surgery. These patients had only partial tears of the external anal sphincter, and were ineligible for inclusion. Of the remaining three patients in the overlapping group who were excluded, one actually had an intact external anal sphincter below a large second-degree tear, and two had previous third-degree tears. None of these patients should have been randomized, and none of them received the randomization treatment. We would argue that the only way a bias could have been introduced is if there were ineligible patients who were included (of which there were none), not if there were ineligible patients who were excluded.

The second group that was excluded was multiparous women, 11 in the end-to-end group and 14 in the overlapping group. These patients were originally enrolled in the study because they had undergone cesarean delivery for their first delivery, and they were considered to have pelvic structures unaffected by previous vaginal delivery. The authors decided at the time of analysis that any possible confounding factor should be excluded to make the study population as pure as possible. The multiparous women were thus excluded from the analysis. When we include this group, the primary outcomes of the study are unchanged (overlapping compared with end-to-end; flatal incontinence odds ratio 2.3, confidence interval 1.2, 4.6; fecal incontinence odds ratio 1.3, confidence interval 0.5, 3.7). Thus, their exclusion did not influence the results.

We would argue that the above exclusions were made for good reasons, and did not affect the "power of randomization to reduce selection bias for known and unknown factors." Our principal focus was on outcomes in a pure group of nulliparous women; thus, multiparous women were excluded.

Eight women in total were lost to follow-up, one in the overlapping group and seven in the end-to-end group. It was well documented by our research nurse that the reasons for the loss to follow-up were, in all cases, relocation of residence. To show that our exclusion of these women did not measurably distort the statistical significance of our results, we can assign outcomes to them that dilute the strength of the evidence favoring end-to-end repair, and instead favor the null hypothesis. We do this by replacing the one woman who was lost in the overlapping group by two women(one continent and one incontinent), and by replacing the seven women who were lost in the end-to-end group by eight women (four continent and four incontinent). Therefore, the outcomes of these additional 10 women tend to favor the null hypothesis. By this conservative inclusion strategy for women who were lost to follow-up, the two-sided *P*-value comparing the incontinence rate in the two surgical repair groups changes from $P = .015$ to $P = .020$.

Financial Disclosure: *The authors did not report any potential conflicts of interest.*

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Epidemiologic Research Using Administrative Databases: Garbage In, Garbage Out

To the Editor:

In a commentary,[1] Dr. Grimes argues that data from national discharge diagnosis registries are nothing but garbage, and research based on such data consequently also is garbage. He makes specific reference to a recent Danish National cohort study that suggested using combined oral contraceptives with the progestins desogestrel or drospirenone entails a 64–82% higher risk of venous thrombosis than using oral contraceptives with the progestin levonorgestrel.[2] This result was adjusted for length of use, estrogen dose, and educational length.

However inconvenient epidemiological results might appear, they do not become less true owing to the assertions by Dr. Grimes.

First, Dr. Grimes' starting point is erroneous. Discharge diagnoses have been used for decades all over the world, and are used by clinicians to communicate disease entities. Discharge diagnoses are the final clinical conclusion on what a patient suffered from and was treated for. Such data are therefore made by clinicians and used by clinicians, and not administrative data, as Dr Grimes argues.

Next, the validity of clinical data is – unfortunately – not always 100%. This is not owing to the fact that some countries like Denmark collect these diagnoses in databases, but a result of imprecise use of some codes by the clinicians. Generally, the validity of discharge codes decreases by increasing age of patients, but varies more according to different clinical fields.

Third, Dr. Grimes argues for rejecting the Danish registry data with reference to a validation study made in older patients (older than 50 years) with venous thrombosis in Denmark, including codes from emergency departments. If Dr. Grimes had made the effort to read our study, he would have realized that 1) we did not include discharge codes from emergency departments; 2) we previously made a validation of all the discharge diagnoses of venous thrombosis in the Danish National Patient Registry through a 5-year period in women aged 15–44 years, and found 90% to be valid, which was indicated in the article; and 3) the validation study included two unspecific codes (DI808 and DI809) which were not included in our study, and 4) the consequence of including about 10% uncertain diagnoses will tend to underestimate the influence of oral contraceptives on the risk of venous thrombosis, rather than the opposite, and is unlikely to disturb rate to ratio estimates between different product types.

Fourth, the results of the Danish registry data, including 4,213 women with venous thrombosis, are very much in line with the results of another independent Dutch study.[3] This study had access to some of the missing potential confounders in the Danish study, and first confirmed that adjustment for these confounders did not change the risk estimates materially, and that the adjusted rate to ratio estimates between oral contraceptives with different progestin types were the same as in the Danish study.

Finally, Dr. Grimes concludes that "Publications relying on unconfirmed database reports of venous thromboembolism should be ignored." I am not aware of any scientific study based only

on administrative data, and all studies, to my knowledge, address the issue of the validity of outcomes.

In Denmark, we have the opportunity to link the discharge diagnoses with data from the National Registry of Medicinal Product Statistics, in which we can access those who were anticoagulated after the diagnosis, thus validating each case from this simple merge of data. Prescription data are transferred to a central database by streak codes from all pharmacies in Denmark. That provides us with exceptional detailed information about use of medicinal products in Danish citizens since 1994. Thereby, we eliminate two of the important biases from previous observational studies: recall bias and incomplete updating of exposures through a study period.

We have, in recent years, received researchers from all over the world, including the United States, who want to take advantage of the comprehensive national health care databases we have established, acknowledging the many scientific strengths we achieve by linking data from these different sources.

My guess is that new studies will (again) confirm the Danish results, and that also American women will have experienced benefits from the Danish results in the meantime, by being able to choose those hormonal contraceptive products with the lowest risk of venous thrombosis.

Meanwhile, Dr. Grimes could perhaps give us just one example of scientific results from Danish databases which was proved false after, in his opinion, more valid studies were conducted elsewhere. Just one.

Financial Disclosure: *Dr. Lidegaard has received fees for speeches in pharmacoepidemiologic issues, including 1–2 from pharmaceutical companies per year. His institution received a grant for conducting supplementary analyses to his study published in BMJ referred to in this article, after a request from European Medicines Agency (EMA). The expenses for these supplementary analyses were covered by Bayer Pharma (Berlin, Germany).*

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In Reply:

Dr. Lidegaard's trust in the accuracy of discharge diagnoses is misplaced. An extensive amount of international literature documents that this coding is inaccurate and incomplete.

Both the diagnosis and coding of venous thromboembolism are problematic. Hence, for epidemiologic studies, these diagnoses must be confirmed by chart review or patient interview, neither of which was done in his 2009 BMJ publication.[1]

Specifically, misclassification of venous thromboembolism is a problem in the Danish National Patient Registry. In Dr. Lidegaard's earlier case-control study using this registry,[2] 51 of 1,660 potential cases had an invalid diagnosis according to the hospital department, 95 had an invalid diagnosis according to the patient, 80 were pregnant at the time of the event (despite being coded as nonpregnant), and 52 had a previous venous thromboembolism or other thrombotic disease. As noted in my commentary, Severinsen et al recently documented inaccurate coding of venous thromboembolism in patients aged 50–64 years in this registry as well. Misclassification of venous thromboembolism associated with oral contraceptive use is likely differential, which biases results.

The Dutch MEGA case-control study had important limitations.[3] Because of bias, many case-control studies reach incorrect conclusions, such as the putative link between abortion and later breast cancer or between intrauterine device use and infertility. These alleged associations were later refuted by better studies.

Contrary to Dr. Lidegaard's claim, the literature is replete with reports from administrative databases lacking validation of outcomes. More importantly, numerous Danish investigators have documented the uneven,[4] sometimes poor,[5] accuracy of diagnoses in the Danish National Patient Registry. As noted in one assessment, "… these figures conceal large differences in [accuracy of] specific diagnoses. Work with selected data requires the data to be confirmed in another registry or at the primary source."[4] I agree.

In response to Dr. Lidegaard's final question, such a study has indeed been published. The European Active Surveillance Study followed 58,674 women for 142,475 person-years of exposure to oral contraceptives; the focus was venous thromboembolism.[6] Each reported venous thromboembolism event had blinded adjudication, and 98% of participants had follow-up information. This study trumps methodologically weaker case-control, retrospective cohort, and registry studies.[7] It documented that all pills used were associated with a similar risk of venous thromboembolism; this suggests that the Danish National Patient Registry study was wrong about "generations" of progestins.

Financial Disclosure: *Dr. Grimes has served as a consultant for Bayer.*

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