APPENDIX A

Response to Mr. Albert Wilson and Industrial Wind Action Group Critiques

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Introduction

On November 20, 2009, the Industrial Wind Action Group (IWAG) posted an editorial that, in part, lists a number of concerns about Berkeley Lab's efforts to investigate the presence of residential property value impacts associated with U.S. wind power facilities.[2] That editorial follows from more-extensive review comments provided on September 11, 2009 by the Industrial Wind Action Group.[3] The more extensive comments were provided during the external review of the draft Berkeley Lab report, and were one of roughly 20 sets of external review comments received by stakeholders and experts at that time. All of these comments were considered during revisions to the draft report, culminating in the final analysis and report issued on December 2, 2009. More recently, Mr. Albert Wilson posted his critique of the report, which in many respects is similar to that of the IWAG postings.[4]

Though the final Berkeley Lab study largely speaks for itself, this memorandum offers a brief response to the specific comments enumerated in these critiques.[5] Before responding to the specific comments offered, however, one important observation should be made: the Berkeley Lab report does not offer definitive proof that wind projects, under all circumstances, will never impact residential property values. Therefore, as the IWAG correctly claims, the results of this work should not be summarized as such. Rather, the Berkeley Lab work, as discussed extensively in the final report, finds <u>no persuasive evidence</u> of any consistent, measurable, and statistically significant effect given the sample of home sales transactions evaluated. As noted on several occasions in the report, although the analysis cannot dismiss the possibility that

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[1] This memo is a revised version of the "Response to Industrial Wind Action Group Critiques" issued on December 2, 2009.

[2] "False conclusions based on flawed real estate studies," http://www.windaction.org/faqs/24176.

[3] "Hedonic analysis of the impact of wind power projects on residential property values in the United States," http://www.windaction.org/documents/24178.

[4] "Wind Farms, Residential Property Values and Rubber Rulers," http://www.arwilson.com/pdf/newpdfs/WindFarmsResidentialPropertyValuesandRubberRulers.pdf .

[5] The IWAG editorial posted on November 20th makes a number of additional claims, suggesting that the authors of the study were predisposed to a preferred outcome, had no interest in conducting a legitimate study, and had no interest in releasing a final report. These claims are baseless, and are therefore not addressed here.

individual homes or small numbers of homes have been or could be negatively impacted, the extensive research finds that, if these impacts do exist, they are either too small and/or too infrequent to result in any statistically observable impact within the sample of nearly 7,500 home sales transactions evaluated.

Mr. Wilson and IWAG Comment: Regression analysis is not in accordance with the International Association of Assessing Officers' (IAAO) established methods

Both the IWAG and Mr. Wilson claim that the methods employed by the Berkeley Lab study are not in accordance with the established methods of the International Association of Assessing Officers' (IAAO).

This claim encompasses two issues, both of which will be addressed below: 1) The IAAO standards for estimating the selling prices of homes are not met by the analysis contained in the report, and, 2) that an hedonic analysis is not the appropriate model to use for evaluating effects environmental contamination on property values.

Response to the IAAO standards for estimating the selling prices of homes are not met by the analysis contained in the report: The methods of the IAAO are irrelevant for estimating a hedonic pricing model of the nature used in the Berkeley Lab report.

As noted in the final report, the Berkeley Lab research is not designed to assess, appraise or predict selling prices of properties (i.e., an "appraisal model" used to establish an estimate of the market value of a home at a specified point in time). Rather, the hedonic models as used in the report are designed to estimate the marginal contribution of individual house or community characteristics to sales prices. The distinction is that an appraisal model, as governed by the IAAO standards, requires a very accurate final prediction of price, because it is this "selling price" which is the purpose of the model. Alternatively, a hedonic model, as used in the LBNL report, is not particularly concerned if a final selling price is accurately predicted, but rather, that the individual contribution of one or more characteristics to that selling prices are accurately estimated.

Moreover, predicting selling prices accurately is inherently difficult because of the heterogeneous qualities of markets, buyers and sellers. For that reason, appraisal models often use relatively small localized data sets (i.e., "comps") pertaining to nearby properties that sold over a short period of time and a limited number of explanatory variables. On the other hand, a hedonic model relies upon large data sets, with a sizable number of explanatory variables, potentially occurring over a long time period. For these reasons, the datasets, resulting models are quite different and therefore standards for one type are not necessarily applicable to the other.

As an example, the specific statistic that both Mr. Wilson and the IWAG cite, as important to the IAAO standards, is the R^2. This statistic is a measure of "goodness of fit" or, in other words, how well the model estimates final selling prices. A model that perfectly predicts selling prices will have an R^2 of 1.00, indicating there is no difference between actual selling prices, and those predicted by the model. As discussed above, the ability of the model to predict final selling prices is somewhat irrelevant to the task intended in the LBNL report, but not so for appraisal or

assessing models. It follows that for IAAO standards, which govern the prediction of selling prices, an R^2 less than 0.90 to 0.95 is considered unacceptable. (It is for this reason that the data used for these models is often homogeneous (i.e., similar types of homes) with limited spatial and temporal diversity.) Alternatively, for hedonic models as thay are used in the report, an R^2 of 0.70 or lower is not considered unusual, because the data is intentionally heterogeneous with a wide spatial and temporal diversity. More important to the research is that the estimates for the marginal contribution to sale prices are accurate. Nonetheless, an R^2 is used in studies similar to that of the LBNL report to gauge appropriateness of the model variables. To that end the relatively high R^2 (~ 0.77) found in the Berkeley Lab analysis - as compared to other cross-sectional analyses - substantiates the appropriateness of the variables used.

Response to an hedonic analysis is not the appropriate model to use for evaluating environmental contamination: The hedonic method is not the only method to estimate the marginal impacts that environmental dis-amenities (and amenities) have on selling prices, but is most definitely an accepted one, if not the preferred method when adequate data is available. [6]

As discussed in Section 2.1 of the final report, the hedonic pricing method is well established and widely used in the economics and real estate literature for evaluating the marginal impacts of environmental amenities and disamenities on housing prices. An extensive literature that began with the seminal works of Rosen (1974) and Freeman (1979) has developed outlining it use. The Berkeley Lab report clearly documents the history and use of the hedonic pricing model, its appropriateness for exploring the possible impact of wind projects on property values, and how the multiple statistical models employed in the Berkeley Lab research relate to the broader economics and real estate literature. For instance, the literature is replete with analysis conducted using similar methods (see for example the reviews Kroll and Priestley 1992; Farber 1998; McCann 1999; Bateman et al. 2001; Boyle and Kiel 2001; Jackson 2001; Nelson 2004; Ready and Abdalla 2005; Simons and Saginor 2006; Simons 2006b; Leonard et al. 2008).

IWAG Comment: Study neglects to explain the risks of employing Hedonic analysis

The IWAG claims that the study neglects to explain the risks of employing hedonic analysis, that causal conclusions drawn about a dataset when utilizing hedonic analysis are often unsupportable, and that the literature is highly critical or even dismissive of the hedonic method. Further, the IWAG notes that, "a major limitation of observational data is that they often do not provide adequate information about cause-and-effect relationships" (i.e., correlation does not necessarily imply causation).

Response: The final report offers a review of the hedonic literature, and provides a number of citations (some noted above) to which a reader can go for a more extensive review of the history and use of this method. As discussed in the report, though all methods have limitations, the hedonic pricing method is well established. The literature is neither "highly critical" nor "dismissive" of the method; if anything, the opposite is true (see footnote 6 below). Moreover,

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[6] For example, see the two articles by Thomas Jackson, MAI, who was former member (2001 and 2002) of the six-person Uniform Standards of Professional Appraisal Practice (USPAP) board, "Methods and Techniques for Contaminated Property Valuation" (2003) and "Evaluating Environmental Stigma with Multiple Regression Analysis" (2005). Hedonic pricing models are a form of "Multiple regression analysis".

as discussed in the Berkeley Lab report, there is an extensive literature that has steadily improved the method, and the method is regularly used by both economics and real estate experts to evaluate the marginal impacts of environmental amenities and disamenities on housing prices. The hedonic method is the most appropriate approach to evaluate the question at hand: whether wind energy facilities have any demonstrable and widespread effect on home prices.

Moreover, the study employs not one, but eight different hedonic models, as well as both repeat sales and sales volume models, all of which provide tests for the robustness of the results. The consistency of the results of these various analyses provides confidence in the final results discussed in the report and, in combination with the extensive data collection effort, produces the most comprehensive and data-rich analysis to date in the U.S. or abroad on the possible impacts of wind projects on property values.

The IWAG is correct that hedonic analysis focuses on correlations, and that correlation does not necessarily imply causation. At the same time, the Berkeley Lab analysis finds no correlation between wind facilities and home sales prices. Because of this finding, and because of the care taken by Berkeley Lab in the measurement of the variables of interest, the difference between correlation and causation is moot: with no correlation there can be no causation.

IWAG Comment: Background review of other studies [was not thorough]

The IWAG notes that much of the previous work that has investigated the potential impact of wind projects on property values has limitations, rendering the results of some of this literature misleading or invalid.

Response: The Berkeley Lab report authors agree that there are a number of limitations to the previous work, a point made clearly in Section 2.2 of the final report. Specifically, a large number of the previous studies investigating property value effects surrounding wind facilities have not been peer reviewed, and suffer from a variety of substantive limitations (e.g., lack of reliance on market data, small sample sizes, overly simplistic statistical techniques, and unreported statistical significance). As discussed extensively in the report, the methods applied by Berkeley Lab were specifically intended to overcome many of the limitations of this previous literature. As a result, the Berkeley Lab research is the most reliable, comprehensive, and data-rich analysis to date on the possible impacts of wind projects on property values.

Mr. Wilson and IWAG Comment: No clear evidence the data used was checked for accuracy

The IWAG argues that there is no evidence that the data used in the model were checked for accuracy, and that non-valid sales transactions (i.e., "dirty sales") might have been included in the final data set. Similarly, Mr. Wilson argues that the results can be adversely affected by outliers and influential observations.

Response: As noted in the final report in Section 3.2.1, only "valid" sales are included in the dataset; as discussed, the validity of those transactions is determined as follows:

"Validity was determined by each individual county data provider. A sale that is considered "valid" for county purposes would normally meet the minimum requirements of being arm's length; being a transfer of all rights and warrants associated with the real estate; containing an insignificant amount of personal property so as not to affect the price; demonstrating that neither party in the sale acting under duress or coercion; not being the result of a liquidation of assets or any other auction, a mortgage foreclosure, a tax sale, or a quit claim; and being appropriate for use in calculating the sales price to assessed value ratios that are reported to the state. Due to the formal requirements associated with this calculation, "validity" is often defined by a state's Department of Revenue..."

Though the study therefore relies, to some degree, on individual county-level data providers to help ensure the validity of the resulting sales data, it is highly unlikely that the many kinds of sales of concern to the IWAG are included in the final data set. Moreover, to provide greater certainty to that finding, the authors also excluded transactions that had certain characteristics that might place them in doubt (e.g., transactions that occurred within six months of a previous sale of the same home, and transactions that produced a statistical residual greater than six standard deviations from the mean of all residuals).[7] In addition (addressing Mr. Wilson's claim), tests were conducted to evaluate whether certain additional transactions that might be classified as outliers and/or influencers (i.e., dirty sales) might be inappropriately influencing the results. A thorough inspection of this group of outliers and/or influencers was conducted to help ensure that the dataset met the requirements for a hedonic model and that the results are not inappropriately influenced by suspect data. These procedures are documented clearly in the final report in Appendix G.

Mr. Wilson and IWAG Comment: No information in the study confirms whether the model was tested or calibrated using actual sales data

Both Mr. Wilson and the IWAG claim that, according to IAAO, when a model is specified an iterative process of calibrating the model using alternative data sets is necessary to test and fine tune the model's coefficients. The IWAG also notes that thousands of possible models can be applied in a given situation, and argues that the authors should explain what process was followed in the Berkeley Lab analysis.

Response: As stated above, the IAAO standards are not relevant for the hedonic pricing models used in the Berkeley Lab research: the research is not designed to appraise properties (i.e., predict selling prices) so calibration to actual sales data is not relevant.

The research does, however, follow typical research protocols for estimating and interpreting a hedonic price function. As noted clearly and repeatedly in the body of the report and in the appendices, a variety of hedonic models were tested, from which the final models were selected.

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[7] The rationale for these restrictions is provided in the full Berkeley Lab report. As noted in Section 3.2.1, these excluded transactions total 39, 32 of which occurred following construction, two were for homes that had a view of the turbines (both minor), and one was for a home located inside of one mile. Although the sale that involved a home located inside of one mile was removed, a number of other homes from the same neighborhood, also inside of one mile, were included in the final dataset.

The process of selecting the final eight hedonic models is discussed throughout the document, and the results of alternative model specifications are discussed in a number of footnotes and in the appendices. The performance of the final models are reported (e.g., adjusted R^2 and other statistics), and are consistent with hedonic analyses conducted by others. Moreover, the results are benchmarked to the broader hedonic literature as discussed in the following passage from Section 4.3:

> *"To benchmark the results against those of other practitioners the research by Sirmans et al. (2005a; 2005b) was consulted. They conducted a meta-analysis of 64 hedonic studies carried out in multiple locations in the U.S. during multiple time periods, and investigated the coefficients of ten commonly used characteristics, seven of which were included in the model. The similarities between their mean coefficients (i.e., the average across all 64 studies) and those estimated in the present Base Model are striking."*

The report then compares each coefficient in the base hedonic model to those in Sirmans et al. and finds, in conclusion,

> *"As a group, the Base Model estimates differ from Sirmans et al. estimates in all cases by no more than a third of the Sirmans et al. mean estimate's standard deviation. This, taken with the relatively high adjusted R^2 of the…model [0.77], demonstrates the appropriateness of the model's specification."*

Mr. Wilson Comment: The estimated hedonic coefficients must be tested to determine if they accurately and only represent the explanatory variables and they represent an economic impact

Mr. Wilson claims that the coefficients estimated in the model must be tested to determine if they accurately and only represent the explanatory variables (i.e., are they "biased") and that even if they are not statistically significant that they might represent and "economically" significant effect.

Response: This claim essentially puts forward that the authors should consider effects worth discussing the importance of, even if they are not statistically identifiable (i.e., less than the margin of error). There is merit in this claim, in that the numbers of data can influence the statistical significance of a variable, even if the estimated effect is accurate. Therefore, for purposes of this discussion the variables of interest can be broken into two groups, those that were identified as statistically significant and those that were not.

Statistically significant variables: All statistically significant variables were discussed at length in the report. For variables to be considered a valid measurement of an effect, they needed to be significant across many models, and, moreover, be intuitive. For example, a variable indicating a positive effect on selling price despite the assumption of a negative effect, or one that contradicts the effects found in the same model by other similar variables, would not be given much weight in the discussion.

Statistically insignificant variables: In all cases a statistically insignificant variable is smaller than the margin of error. It is possible that the margins of error, though, are so large, as to obscure a potentially "economically" significant effect. That notwithstanding, putting much weight on a variable of this type is risky for the practitioner, for statistically speaking, the analysis cannot determine reliably if the effect is above or below zero - a difference that would also affect the determination as to whether it is "economically" significant. Nonetheless, the report makes every effort to flush out these potentialities. For instance, when non-statistically significant effects were found to be near -5% for homes within one mile and that sold after the wind facilities were constructed, extensive analysis was conducted to determine if those effects should be considered accurate. The report conducted no less than 4 alternative models investigating this issue, only to find that effects that pre-existed the wind facilities announcement and construction, likely were driving these adverse effects, and therefore less confidence could be placed in them.

Finally, as regards bias in the explanatory variables, extensive efforts were made to explore such biases, and offer alternative interpretations of results taking into account such biases. To that end, robustness tests are conducted for alternative explanatory variables, alternative samples, outliers/influential observations, pooling, etc. In all cases the results pertaining to the focus variables (i.e., the estimated coefficients on variables of interest) were robust.

Mr. Wilson and IWAG Comment: The data set is not homogeneous; data is drawn from across the country

Mr. Wilson and the IWAG claim that lack of homogeneity in the final data set is fundamentally problematic, and argues that a basic assumption of a regression analysis is that the data are reasonably homogenous (i.e., that the homes included in the dataset are reasonably similar in characteristics, amenities, etc.). The IWAG also argues that applying the same weight to property characteristics (e.g., fireplaces) across the entire nine-state region is inappropriate. Finally, the IWAG claims that the model does not allow one to understand how the age of the home impacts sales prices or, for that matter, square footage, number of baths, etc.

Response: Overall, Mr. Wilson and the IWAG concerns encompass three different themes: (1) the data are pooled from different study areas across the country, (2) individual home characteristics have a significant amount of variation (e.g., price of homes and the age of homes), and (3) the estimated coefficients are not allowed to vary across study areas but rather are estimated across the entire dataset. Each concern is addressed in turn.

- Data are pooled from different study areas across the country: As discussed in detail in the Appendix F, models specific to individual study areas were extensively tested and evaluated. These models, however, were found to be less parsimonious than the final models and exhibited divergent and spurious coefficients, as well as large standard errors, for the variables of interest, presumably because of the small number of home sales in each of the individual study areas near the wind turbines. As a result of this analysis, a pooled model is used. The details of this process and the rationale for selecting a pooled model are clearly documented in the final report. Moreover, allowing study area influences to be estimated at a micro-level, as discussed in Appendix F, does not impact the variables of interest.

- Individual variables have a significant amount of variation: Though the IWAG argues that homogeneity in the dataset is a prerequisite for a regression analysis, the very purpose of a hedonic model is to control for heterogeneity in the data to evaluate the marginal impact of varying house characteristics. In general, then, variation in housing characteristics within the data set is valuable as long as the variation in the independent variables explains the variation in the dependent variable, and there are no omitted variable biases. The relatively high adjusted R^2 (~ 0.77) found in the Berkeley Lab study - which is a cross-sectional property value analysis - substantiates the appropriateness of the data and model used. Further, as discussed in the report and above, coefficient estimates for a variety of property characteristics are consistent with those of other practitioners using similar methods. Finally, as discussed above and in Appendix G in the report, extensive testing regarding the impact of outliers and influential observations is conducted, ensuring that individual questionable sales transactions are not unduly influencing results of the study.

- The estimated coefficients are not allowed to vary across study areas: As addressed in the first bullet above (and in Appendix F in the report), alternative hedonic models were tested in which all variables were interacted with dummy variables for the individual study areas; in these models, the value of a fireplace in one study area, for example, is allowed to differ from the value in other study areas. Appendix F clearly reports how the final models were selected from multiple alternative specifications. Importantly, the focus variables, namely the effect of proximity and views of wind facilities, are robust to the inclusion/exclusion of these interactions. As such, including these interactions in the model does not impact the results of the Berkeley Lab analysis.

With respect to understanding how the age of the home impacts sales prices or, for that matter, square footage, number of baths, etc., this information is clearly provided in the regression results presented in Section 4.2 (for the base model) and in Appendix H (for the other models).

Mr. Wilson and IWAG Comment: The data set omits property characteristics

Both Mr. Wilson and the IWAG claim that a variety of important property characteristics were omitted from the analysis, noting specifically the omission of the number of bedrooms. If, as Mr. Wilson correctly contends, variables omitted from the model are inappropriately influencing results, one is likely to see coefficients that represent the combined effects of the focus variable and the omitted variable.

Response: The protocols for estimating a hedonic price function, as discussed in Appendix G, are clear: including too many independent variables that measure the same basic thing (e.g., square footage of living area and total rooms) can produce harmful collinearity in a regression model. Further, testing multiple forms of the model, to explore possible omitted variable bias and test robustness of results is crucial.

Therefore, to address collinearity, the accepted method for hedonic analysis is therefore not to include all possible independent variables, but to instead specify a relatively parsimonious model

that contains <u>key</u> variables that represent the various aspects of a home (e.g., size as measured by square footage; quality as measured by condition and the number of specialty items such as fireplaces, bathrooms, etc.; neighborhood influences such as school quality, etc.) and then to test whether the inclusion/exclusion of specific independent variables significantly impacts the coefficients of the focus variables. This was the protocol used in this study, as discussed in Appendix F and G, and is entirely consistent with the broader hedonic literature. Similarly, to address omitted variable bias (i.e., that the variables are measuring more than one characteristic), efforts should be made to construct many model specifications, with alternative datasets etc to test the reliability of the results. The results for the LBNL focus variables were found to be robust to the inclusion/exclusion of various potential sets of independent variables (including the number of bedrooms), and a variety of forms and datasets.

Mr. Wilson and IWAG Comment: Model is not peer-reviewed; data withheld from independent reviewers

Both Mr. Wilson and the IWAG claim that the Berkeley Lab report was not "peer reviewed" because the authors "refused to release the data set to reviewers."[8]

Response: Berkeley Lab conducted a thorough external review of the draft report, responded to follow-up inquiries upon request, and provided a full set of results with the draft report, all of which are customary for this type of report. The comments received during that process from roughly 20 external reviewers made up of experts and stakeholders were considered in the preparation of the final report.

Moreover, the authors plan to submit a shortened version of the report for consideration in a peer-reviewed academic journal. At that time, the authors hope to be able to release the dataset used in the analysis so that others can further verify the results. A number of confidentiality arrangements were required to obtain the data used in this report from the individual study areas, however, and those arrangements will need to be revisited and potentially re-negotiated before the final data set can be made available.

Mr. Wilson Comment: Dataset has too few observations with a view of the turbines and/or in close proximity to the turbines.

Mr. Wilson claims that the Berkeley Lab report dataset, despite being 7,500 transactions, has too few near the turbines, and/or with a view of the turbines to reliably estimate effects.

Response: Consider homes with a view (or near the turbines) to be the treatment group and the homes without the view (or further away) to be the control group. This comment can be taken in one of two ways: (1) the control group is too large; or (2) the treatment group is too small. With regard to the former, the size of the control group cannot be too large when determining the

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[8] Mr. Wilson also contends that the peer review process for journals, "does not, in any meaningful way, address the validity of the underlying work". Admittedly the journal review process is imperfect, but to imply that it is ignorant of the validity of the underlying work is "throwing the baby out with the bath water". The peer review process and ultimate publishing of research allows ample opportunity for methods and results to be challenged (e.g., Assorted 2006; Wilson 2006).

impact of the treatment (note it could be too small but this is not a consideration in the Report). And a large control group is required to estimate the other parameters in the model (effect of living area, bathrooms, house quality, etc.). With regard to the latter, if the size the treatment group is too small then the research could be affected by small sample size issues (e.g., spurious correlations, results driven by outliers, etc.). This is one reason why individual area data sets were pooled and the Report included extensive analysis of outliers and influential observations. The results pertaining to the focus variables were unaffected by either pooling or outliers/influential observations.

Conclusion

Although the IWAG's concerns are extensive, the majority of those concerns are not consistent with the extensive literature on the hedonic pricing method and its use in investigating the possible impact of amenities and disamenities on property values. Moreover, as discussed above, the authors believe that any relevant concerns expressed by the IWAG are already adequately addressed in the final report. The hedonic pricing model, as used in this study, is the appropriate method to address the question whether views of and proximity to wind facilities affect residential sales prices. Further, many of the limitations of the previous literature (e.g., small sample size, unreported statistical significance) are directly addressed by the Berkeley Lab analysis. The efforts made to benchmark the results to other literature and to test the robustness of the report's findings further substantiate the approach and results of the research. Therefore, although all analysis has limitations and additional research is warranted, the authors maintain that the Berkeley Lab work is the most reliable, comprehensive, and data-rich research effort to date in the U.S. or abroad on the possible impacts of wind projects on property values.

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