issues which may impact any comparisons between groups." We disagree with the authors in this regard. Even if the OASIS study groups were considered as consecutive series, the findings still clearly demonstrated the safety and efficacy of the comparison approaches in nonparoxysmal AF patients.

Furthermore, the authors have compared the outcome of the OASIS trial with that of the LIBERATION (Outcome of Atrial Fibrillation Ablation After Permanent Pulmonary Vein Antrum Isolation With or Without Proven Left Atrial Posterior Wall Isolation) trial (5), where a 20% success rate at 1 year from pulmonary vein antrum isolation alone in persistent-AF patients was reported. We find this comparison inappropriate for the following reasons:

1. The authors have claimed that the substantial improvement in the success rate (52% in the FIRM+PVI group of the OASIS trial vs. 20% with PVI only in the LIBERATION trial) was due to the adjunctive FIRM ablation. This is most unlikely because the FIRM-only arm in the OASIS trial had a meager 14% success rate at follow-up.
2. Because the LIBERATION trial was designed to detect proven isolation of pulmonary veins in persistent-AF patients, stringent selection criteria were used to choose patients for this study. Furthermore, the follow-up duration was 3 years, whereas it was 4 to more than 12 months in the OASIS trial. Also, in the LIBERATION study, the success rate at 1 year was calculated using the outcome data collected at the end of 1 year in every patient. Therefore, it is not appropriate to compare the outcome of the LIBERATION trial with that of the OASIS trial.

Several articles from the founders of the FIRM-mapping technology have reported a very high success rate of rotor ablation, although other independent groups of highly skilled operators have failed to replicate their study findings. Clearly, large randomized trials will be necessary to resolve this controversy.

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REPLY: Are Rotors Markers of Substrate or a Mechanism of Perpetuation of Atrial Fibrillation? Increasing Data for Rotational Drivers of Human AF

We thank Dr. Mohanty and colleagues for their letter concerning our study (1). Their philosophy that it is unnecessary to map paroxysmal atrial fibrillation (AF) highlights our central point that "paroxysmal" or "persistent" encompasses heterogeneous AF populations who are often misclassified when examining continuous ECGs. Moreover, recent trials in paroxysmal AF report ~65% ablation success at 1 year, which are lower on longer follow-up and in persistent AF. Accordingly, there is a growing body of published reports on mapping electrical (drivers, autonomic ganglia) and structural (fibrosis, scar) substrates, as well as triggers at repeat ablation.

Dr. Mohanty and colleagues discuss the OASIS (Outcome of Different Ablation Strategies In Persistent and Long-Standing Persistent Atrial Fibrillation) trial (NCT02533843), retracted for stated issues with randomization and enrollment before trial registration. It appears that Gianni et al. (2) initially reported an 'observational study of consecutive non-paroxysmal AF patients' submitted before the registration date of the OASIS trial on ClinicalTrials.gov, of whom 29 were then included as a randomized cohort in NCT02533843 (OASIS) (3). Even if considered as consecutive patients as now proposed, inconsistencies include procedural components that were shorter for driver ablation plus pulmonary vein (PV) antrum isolation than driver ablation alone, and even shorter

for the most extensive approach, and other issues (4). Dr. Mohanty and colleagues discuss their LIBERATION study (Outcome of Atrial Fibrillation Ablation After Permanent Pulmonary Vein Antrum Isolation With or Without Proven Left Atrial Posterior Wall Isolation) which, in the same time period as the OASIS trial, produced <20% success from PV antral isolation at 18 months (not at 3 to 4 years as alluded; refer to its Kaplan-Meier curve) which is lower than other PV isolation reports. We agree with the authors that outcomes must be reconciled between reports, yet this serves as an interesting comparator for driver-based ablation in OASIS which lacked a PV ablation only limb. Finally, Dr. Mohanty and colleagues cite 2 studies of AF driver-based ablation with low success, yet omit several positive and far larger recent independent studies.

Electroanatomic mapping (e.g., Carto, NavX) is rarely used to map AF because it is difficult to mark activation times in AF. Dr. Mohanty and colleagues cite Benharash et al., who unsurprisingly did not show AF drivers using that method, but not Jalife et al. (5), who highlighted shortcomings of that study, including cycle lengths of 250 to 500ms in one-half of recordings (frequency 2 to 4 Hz in their graphs) that are hard to understand in AF regardless of the cycle length of induction stimuli (to which Dr. Mohanty and colleagues refer), basket catheters prolapsed into the ventricles, and inaccurate use of Shannon entropy. Dr. Mohanty and colleagues mention noncontact balloon mapping, yet this is seldom used in AF where correlation between its virtual electrograms and contact catheter recordings is often poor.

We welcome an open and honest discussion of hypotheses, methods, and results that will help move the field forward. Guiding AF ablation by substrate mapping could improve outcomes for patients, and much-needed multicenter randomized trials of AF driver ablation are underway. We thank Dr. Mohanty and colleagues for their interest in our work.

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