Appendix A: Non-Reporting and Strategic Misreporting

Appendix A explores the potential problems of non-reporting and strategic misreporting. However, as only 0.86 % of the observations lack data on measured discharges, non-reporting is minimal. While strategic misreporting is a potential issue, intentional misreporting is punishable by large criminal sanctions, including incarceration, which are imposed directly on individual employees (Shimshack and Ward, 2005). At a minimum, the EPA does not seem to believe that misreporting is a widespread problem (Bandyopadhyay and Horowitz, 2006; EPA, 1999b). To strengthen these claims, we test for strategic non-reporting and misreporting.

To test for strategic non-reporting, we implement a probit model that estimates the failure to report discharges in a given month as a function of the lagged discharge ratio (over the preceding 12-month period) and lagged inspections, along with control variables. The estimates reveal that the lagged discharge ratio does not significantly affect the dependent variable, consistent with Shimshack and Ward (2005). However, lagged inspections significantly reduce the likelihood of non-reporting, consistent with agencies' hopes. Agencies conduct inspections to ensure compliance with both effluent limits and reporting requirements. The effectiveness of these monitoring efforts does not reveal strategic non-reporting. Instead, this effectiveness reveals that facilities rationally respond to regulatory pressure.

We test for strategic misreporting in two ways. First, we follow Laplante and Rilstone (1996) by conducting a Sample Means Test on the discharge ratio derived from reported discharges. One sub-sample includes monthly observations when an inspector audited a particular facility; the other sub-sample includes observations when an inspection did not audit a particular facility. The test result reveals no significance difference between the two sub-samples' means (statistic = 0.16; p = 0.874). Second, we refine this Sample Means Test of the discharge ratio by restricting the overall sample to observations where the specific deterrence enforcement measures reveal a sanction against the particular facility in the preceding 12 months. Presumably, recently sanctioned facilities are more sensitive to the presence of inspectors on their premises. Again, the test result reveals no significant difference between the two sub-samples' means (statistic = -0.55; p = 0.586).

Lastly, we assess how the use of self-reported data may potentially impact the estimated coefficient on enforcement-related certainty and severity. In the case of misreporting, an increase in either enforcement component should prompt a facility to report a discharge level lower than true discharges because the marginal benefits of a reduction in discharges exceed the marginal costs of a reduction, akin to a true reduction in discharges. However, greater enforcement

pressure on discharge-based compliance, as captured by the discharge ratio, may also imply greater scrutiny on the discharge reporting process as reflected in a greater likelihood of catching discharge misreporting or bigger penalties on reporting violations; if true, a facility should report its discharges more truthfully. Thus, the overall effect on the extent of truthful reporting is ambiguous; yet, given agencies' limited ability to catch misreporting, the net effect is probably more under-reporting of discharges (i.e., lower discharge ratio relative to the truthful level). If a facility under-reports more, then the coefficients on enforcement-related certainty and severity are negatively biased (i.e., reveal excessive effectiveness). Thus, the reported coefficients represent upper-bounds in absolute terms. Similar to misreporting, an increase in either enforcement-related certainty or severity ambiguously affects non-reporting but most likely expands strategic non-reporting. By dropping high discharge levels, this expansion downwardly biases the enforcement coefficients.