Prokof'ev and Stamp Reply: The Comment [1] addresses both our nonlocal kinetic equation (whose form depends on intermolecular dipolar interactions, and was derived for arbitrary sample shapes and under various conditions of annealing), and also the relaxation function $\tau_Q(\xi)$, whose form depends on both dipolar interactions and nuclear spins. We begin by recalling salient features of these, and then address the Comment.

The kinetic equation (4) in our paper [2] leads to analytic predictions for an initially "factorized state" [when the two-molecule distribution $P_{\alpha\alpha'}(\xi, \xi', r, r'; t) = P_\alpha(\xi, r, t)P_{\alpha'}(\xi', r', t)$ at $t = 0$]. This includes (a) initially polarized states, and (b) initially annealed states; we suggested experiments on both. The internal longitudinal bias field $\xi(R, t) = [E(r, t) + \delta\xi(r, t)]$, with $E(r, t)$ the slowly spatially varying part, and $\langle\delta\xi(r, t)\rangle = 0$ for the rapidly varying part. Relaxation occurred [2–4] around the slowly moving surface $E(r, t) = 0$, with "hole digging" [4] in the distribution $M(\xi, t) = P_\uparrow(\xi, t) - P_\downarrow(\xi, t)$, initially over a bias range ξ_0 determined explicitly by the coupling to the nuclear spins [2,5].

The short-time relaxation was predicted to have the square root form $[t/\tau_Q(H_0^\parallel)]^{1/2}$, for general sample shape and material, provided it was pure, and in the quantum regime (for the effect of impurities, see [4]). The function $\tau_Q(H_0^\parallel)$ was called $\tau_{\rm short}^{\rm inh}$ in our paper, except for the special case of an ellipsoid (where it was called $\tau_{\rm short}$). We found [see Eq. (12), and also the last paragraph on page 5796, in [2]]:

$$\tau_Q^{-1} \to 1/\tau_{\rm short}^{\rm inh} \sim \xi_0^2 \frac{N(\xi = g\mu_B S H_0^\parallel)}{E_D} \frac{\Delta_{10}^2}{\Gamma_2}, \quad (1)$$

which depends on the nuclear spin properties through ξ_0.

Most of the Comment is irrelevant to all this. Chudnovsky "derives" a relaxation equation by rewriting our preliminary analysis of the ellipsoid [incorrectly—he misses the geometrical factor in Eqs. (6),(9), and Ref. [19] of our paper]. For some reason he then spends most of his Comment on the ellipsoid. His claim that "nuclear spins vanish from the final result" for τ_Q is simply wrong—for the general shape we analyzed, $\tau_Q^{-1} = \xi_0 N(\xi = g\mu_B S H_0^\parallel)\tau_{\rm short}^{-1}$ (*not* $\tau_Q = \tau_{\rm short}$, which is valid only for the ellipsoid). Since τ_Q depends explicitly on ξ_0 (in fact $\tau_Q^{-1} \propto \xi_0$ in the limit where $\xi_0 \propto \Gamma_2$), it depends directly on the coupling to nuclear spins. We gave a detailed theory of how the nuclear couplings determine ξ_0 (and thence the tunneling hole width). The tunneling hole has now been experimentally observed with roughly the predicted width, itself varying strongly with different nuclear isotopes [6–8]. The dependence of τ_Q on field, shape, and initial conditions, were also key features of our predictions—this is discussed at length in Ref. [2] and our other papers [3,4,9]. Monte Carlo simulations, by ourselves and others [2,4,10], illustrated this and confirmed the analytic work.

In view of this, Chudnovsky's remark that "contrary to their statement, however, the relaxation depends on the initial conditions and the sample shape" is simply incomprehensible, as are all the other remarks he makes about the general case (which are based on his discussion of the ellipsoid).

We are also mystified by the assertion that we "leave the impression that the square root is a consequence of nuclear dynamics." In our derivation the $\sqrt{t}$ form (but *not* the coefficient τ_Q, nor the holewidth ξ_0) plainly comes from the dipolar interactions.

In view of all this we see little point in discussing Chudnovsky's remarks concerning the relationship between experiment and theory (although we are surprised by his cavalier dismissal of so many experimental papers [6–8,11–13]).

N. V. Prokof'ev[1] and P. C. E. Stamp[2]
[1]Department of Physics
University of Massachusetts
Amherst, Massachusetts 01003
[2]Spinoza Institute
Leuvenlaan 4
University of Utrecht
3584CE Utrecht, The Netherlands

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