Pankove *et al.* Respond: Firstly, since our Letter was published we found that in some samples the boron-hydrogen complex becomes unstable above $\sim 130\,^{\circ}\mathrm{C}$; we agree with Sah *et al.*[1] that this complex is dissociated above 185 °C. Therefore, we must amend the statement in our abstract that neutralization of B by H occurs "between 65 and 300 °C" and restrict this to a lower range of from about 65 to about 150 °C. The reason why neutralization was observed in the early 300 °C run is that the hydrogenation occurred during cooldown with the plasma on. Since then, all the experiments were made with fast cooling (a few minutes).

It is well known that all the hydrogen evolves from silicon above $\sim 500\,^{\circ}\mathrm{C}$. The only purpose for our vacuum annealing at 500°C was to restore hydrogen-free initial conditions.

Secondly, we did not discuss the diffusion of hydrogen, a study we plan to do in the future. However, we do find that in a given sample the neutralization effect extends deeper with time, t, as $t^{1/2}$, and that the diffusion is impeded by high boron concentration. Furthermore, from the secondary ion mass spectroscopy (SIMS) measurements of H-implanted Si,[2] it is evident that a high defect concentration also impeded the diffusion of H. Dubé.[3] *et al.*[3] show that hydrogen diffusion along grain boundaries decreases rapidly with increasing surface recombination (i.e., with increasing dangling-bond concentration). Hence, arguments based on numerical values of the diffusion coefficient are on very shaky grounds. As to the apparent disparity with the results of Hansen *et al.*,[3] it is clear that the experimental conditions were not identical, e.g., our samples are not bathed and heated by the plasma.

Thirdly, our samples were etched and found free of swirl defects. Anyway, one can dismiss swirl defects as getterers of boron and deeper impurities because of the low temperature range we have used. Furthermore, the boron neutralization effect has been observed in a great variety of samples, including float-zone grown silicon. The return to the original flat profile profile of spreading resistance, R_s, upon dehydrogenation is a crucial experiment implicating hydrogen as the active element. As to the hydrogen neutralization of deep impurities or defects, if this were a significant effect, the Fermi level would move closer to the valence band, lowering the resistivity—an effect opposite to the one we observed.

Upon receiving a preprint of Ref. 3, we used SIMS to measure the concentration profiles of boron, oxygen, and hydrogen in several samples doped with $\sim 10^{18}$ and $\sim 10^{19}$ B cm^{-3}. We found the oxygen concentration to be two orders of magnitude lower than that of either boron or hydrogen. Hence, we can exclude any role for the BOH complex in our results.

Fourthly, while Pearton's fourth point is correct, our purpose in mentioning the prior experience with the hydrogenation of defects was only for its historical context.

Fifthly, although a variety of defects could complexify with boron in amorphous Si, our experience with neutralization of boron in crystalline Si suggests that hydrogen might also affect the doping efficiency of B in hydrogenated amorphous Si. The work referred to in our Letter supports this hypothesis.

Finally, Pearton has generously omitted to mention the most important error in our Letter: The misspelling of his name, for which we apologize.

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Received 16 May 1984
PACS numbers: 71.55.Fr, 72.80.Cw

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