure in multiple categories, and you have exposure misclassification specifically between two of those categories, those would bias the results towards each other. So in that particular situation you could have the results for one category go towards the null and another category go away from the null.

But here in the HS 2018 study that we're talking about, all of the results for every category are below 1, you know, except for the reference value, of course, which is 1, so there's no way for two categories to be biased towards each other and for one to be then away from the null.

Q. Okay. So if I understand, and I'm not sure that I do, you're saying that it is impossible for the results of -- strike that.

So because all the results are going away from the null, you're saying that it is

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impossible that exposure misclassification could lead to that result?

MR. LASKER: Objection to form.

BY MR. WOOL:

Q. Did I get that?

A. No, that's not what I said.

Q. Okay. Sorry. I just want to make sure that I'm clear. This isn't a trick question or anything like that.

A. It might be helpful to actually look at the results in Table 2.

Q. Table 2 of Andreotti?

A. Correct.

So if we're looking at the results for NHL, you can see there's -- the reference group that's the no exposure category, and then all of the quartiles of exposure have relative risk estimates that are below 1. Right?

Q. Correct.

A. So it is impossible for this one specific situation that I'm describing where non-differential misclassification can actually in categorical exposure evaluations can drive the relative risk away from the null theoretically, but it can't happen here because

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every single category has a relative risk below 1.
So if you were misclassifying two categories, only two categories, it's possible that they would be biased towards each other. But here that towards each other would still result in estimates below 1.
Q. And so is it your opinion that the Andreotti study is a negative -- or produces a negative result?
MR. LASKER: Objection to form.
A. So that is a term -- I don't know what you mean by "a negative result."
BY MR. WOOL:
Q. So a result -- or a relative risk of 1 is a null result, correct?
A. That would be a null result, yes.
Q. And so, I believe you used the term negative result in your original report, so my apologies if you didn't.
But would a result less than 1 indicate that the substance at issue in this case, glyphosate, has a protective effect?
A. So this is exactly why I think it's very important to always look at the confidence

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intervals and not just the point estimate, so absolutely not. I would not regard this as a protective association. I would call this consistent with no association.
Q. And that's because 1 is within the confidence intervals?
A. That is correct.
Q. All right. Did you do any research on the potential effects of exposure misclassification prior to writing your report?
MR. LASKER: Objection to form.
A. I guess I don't quite understand what you mean. I mean, I think I evaluate all of my own studies that I do in terms of exposure misclassification, and I teach exposure misclassification, but I don't think any of my substantive research is on the issue of exposure misclassification specifically.
BY MR. WOOL:
Q. Forgive me, this isn't on your reliance list. Actually we can go back to that.
Have you heard the term baseline misclassification before?
MR. LASKER: Object to the form.
A. No, I wouldn't know what you meant by

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that term.
BY MR. WOOL:
Q. Okay. So let's just talk about potential misclassification at enrollment.
A. Okay.
Q. Okay. So we have the enrollment form. So is it correct that the cohort members were asked to detail past pesticide use at enrollment?
A. That is correct.
Q. And they were asked to recall several different pesticides, correct?
A. I believe 50 pesticides.
Q. And they were asked to recall the frequency of use for each of those pesticides, correct?
A. That is correct.
Q. And they were asked to do that by filling out a questionnaire?
A. The enrollment questionnaire, correct.
Q. And do you know -- strike that.
Were the cohort members able to compare answers to their own records prior to filling out the questionnaire?
A. What types of records do you mean?

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Q. For example, purchase records.
A. I'm not sure actually if they were able to do that.
Q. Do you know if they were permitted to ask family members about their own use to, say, corroborate their memory?
A. I'm not sure.
Q. Okay. And in the Andreotti study, the relative risks were calculated by comparing the exposed group to the unexposed group, correct?
MR. LASKER: Objection to form.
A. Yes, or in their primary analyses every quartile of use was compared to no exposure.
BY MR. WOOL:
Q. And in the De Roos 2005 paper, which I believe you relied on for your original report, correct?
A. Correct.
Q. The authors of that study looked at the comparison between the upper quartiles and the lowest quartile, correct?
MR. LASKER: Objection to form.
A. So in their dose-response analyses that's correct, it was done slightly

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differently, and the reference group was the lowest exposed group, but they also provided ever-never analyses where they compared it to no exposure. So it was just presented slightly differently, but the same information was available.
BY MR. WOOL:
Q. And do you believe that one of those methods is more reliable than the other?
A. So I mean, I think when we're interested in determining causal associations, what we're really interested in is the comparison of exposure and different levels of exposure to no exposure. So I think if you're looking to make sort of causal inferences from your study, I think in a lot of ways that makes sense.
Q. Is it possible to accurately quantify exposure misclassification in the Andreotti study?
MR. LASKER: Objection to form.
A. So I think the authors did go through a number of different analyses, and there were also external studies that look at the degree to which exposure misclassification could be an

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issue, and I think those studies show that the determination of exposure is quite good, and certainly in line with other types of lifestyle exposures that we typically measure.
BY MR. WOOL:
Q. But I guess my question was a little bit different.
Is it possible to measure -- to get a precise measurement, I should say, for exposure misclassification in the Andreotti study?
A. Right. So we can do sensitivity analyses to determine the degree to which different levels of exposure misclassification could affect our results, but, you know, we can never definitively say that there is this certain degree of exposure misclassification in a particular study, because we would never have that information.
But I think the important thing is that you look to see how much that would impact your findings, and then if it appears that it would have very little impact you can have confidence in your conclusions.
Q. Okay. So let's go to Page 10 of your expert report.

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A. Okay.
Q. I'm sorry. Strike that. We'll go back to that.
Now, in the questionnaires, is it possible that some exposure misclassification occurred at enrollment?
A. So you're asking is it possible that not every applicator correctly reported every single occurrence of every pesticide?
Q. Correct.
A. Yes, that is a possibility.
Q. Okay. And what's the potential effect on Andreotti of misclassification at enrollment?
A. Well, again, I mean, I think there is good data from several external studies that say, you know, they're in the Blair 2002 study, that agreement between pesticide reporting from one year to the next is actually very good. So if it's quite precise, we would expect that to have very minimal impact on the findings.
Q. And so is it your opinion, as you sit here today, that pesticide applicators accurately report use year-over-year?
MR. LASKER: Objection to form.
A. Again, I think that there is always

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some degree of error in reporting of exposure. But, you know, here what we're really trying to do is put people into categories of exposure ranging from low use to high use. And if we can do a reasonable job in estimating their exposure, we can at least get them into the right category, and then our inferences will be valid.
BY MR. WOOL:
Q. And you just mentioned Blair 2002. Let's take a look at that, which I'll mark as Exhibit 33-4.
(Whereupon, Exhibit Number 33-4, Blair, et al article, Reliability of Reporting on Life-Style and Agricultural Factors by a Sample of Participants in the Agricultural Health Study from Iowa, was marked for identification.)
BY MR. WOOL:
Q. And this is the study that you just described?
A. That's correct.
Q. All right. And how did Blair determine that pesticide applicators gave

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reliable reporting for pesticide use?
A. So I agree -- well, they say here in the last sentence of the introduction, "We took advantage of a special situation in Iowa to assess the reporting consistency for agricultural and lifestyle factors on a sample of the cohort that completed two questionnaires approximately one year apart." So I think something happened in Iowa regarding licensing, and so they were able to obtain data on, I believe it's about 4,000 people twice. 2,895 applicators, and a second group of 1,193.
Q. Now, if we turn to Page 96 of the Blair study, look at Table 2. For the days per year mixed or applied statistic, what is the exact agreement for glyphosate?
A. So that would be .71 with a confidence interval of .67 to .75.
Q. Okay. And Blair reports 52 percent exact agreement for glyphosate for days per year mixed or applied, correct?
A. Sorry, where are you getting that?
Q. In Table 2, if you go down you'll see days per year mixed or applied on the far left-hand column.

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A. The exact agreement, yes, 52 percent with a kappa of .71, yes.
Q. And that 52 percent is telling us that the categories are identical for 52 percent of the responders, correct?
A. Well, that's true. But if you go down to the text below the table, they also say that agreement within one category of exact agreement was 98 and 99 percent, much higher than for any category.
So this is actually the advantage of looking at the kappa statistic is the kappa statistic takes into account sort of chance agreement that can happen, but it can also tell you about how close you are to the correct answer, not just whether you're right or wrong.
Q. And so what does that mean when the authors say agreement within one category of exact agreement was 98 and 99 percent?
A. So I believe it means that, you know, if the person would have been classified in the second group -- hold on.
So if the correct category was the second group, you know, 98 or 99 percent of people would have put themselves in the first

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group or the third group, just as an example of what that means by within one category, as an example.
Q. Right. I think I was misunderstanding of your answer.
A. Okay.
Q. And again, the groups -- or strike that.
The categories are less than 5, 5 to 9 --
MR. LASKER: You're talking about days?
MR. WOOL: Sorry, the categories.
MR. LASKER: Can you show us where you were.
MR. WOOL: For days per year of use categories.
MR. LASKER: So this is the footnote to Table 2?
BY MR. WOOL:
Q. Yes, the footnote to Table 2. The categories of less than 5, 5 to 9, 10 to 19, 20 to 39, 40 to 59, 60 to 150, and more than 150?
A. Correct.
Q. And your testimony is that exact

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agreement was 98 and 99 percent within those categories, so 99 -- or 98 to 99 percent of the people who, say, reported as being within 10 to 19 one year were in 20 to 39 the year before, is that right?
A. As an example, correct, yes.
Q. Okay. And so based on this result, which is, again, the days per year mixed or applied, do you believe that the mixed or applied data that's reported in Andreotti, et al is accurate?
MR. LASKER: Objection to form.
A. So as I've said before, I think this lends support to the fact that the relative risks that we're estimating are by and large capturing people with regard to whether they're, you know, low exposed or high exposed individuals and relating that to unexposed individuals. So while yes, some people may have been not perfectly classified, you would still be able to identify elevations in the relative risk across those categories.
Q. Okay. Now, you talk about selection bias in your expert report.
A. Okay.

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Q. And this time we're going to stick on your expert report for a second. If you go to Page 4.
A. Okay.
Q. You had talked about some of the different strategies that have been used by the Andreotti authors to validate the results of the study a couple moments ago, and at kind of the bottom paragraph in the middle you begin talking about the Montgomery study, correct?
A. Yes. I see that, yes.
Q. And what is the pertinence of the Montgomery study?
MR. LASKER: Objection to form.
A. Well, the Montgomery study is one of the studies that looked at how likely or unlikely selection bias was to occur in the Agricultural Health Study.
BY MR. WOOL:
Q. And how did the authors of Montgomery do that?
A. So they look at the differences in the population in terms of responders and non-responders to the follow-up questionnaire, and they find that even though there are some

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differences among non-responders in many variables that they measured, they do analyses to show that this really doesn't have a meaningful impact on the results.
Q. Okay. And I've actually marked as 33-5 the Montgomery study.
(Whereupon, Exhibit Number 33-5, Montgomery, et al Author Manuscript, Characteristics of non-participation and potential for selection bias in a prospective cohort study, was marked for identification.)
A. Okay.
BY MR. WOOL:
Q. And if you look at the conclusions in the abstract section on the first page, the authors note that "Differences between non-participants and participants in follow-up interview were generally small, and we did not find significant evidence of selection bias. However, the incidence of bias may depend on the specific exposure and outcome under study."
Did I read that correctly?
A. Yes, you did.
Q. What do they mean by "the specific

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exposure and outcome under study"?
A. So they here in this study looked at specific exposures and outcomes, and selection bias phenomenon could -- you know, it's not general across a study population, you would need to take into account the specific exposure and disease outcome of interest.
Q. And what exposures did they look at in Montgomery?
A. Let's see. This is one, I believe, they looked at smoking.
Q. I think if you look at the bottom of Page 2.
A. Okay. They looked at chloro -- you're going to make me say that.
Q. No, I won't make you say it.
A. With prevalent depression, smoking with prevalent chronic lung disease, and smoking with incident cancer.
Q. So they did not look at glyphosate and non-Hodgkin's lymphoma, correct?
A. That is correct, they did not. Yes, they did not look at glyphosate and NHL specifically.
Q. And would you view -- strike that.

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And is it possible that there are -- actually, strike that.
And in your expert report you note regarding Montgomery that these results provide evidence that selection bias due to follow-up, survey non-responses not necessarily a major concern, though this issue should also be considered with respect to GBH, which is glyphosate-based herbicides, and NHL specifically?
A. Yes. And I think the authors did that in their sensitivity analyses.
MR. LASKER: Just clarify, authors of what?
BY MR. WOOL:
Q. Go ahead. Yes, fair enough.
A. Sorry. The Andreotti, et al authors in the most recent 2018 publication did several sensitivity analyses that addressed concerns about selection bias from non-response.
Q. Okay. But to the extent that you rely upon Montgomery, this article assumes that glyphosate response patterns are the same as the other pesticides measured in this one, which is chlorpyrifos with a prevalent depression, for

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example?
MR. LASKER: Objection to form.
A. No, I wouldn't characterize it that way, because, again, the primary analysis that was done in the JNCI Andreotti study used multiple imputation, and so the patterns don't need to be exactly the same, you just need to have measured all of the variables which influence response. So they collected all of this other information on, you know, many, many covariates in this questionnaire, and then could use those to predict glyphosate use. So it doesn't necessitate that the patterns need to be the same.
BY MR. WOOL:
Q. Do you agree, though, that the extent of bias could be dependent upon the particular outcome, for example non-Hodgkin's lymphoma?
MR. LASKER: Objection to form.
A. So I think I would be much less concerned about in this study, because we have complete information on all of the outcomes through cancer registry, so this isn't a situation where loss to follow-up causes us to miss some of the cancer cases. So no, I

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wouldn't be concerned about that.
BY MR. WOOL:
Q. So let's talk about that for just a moment.
How were the outcomes captured in the Andreotti study?
A. They used linkage to cancer registries. So they're not relying solely on self-reported outcome data.
Q. Which cancer registries?
A. I believe the state cancer registries, but they also do a search of the death index.
Q. Okay. So if somebody moved out of the state of North Carolina and then developed non-Hodgkin's lymphoma, would that be captured by the Andreotti study?
A. If someone left the state prior to their cancer diagnosis, it is possible that you would miss that case, unless that person died of NHL, and then you would most likely capture them through the death registry.
This is the same method we use in the cohorts that I work on. And like this study, you know, you can say that cancer outcomes are captured at least 98 percent of the time.

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Q. And the Andreotti study that's the subject of your supplemental report adjusts for the presence of confounders, correct?
A. That is correct.
Q. And some of the confounders that the study adjusted for are other pesticides, correct?
A. Yes, they included other pesticides.
Q. And, in fact, one of your opinions in this litigation is that plaintiffs' experts failed to properly adjust for confounders, correct?
A. Could you provide a little more -- that the plaintiffs experts failed to adjust?
Q. Yes. I believe it is. Let's see, for example, on Page 10 of your supplemental report at the very bottom, there's a heading "Overadjustment for Other Pesticides."
A. So that specifically was about an issue that Dr. Ritz raised in her deposition where she was saying that when you adjusted for other pesticides, you could over-adjust and would somehow wash out the effect of GBH and NHL. But I disagree with that. I think that, you know, the most important issue is to -- for

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your results to be internally valid is to make sure that you don't have a common cause of exposure and outcome. It's probably the most basic epidemiologic principle.
It's interesting that in this particular study, the Andreotti JNCI study, the adjustment for those potential confounders didn't have any appreciable affect on the results. But that doesn't mean that those things they adjusted for couldn't be confounders in another study where the population was less homogenous.
Q. So would it be fair to say that it would be improper to fail to adjust for a known confounder in an epidemiological study?
A. I think if you know something that is a common cause of the exposure and the outcome, you would adjust for it, if what you're interested in is interpreting your results causally.
Q. And if something is a potential confounder, is adjustment required for an epidemiological study to be reliable?
A. I think in general if we don't know whether a variable is a confounder, and by that

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I specifically mean a variable that is a common cause of the exposure and the outcome, you first use your sort of biological knowledge about that variable and its relationship with exposure and disease to determine whether or not you should adjust for it.

Q. So if I understand your answer correctly, sometimes yes, sometimes no, it's just something that requires kind of more granular focus depending on the substance, is that fair?

A. Well, I think this is why epidemiologists need to know, you know, something about the relationship between the exposure and the outcome to determine what those potential confounders might be.

The wrong approach is just simply, you know, throwing everything in a model. You have to think that that could actually be a common cause potentially of the exposure and the outcome.

Q. So you must think that a substance is a common cause of both the exposure and the outcome to rule it in as a confounder?

MR. LASKER: Objection to form.

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A. As a potential confounder, correct.

So just to clarify, we wouldn't want to put in our model anything that we thought was an intermediate between our exposure and our outcome, because if you adjust on an intermediate it may take away some of the real causal effect of that exposure on the outcome, but your statistical model can't tell you the difference between the situation where that variable is a confounder and the situation where it's an intermediate. You have to use your own biological knowledge of the relationship between the exposure and disease to determine that.

BY MR. WOOL:

Q. What about when a substance is not a confounder, in that it has no association with the disease, is it proper to adjust for something that is not a confounder?

MR. LASKER: Objection to form.

A. So there are -- in epidemiology I would say age is the most frequently considered potential confounder. We often adjust for age in our analyses, even if age has no appreciable impact on the relative risk that we see, just because it's known to be an important

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confounder.

BY MR. WOOL:

Q. So I'm asking a slightly different question. What I'm curious about is something that you can definitively say is not a confounder. Let's say that there are 1,000 great cohort prospective studies that show that smoking just doesn't have any effect one way or the other on non-Hodgkin's lymphoma, if that were the case would it be proper to adjust for cigarette smoking as an example?

A. I think it's, you know, rare that we ever feel confident enough that smoking wouldn't cause an outcome that you wouldn't at least try to look at it as a potential confounder.

Q. Okay. Smoking was probably a bad example. Let's say Smart Water, for example. If there were a bunch of studies that just said that Smart Water has no effect one way or the other on non-Hodgkin's lymphoma, would it be proper to adjust for the use of Smart Water in an epidemiological study?

A. I think, again, you would only include it if you thought that it could be a common cause of exposure and your outcome. If you

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didn't have a reason to believe that that was true, then no, it would be fine not to include it.

Q. And if you could definitively rule it out, it would be fine not to include it as a confounder?

A. If you could definitively rule it out, yes. But I think, again, there are few situations where we feel comfortable doing that. I think erring on the side of being conservative and adjusting is usually how we would proceed.

MR. WOOL: Do you guys want to take a quick break?

MR. LASKER: Sure.

THE VIDEOGRAPHER: Going off the record. The time is 3:34.

(Whereupon, a recess was taken.)

THE VIDEOGRAPHER: Back on the record. The time is 3:45.

BY MR. WOOL:

Q. Dr. Rider, 37 percent of the Andreotti, et al cohort was lost to follow-up, correct?

MR. LASKER: Objection to form.

A. That's not how I would characterize

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it, because, again, they did have information on outcomes, so they weren't completely lost to follow-up. But it is true that 37 percent didn't respond to the follow-up questionnaire.
BY MR. WOOL:
Q. Right.
You mentioned imputation. And to deal with that percentage of people who did not answer the follow-up questionnaire, the authors performed an imputation, correct?
A. Yes, multiple imputation.
MR. WOOL: I'm going to mark as Exhibit 33-6 the Heltshe article.
(Whereupon, Exhibit Number 33-6, Heltshe, et al article, Using multiple imputation to assign pesticide use for non-responders in the follow-up questionnaire in the Agricultural Health Study, was marked for identification.)
BY MR. WOOL:
Q. You've seen this article before?
A. Yes, I have.
Q. Does this article describe the multiple imputation that the authors performed?

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A. Yes, it does. It goes through the imputation method, and then evaluates it for a number of different pesticides.
Q. Now, if you turn to Page 410, which is the second page of the article, under Materials and Methods the authors state that, "Our specific multiple imputation" --
MR. LASKER: Where are you?
MR. WOOL: Sorry, under Materials and Methods, I believe the third full sentence.
MR. LASKER: Okay.
MR. WOOL: Do you see it?
MR. LASKER: Yes.
BY MR. WOOL:
Q. The authors state, "Our specific multiple imputation procedure imputes four primary AHS exposure metric variables of interest," and then a colon, "(1) use (yes/no) of any pesticide in the interim period between Phase 1 and 2; (2) use (yes/no) of 50 specific pesticides in the interim period," which is referenced in Table 1. "(3) number of days of use for a specific pesticide during Phase 2; and (4) last year of application of any pesticides within the 5-year period between Phase 1 and 2,"

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correct?
A. Correct.
Q. And so am I correct that the imputation performed by the Andreotti, et al authors derived these four metrics that are listed here?
MR. LASKER: Objection to form.
What do you mean by "the Andreotti authors"?
MR. WOOL: Strike that. I agree that was a confusing question.
BY MR. WOOL:
Q. So the imputation was used to discern these four metrics that are listed here, correct?
A. That is correct. They were looking at four -- they used models to predict these four different outcomes. They were exposures, but they were the outcomes of the imputation models, correct.
Q. How was number of days of use for a specific pesticide during Phase 2 calculated?
A. The number of days of use of any pesticide?
Q. Yes. Strike that. I should have

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clarified.
So for the -- in the original study for the responders, how did they calculate that figure, the number of days of use for a specific pesticide?
MR. LASKER: Objection to form.
I'm sorry, which study?
BY MR. WOOL:
Q. The Andreotti study.
A. Could I ask you --
Q. Let me clarify.
In the Andreotti study, how did they calculate the number of days of use for a specific pesticide between the original questionnaire and follow-up questionnaire?
MR. LASKER: Objection to form.
A. I apologize, I still don't understand the question. I don't know whether you're talking about responders or non-responders.
BY MR. WOOL:
Q. Okay. We're talking about responders here. So let's go back to the Andreotti study, actually, at the top of Page 2.
A. Okay.
Q. Are you there?

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A. Yep.
Q. Okay. So looking at the top right-hand column --
MR. LASKER: I'm sorry, right hand on Page 2?
A. Of the Andreotti study.
MR. WOOL: Of Andreotti.
MR. LASKER: Sorry.
BY MR. WOOL:
Q. This will make it easier.
MR. LASKER: What column are we under?
MR. WOOL: The right.
BY MR. WOOL:
Q. And it states that, "At enrollment applicators reported a number of years and days per year each pesticide was used, while at follow-up applicators reported the number of days each pesticide was used in the most recent year farmed." Correct?
A. That's correct.
Q. So am I correct that the authors used the most recent year farmed as one of the metrics to determine the -- what did they call it -- the intensity weighted lifetime days of use?

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MR. LASKER: Objection to form.
A. So in the Andreotti, et al article, their primary analysis used information from the baseline questionnaire where they had asked about the number of years and days per year of use for each pesticide, as well as information from the follow-up questionnaire where they asked just about pesticide use in the most recent year farmed.
BY MR. WOOL:
Q. Okay. And the number of days of use for a specific pesticide during Phase 1 and 2 for responders was calculated using the number reported for the most recent year farmed, correct?
MR. LASKER: Objection to form.
A. So the, like I said, the authors used all of the information that they gathered on exposure in terms of number of years of use and days of use for each pesticide from enrollment, and then they distributed a follow-up questionnaire roughly five years later, and that questionnaire included questions about the number of days of use for each pesticide within the most recent year farmed.

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BY MR. WOOL:
Q. Okay. And to clarify, so between enrollment and follow-up, that figure was the only metric that was gathered for days per year of use, that answer for the most previous calendar year, correct?
A. Right. So the authors state that there's an approximately five year period between enrollment and the follow-up questionnaire, and at the time of the follow-up questionnaire the questionnaire included questions just on the most recent year farmed, not on every year, that is correct.
Q. Okay. Now you can go back to Heltshe.
A. Okay.
Q. Sorry, that was more complicated than it should have been. And again, we're on Page 410.
A. Okay.
Q. So if we go back to where we were, number 3, the number of days of use for a specific pesticide during Phase 2 refers to -- actually, strike that.
Okay. So Heltshe, kind of at a 10,000-foot level, used the information of

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responders to impute what non-responders would have answered had they responded to the questionnaires, correct?
MR. LASKER: Objection to form.
A. The goal of the imputation procedure is to be able to use data from the whole cohort even if participants had not responded to the follow-up questionnaire, so they're using this method to predict what a person's exposure would have been at that particular time period.
BY MR. WOOL:
Q. And so if responders' use data was inaccurate, then that would decrease the reliability of the imputed results, correct?
MR. LASKER: Objection to form.
A. So the information that's used in the imputation it takes into account, of course, the responders' data, but all of -- but also all of the other variables and information that they have for the entire cohort. So they're using every variable that could predict exposure to predict particular exposure values.
BY MR. WOOL:
Q. Is it possible for statistical analysis to correct for exposure

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misclassification?
MR. LASKER: Objection to form.
A. There are a number of methods actually that do corrections for exposure misclassification, yes.
BY MR. WOOL:
Q. Now, if the imputation model was systematically biased to imputing no exposure, would that diminish the power of the Andreotti study?
MR. LASKER: Objection to form.
A. Would you mind just restating the question one more time?
BY MR. WOOL:
Q. Yes.
If the imputation model was systematically biased to imputing no exposure, would that reduce the power of the Andreotti study?
MR. LASKER: Same objection.
A. So I think, first of all, I just want to point that that their primary analysis wasn't ever exposure versus no exposure, it was levels of exposure compared to no exposure. So I guess I'm not sure how there would be a systematic

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bias towards imputing no exposure.
BY MR. WOOL:
Q. Now, have you used imputation in any of your epidemiological publications?
A. I do not -- well, I can't say definitively that any of my papers does not include imputation, but at the same time I can't come up with an example right now that does.
Q. So let's see. You might have used multiple imputation, you might not have, you just don't know sitting here?
A. Yes, there's a possibility on a paper for which I'm a co-author that multiple imputation was used when there was missing data.
Q. Okay. So is it your opinion that the imputation method utilized in the Andreotti study has general acceptance within the epidemiological community?
A. Well, I think that it's well-suited for certain situations when the data are missing at random. So I think when you're deciding which method for strategy for handling missing data you're going to use, you consider why the data are missing. And when they're missing at random, multiple imputation is preferable to

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other approaches like, say, the complete case analysis.
Q. And in Andreotti the imputation was performed using answers from the entire cohort that answered Phase 2 questionnaires corrected, and it -- strike that. Let me just ask that first.
MR. LASKER: Objection to form.
A. So could you just ask the question?
BY MR. WOOL:
Q. Yes.
So I guess what I'm getting at is that the imputation that was performed did not look at whether the non-responders were from North Carolina or from Iowa, correct?
MR. LASKER: Objection to form.
A. I completely disagree. The imputation procedure takes into account all types of information that's available on all of the applicators, so that's how they build their imputation model. And that's even described in Heltshe.
BY MR. WOOL:
Q. Okay. So help me out here, I guess. So the authors did take into account whether the

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responder was in -- strike that.
The authors did take into account whether the non-responder was in North Carolina or Iowa?
A. I mean, I would need to look at the Heltshe paper to recall exactly what variables they ended up including in their imputation, but they used a strategy where all of the variables that most strongly predicted response were included. So again, I would have to review to see whether state was one of those. But if it wasn't included, it was because it didn't affect response -- I mean, it didn't affect the exposure value. Sorry.
Q. Okay. Now, if you turn to Page 413 of Heltshe, and to Table 3.
A. Okay.
Q. And Table 3 provides metrics for reference Brier scores, Brier score, and skill Brier score, correct? Table 3 on 414.
A. Here we are. Thanks. Yes.
Q. Okay. Did you hear my question?
A. That this table contains scores for reference Brier, Brier score, Brier skill score.
Q. Now, what is a Brier score?

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A. A Brier score is just a statistic that is used to measure the accuracy of a prediction that's in discrete categories.
Q. Have you ever used a Brier score in your own research?
A. No. I, in fact, I think they're fairly uncommonly used in epidemiology overall. Sorry. I mean, I think they're often used in weather forecasting.
MR. LASKER: Just for clarification, did you say that Brier score is used to measure the accuracy of a prediction that's in those three categories?
A. No, in discrete categories.
MR. WOOL: Good catch.
BY MR. WOOL:
Q. And so based on that answer, it would be fair to say that you have not calculated a Brier score before?
A. I have never personally calculated a Brier score, correct.
Q. But you had heard the term Brier score before reading the Heltshe paper, correct?
A. I actually had not encountered Brier scores before.

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Q. So this paper was the first time that you had encountered them as an epidemiologist?
A. That is correct. Like I said, I think that I've never seen one in all of the papers that I've reviewed, so I had to do some reading on them.
Q. What is the cutoff point at which you believe a Brier score indicates accuracy that would make the imputation methodology reliable?
MR. LASKER: Objection to form.
A. There is no such cutpoint that exists. And, in fact, even for statistics that we use very commonly in epidemiology, like sensitivity and specificity, there's no cutpoint at which you would say this is a good value or this is a bad value, because it very much relates to what you're trying to predict.
BY MR. WOOL:
Q. So whether a Brier score indicates that accuracy is unreliable -- strike that.
Within the field of epidemiology, is there any sort of general consensus as to what an acceptable Brier score is before, say, accuracy is deemed unreliable?
MR. LASKER: Objection to form.

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A. I wouldn't be aware of that, but it would surprise me because, like I said, we tend to not utilize cutpoints like that because it's very situation-specific.
BY MR. WOOL:
Q. So it would depend on the situation, correct?
A. Yes, that's what -- I'm saying for different measures that I am familiar with, like sensitivity and specificity, you judge those measures in context.
Q. So how would you go about evaluating an opinion that a Brier score was, I guess, you know, too low to be deemed reliable? I guess, if I wanted to -- you know, if I was looking at a paper and I saw a Brier score, I guess what I'm trying to get at is how I would go about evaluating, oh, this Brier score is way out there or, you know, or is within a range that would be considered acceptable?
MR. LASKER: Objection to form.
A. So like I said, I'm not -- I don't use Brier scores, I hadn't been familiar with them, so I think there are a number of other results provided in this paper that are actually much

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more useful to me than the Brier score, which you can see corresponds very tightly with the prevalence of the particular pesticide, which could be problematic.
So, you know, if you go to the next page, for instance, they talk about the relative errors, and that to me has more meaning than the Brier score.
BY MR. WOOL:
Q. Okay. But generally am I correct that the smaller the Brier score, the more accurate the prediction?
MR. LASKER: Objection to form.
A. That is how a Brier score is calculated. 0 would be perfect prediction.
BY MR. WOOL:
Q. And how does a Brier skill score, how is that different than just a Brier score?
A. So they're comparing the Brier score to some naive reference prediction. In this particular case they used the prevalence of pesticide use in the 80 percent of the cohort, you know, without the cohort that they held out to do the imputation. So they're basically subtracting the reference Brier from the Brier

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score to get the Brier skill score. So in the skill score, you know, those could range between negative 1 and 1, I believe.

Q. Now, if you turn the page to Page 414, I believe you said that that Figure 2 was more important to you in determining the accuracy of the imputation. Did I hear that correctly?

A. Well, I just -- I mean, this has information that's more meaningful to me because, again, the relative errors of the imputed prevalence, you know, tell you something about the error, taking into account how common that particular pesticide is in the cohort.

Q. Okay. Sorry, did I interrupt you?

A. No. Thank you.

Q. What is Figure 2 telling us with respect to glyphosate?

A. It's about in the middle of the pack.

Q. And what does that mean?

A. That there are pesticides with much higher relative error than glyphosate. And it's also interesting that this relative error doesn't relate at all to the Brier scores in Table 2. So you can find examples of pesticides with very high relative errors like

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methylbromide, but that have the lowest of the Brier scores.

Q. And what does a negative relative error indicate to you?

A. Well, it's just that it's underreporting the prevalence.

Q. Right.

Now, still on Page 414, if you look at the right-hand column, the first full paragraph, the authors state, "A key assumption of any imputation is that missingness is independent of the unobserved outcome of interest or unobservable confounders."

Do you agree with that statement?

A. Yes, I do.

Q. They go on to say, "The reduction of bias and increase in precision from multiple imputations is dependent on the covariates associated with both non-response and the endpoint variable, and factors associated with non-participation which were included and are in our imputation model."

Do you agree with that statement?

A. Yes.

Q. And they go on to say, "For our

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imputation analysis, the outcome of interest is the missing pesticide use itself. Montgomery, et al showed there's little evidence for selection bias in Phase 2 of the AHS, however missing at random is an untestable assumption without additional data, thus it is possible that non-responders differ from responders in variables we have not measured."

Do you agree with that statement?

A. I mean, this is always the case in epidemiologic studies. The mechanism of missingness is always untestable, but you're in a much better position in a cohort study like this where they have measured many, many variables that could be used to predict exposure levels.

Q. So it would be fair to say that you do agree with that statement?

MR. LASKER: Objection to form. Asked and answered.

A. So do I agree -- do I agree with the statement that it's an untestable assumption without additional data?

BY MR. WOOL:

Q. Yes.

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A. Yes, it is almost always the case that in an actual epidemiologic study, not some sort of simulation, that that is an untestable assumption.

Q. What is selection bias?

A. I believe I've already answered this question earlier, but --

Q. I'm sorry. Go ahead.

A. I can answer it again.

Q. If you don't mind.

A. So if you're thinking about it structurally, it's when you're conditioning on a factor that is an effect of both the exposure and the outcome.

Q. Do you believe that maintaining a high rate of follow-up in a cohort study is integral to ensuring validity?

MR. LASKER: Objection to form.

A. That is a very general question. I think that if you're talking about outcome data, that's one particular issue. So, as I mentioned before, the AHS is in a good position because they obtained outcome data on all or virtually all of the participants in terms of NHL through cancer registries, so in that place, and, you

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know, I think that that is an integral aspect of the validity of the study.
BY MR. WOOL:
Q. Is there any agreement within the field of epidemiology as to what constitutes a high rate of follow-up?
MR. LASKER: Objection to form.
A. No, because, again, I think it really depends on the particular situation in the study, and how long you're following people, and is this a chronic disease that you're looking at or some short-term outcome. So it's very hard to provide a number of what's acceptable.
BY MR. WOOL:
Q. Would it be reasonable for an epidemiologist to put less weight on a study due to a 37 percent loss in follow-up?
MR. LASKER: Objection to form.
Are you talking about in general, or in the Andreotti study?
MR. WOOL: In general.
A. I mean, I think if I was reviewing a study and there was 37 percent of, again, not lost to follow-up of the whole cohort, but we're talking about missing follow-up data, I would

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want to know that they thought carefully about how they were going to handle that missing data in their analysis, and that that was -- and that the assumptions that they were making about why that data were missing seemed appropriate.
BY MR. WOOL:
Q. With respect to the Andreotti study, would it be reasonable for an epidemiologist to put less weight on that study due to the lost to follow-up?
MR. LASKER: Objection to form.
A. So I think I already answered this. I can't speak for epidemiologists in general, but I know that my own approach would be to try to determine if the ways that they handled that missing data was appropriate given why the data were missing.
BY MR. WOOL:
Q. Well, say, for example, with the Andreotti study, if they got complete responses from every participant, would the study be more powerful in your mind?
MR. LASKER: Objection to form.
A. I mean, it's hard to speculate because that's not the case. And I think that, you

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know, the sensitivity analyses that they did show us that regardless of how they handle that missing data, they're really coming up with the same conclusions. They get very, very similar results. And so in that way it seems like the missing data isn't having a large impact on the findings, so in that way I think it would be inappropriate not to consider this study.
BY MR. WOOL:
Q. So I'm not talking about whether to consider it or not. I'm talking about the amount of weight that you would afford to the study. Do you understand the distinction?
A. So I mean, I know how I would weight it in terms of all of these studies that currently exist on GBH use and NHL. I think that even with 37 percent missing data on a follow-up questionnaire, you know, incomplete data, in essence, on the enrollment questionnaire, and follow-up for decades, and much more information on co-variates that can be adjusted for as confounders, and not having concerns about the impact of recall bias or selection bias from improper selection of controls, I think all of those things lead me to

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weight this study, to rank it highest among all of the data that's currently available.
Q. Have you ever published a study where 37 percent of the population was lost to follow-up?
A. So again, you know, you keep saying lost to follow-up, and that's not really how I would characterize it here, because they're just missing data on exposure on one questionnaire. They're not lost because we have their outcome data, so that's not how I would describe it.
So I think in cohorts that I've worked on where they have done repeated exposure measurements in questionnaires, you know, over, say, four year intervals in the health professionals follow-up study it is not at all uncommon for a given exposure to be missing on, you know, a third of the cohort for a given survey cycle, so I can imagine that it wouldn't be that difficult for me to find an example where that was the case on a study that I'd worked on.
Q. Fair enough.
Have you done any research on changing use patterns of glyphosate following the advent

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of Roundup Ready crops?
MR. LASKER: Objection to form.
A. So you asked if I had done any research on it?
BY MR. WOOL:
Q. Yes. I don't think that -- I could be mistaken, but I don't think that your expert reports contains the Benbrook article.
A. I don't believe I cited the Benbrook article, but I have read that article.
Q. You have read the article?
A. Yes.
Q. Okay. So fair to ask you some questions about it?
A. Sure.
Q. Thanks.
MR. WOOL: I'm going to mark the Benbrook article as Exhibit 33-7.
(Whereupon, Exhibit Number 33-7, Benbrook article, Trends in glyphosate herbicide use in the United States and globally, was marked for identification.)
MR. LASKER: You gave me two.
MR. WOOL: Christmas came early.

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MR. LASKER: 33-7?
MR. WOOL: Yes.
BY MR. WOOL:
Q. And you have reviewed this article?
A. I have, yes.
Q. All right. Now, if you look at the abstract box, the second sentence, the authors note that, "Globally, glyphosate use has risen almost 15-fold since so-called 'Roundup Ready,' genetically engineered glyphosate" --
MR. LASKER: Where are you reading?
MR. WOOL: The second sentence of Results.
BY MR. WOOL:
Q. I'll start from the beginning.
"Globally, glyphosate use has risen almost 15-fold since so-called 'Roundup Ready" genetically engineered glyphosate-tolerant crops were introduced in 1996. Two-thirds of the total volume of glyphosate applied in the US from 1974 to 2014 has been sprayed in just the last 10 years."
Did I read that correctly?
A. Yes.
Q. So meaning that approximately

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two-thirds of the total amount of glyphosate sprayed in the United States occurred between 1974 and 2014, is that what that sentence means?
A. Yes, I think so.
Q. All right. And if I'm not mistaken, 2005 was the last year of follow-up in the Andreotti study, correct?
A. For -- well, it was the last time that they collected data from the follow-up questionnaire on exposure.
Q. So 2004 was the last year that follow-up data from the questionnaire was collected in Andreotti?
A. You said 2005, and then 2004. So the follow-up questionnaire period occurred between '99 to 2005.
Q. So I think here's where the confusion lies. The question -- or the responses in 2005 dealt with the 2004 calendar year, correct?
MR. LASKER: Objection to form.
A. Yes, I believe that that's correct.
BY MR. WOOL:
Q. All right. So if you will turn with me to Page 15 of the Benbrook article.
MR. LASKER: 15?

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BY MR. WOOL:
Q. Sorry, 515.
A. Okay.
Q. And I'll ask you to look at Table 1.
A. Mm-hmm.
Q. Okay. So in -- and they give figures in thousand kilograms, and then in thousands of pounds.
A. Mm-hmm.
Q. Since we're in the United States, I'll ask you about the pounds.
A. Okay.
Q. Now, for glyphosate agricultural use, in 1990 the authors report 7,400 thousand pounds of glyphosate use?
A. Correct, 7,400 in 1990 pounds, yes, correct.
Q. And then 27,500 in 1995?
A. Correct.
Q. And then 78,750 in 2000?
A. Mm-hmm.
Q. And then 157,500 in 2005?
A. Yes.
Q. And then the figure goes up to 235,814 in 2010?

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A. Mm-hmm.
Q. Correct?
And then it looks like it sort of levels off a little bit, it goes up to 236,318 in 2012, correct?
A. Correct.
Q. And then it goes to 249,906 in 2014?
A. That is what it says, yes.
Q. And the Andreotti study -- strike that.
So would it be fair to say that use of glyphosate changed pretty dramatically over the years that the Andreotti study was collecting follow-up data?
MR. LASKER: Objection to form.
A. So here in this table they report glyphosate use in pounds, and you can see that there is an increase between 2000 and 2005, which would be the follow-up questionnaire period.
But what's interesting about this article is that it shows how that's really related to the availability of Roundup Ready crops, and in many ways you can predict that increased use if you know what farmers are

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farming.
BY MR. WOOL:
Q. So do you recall if the follow-up questionnaire for Andreotti asked participants about the specific crops they were farming?
A. Well, that is available in the publication, another HS publication, so they did ask for that information. It was in, I think, Alavanja 2006 they describe what types of crops the farmers farm.
Q. So if a cohort member had reported no use of glyphosate at enrollment, but then began using Roundup Ready crops, say, in 2004, if they responded in 1999 to the questionnaire, the questionnaire would not capture their glyphosate use, correct?
MR. LASKER: Objection to form.
A. So just one clarification. In the Andreotti JNCI paper they talked about how the follow-up questionnaire is approximately five years after enrollment for the participant, so we don't really have a reason to believe that someone who answered on the earlier end of the enrollment questionnaire would then, you know, answer more than five years later on the

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follow-up questionnaire.
And that situation you described, yes, certainly in some cases that could have happened, but that would have a relatively small effect on, you know, the total impact of exposure measurement in the entire cohort.
So yes, for some people it will not be imperfect -- it will be imperfectly captured, but it's unlikely to have a meaningful impact on the results given how much information they did collect.
BY MR. WOOL:
Q. Well, using the results of Andreotti, is it possible to measure the frequency that that particular scenario would have played out?
A. I don't think it's possible directly in the Andreotti paper, but I think there are a number of pieces of information in this Benbrook article that are actually really helpful in terms of the increase in use between that period that you're talking about. It's actually -- it doesn't really increase that much in that period.
Q. So what pieces of information, I guess, would you point me to to establish that?

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A. So, for instance, if you look at figure 1.
Q. In what?
A. In the Benbrook paper. So this is specifically for soybeans. We know that in Iowa 80, percent of the farmers are soybean farmers. So if you look in that period, what we're using between, you know, '90 --
Q. During the follow-up period is what I was asking about.
A. Yes, so during '99 and 2005, you know, you can see how much use increased during that period. So a lot of the increases would have been -- would have been captured in that interim period.
Q. Do you know if any of the other pesticides and herbicides that were surveyed in the Andreotti study, if any of those had increases in use that compared to glyphosate?
A. Could you ask the question one more time?
Q. Yes. I don't think I phrased it very well.
Did any other pesticide or herbicide surveyed in Andreotti, et al increase -- or did

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the use of that pesticide or herbicide increase to the extent that glyphosate did during the follow-up period?

MR. LASKER: Objection to form.

A. Again, I'm not an expert on the use of -- glyphosate use patterns, but from reading of this -- of the Benbrook article, and also there was another paper I'm not recalling now on trends, I think we know that because of Roundup Ready crops, glyphosate has increased more dramatically.

But in some ways when you're trying to predict people's patterns of exposure, the availability of Roundup Ready crops is really helpful, because then those patterns of exposure become determined by farming those particular crops, and there's are much more specific regimen for application. So that information can actually be helpful.

BY MR. WOOL:

Q. So I guess due to the increase in use of glyphosate, would it be fair to expect fewer never responses in, say, 2000 as opposed to earlier in the follow-up period?

MR. LASKER: Objection to form.

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BY MR. WOOL:

Q. Do you think that, say, people who at enrollment had never used glyphosate, that as we continue from 1997 to 2005 that the frequency of never use responses is likely to decrease?

MR. LASKER: Objection to form.

A. I mean, we know that glyphosate use increased, particularly among certain types of farmers, right. So you can see in this paper that, you know, it's really due to three crops. Right? So there were definitely increases in glyphosate use. Whether that resulted in fewer never users or just a greater degree of use, I don't really know. But again, you can predict those patterns to a large degree by just knowing what products -- what people are farming.

BY MR. WOOL:

Q. All right. Let me just ask a couple more questions, and then I think we can wrap things up. I know that I've asked this at the beginning, so my apologies if you've answered.

MR. LASKER: Objection. Asked and answered. Sorry.

MR. WOOL: I knew it was coming.

BY MR. WOOL:

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Q. Did you have a chance to review any of the plaintiffs' deposition transcripts before today?

A. For the most recent depositions?

Q. Yes, for depositions conducted pursuant to Pretrial Order 34.

A. Yes, I have.

Q. Okay. And which experts?

A. I have reviewed Dr. Neugut's deposition, Dr. Ritz, and Dr. Portier's deposition.

MR. WOOL: Okay. Let me take a quick break. I'll chat with Jeff, and maybe we can wrap this up.

THE VIDEOGRAPHER: Going off the record. The time is 4:27.

(Whereupon, a recess was taken.)

THE VIDEOGRAPHER: Back on the record. The time is 4:34.

BY MR. WOOL:

Q. Now, we spoke about this briefly, but one of the reasons for which you believe that misclassification could not account for the -- or exposure, non-differential exposure misclassification could not account for the

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results in Andreotti is because the relative risks in all categories of the Andreotti study are below 1.0, correct?

MR. LASKER: Objection to form.

A. So I explained earlier that, you know, we usually say that on average -- that doesn't mean in every study, single study all the time, but on average non-differential exposure misclassification would bias the results to the null. That's true for ever-never -- dichotomous exposures when you just have yes versus no.

But when you start looking at exposure in more than two categories, you can have the situation where if you're just misclassifying between two categories, those relative risk estimates would be biased towards each other, so you could actually get, for one of those categories, bias away from the null.

So what I'm talking about in my report is how that particular situation can't explain -- can't be happening in the Andreotti, et al 2018 study because all of the relative risk estimates for all of the categories are below 1.

BY MR. WOOL:

Q. And one of the things that the authors

did in an attempt to minimize non-differential exposure misclassification was to perform some sensitivity analyses?

A. Yes. That's correct.

MR. LASKER: Objection to form.

BY MR. WOOL:

Q. And if you turn to Page 5 of your report.

A. Okay. All right.

Q. In the final paragraph on Page 5, you describe one of these sensitivity analysis that sort of truncated the results of 2005.

A. That is correct, that's one of the analyses they conducted.

Q. Okay. And why did they truncate the results of 2005?

A. Because the follow-up questionnaire period ended at 2005, and so ending follow-up at 2005 wouldn't make any assumptions about a person's exposure after 2005.

Q. Okay. And in this particular instance, you say that the risk ratio comparing the highest quartile of intensity weighted exposure to no exposure in analysis, the truncated follow-up in 2005 was 1.04, which is

also consistent with the primary analysis, correct?

A. Yes. I also report the 95 percent confidence interval around that estimate, which is from .7 to 1.57.

Q. Okay. And so when the analysis was truncated, that resulted in the risk ratio of the highest exposure group going up, correct?

MR. LASKER: Objection to form.

A. Well, the confidence intervals for this estimate and the estimate of the primary analysis and the results of the two other sensitivity analyses all overlap and they all contain the null value. And so, you know, I think I mentioned earlier on, you know, we don't dwell too closely on this point estimate because it is, you know, subject to random error. The confidence interval takes that into account.

So I would interpret this result of 1.04 the same way that I would interpret the result in the primary analysis of -- there's NHL, for the highest quartile to no exposure of 0.87 in exactly the same way, and that's being consistent with no association.

MR. WOOL: That's it. I don't have

any other questions.

A. Okay.

MR. LASKER: No questions.

THE VIDEOGRAPHER: This concludes the January 23, 2018 deposition of Dr. Jennifer Rider. Going off the record. The time is 4:39.

(Whereupon, the deposition was concluded.)

COMMONWEALTH OF MASSACHUSETTS)
SUFFOLK, SS.)

I, MAUREEN O'CONNOR POLLARD, RMR, CLR, and Notary Public in and for the Commonwealth of Massachusetts, do certify that on the 23rd day of January, 2018, at 2:39 o'clock, the person above-named was duly sworn to testify to the truth of their knowledge, and examined, and such examination reduced to typewriting under my direction, and is a true record of the testimony given by the witness. I further certify that I am neither attorney, related or employed by any of the parties to this action, and that I am not a relative or employee of any attorney employed by the parties hereto, or financially interested in the action.

In witness whereof, I have hereunto set my hand this 5th day of February, 2018.

_____

MAUREEN O'CONNOR POLLARD, NOTARY PUBLIC
Realtime Systems Administrator
CSR #149108

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ACKNOWLEDGMENT OF DEPONENT

I, _____, do Hereby certify that I have read the foregoing pages, and that the same is a correct transcription of the answers given by me to the questions therein propounded, except for the corrections or changes in form or substance, if any, noted in the attached Errata Sheet.

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JENNIFER R. RIDER, ScD DATE

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To before me this
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