Igarashi and Lin Reply: In the previous paper [1] Matveenko criticized our earlier paper [2] where we presented a calculation of the cross section for the $D^+ + H(1s) \rightarrow D(1s) + H^+$ charge transfer process at low energies. We address the two specific issues raised by him and present our rebuttal to his criticism.

First he claimed that the statement "The method is free from all the inherent ambiguities associated with the conventional BO approach, such as ... spurious couplings" made in the abstract of [2] is wrong. He was referring to the nonphysical couplings in the hyperspherical-adiabatic (HA) approach, as given in Eq. (1) of his Comment [1]. However, this nonphysical term is well known since the early work of Macek [3] in 1968. It is easily corrected in the manner of Eq. (2) of his Comment [1]. Our calculation did account for this correction. Another point: the spurious coupling we mentioned in the abstract referred to the strict BO approach. The transformation leading to Eq. (3) of his Comment is a departure from the BO approach. That transformation cannot be carried out for general collision systems. Our statement in the abstract is correct.

The second criticism is that the "rotor model" we employed has been obtained by Matveenko and Abe [4] and also in a more recent paper by himself [5]. To begin with, he has not understood that the rotor model we employed is unrelated to his two quoted works. Our rotor model has nothing to do with the Coriolis coupling, while his work is an attempt to incorporate Coriolis coupling into his $J = 0$ formulation. It is not our responsibility to challenge the validity of his quoted works here, but we are aware of no numerical calculations ever supplied to support the approximations employed in his derivations. In our paper [2], we provided numerical evidence for the validity of our rotor model.

In fact, the rotor model appears trivially in the hyperspherical approach. Referring to the Hamiltonian in the body frame, the kinetic energy operator can be written as the sum of three terms, $T_0 + T_1 + T_2$. The explicit expression of each operator is given in Lin [6]. We need only to note that T_0 does not depend on the angular momentum J, and T_2 is the Coriolis coupling. The expression for T_1 is

$$T_1 = \frac{I^2}{\sin^2\phi \cos^2\phi \sin^2\theta} + \frac{J(J+1) - 2I^2}{\cos^2\phi}. \quad (1)$$

It is clear that for a fixed I (the azimuthal quantum number in the body-fixed frame), the J dependence occurs only in the last term. For ion-atom collisions the range of the hyperangle ϕ is limited to very small values near zero except at very small hyperradius. "Our" rotor model emerges immediately if we approximate $\cos\phi = 1$ in Eq. (1). Our numerical results support the validity of this approximation, as shown in Ref. [2]. We have not made any approximations on the Coriolis coupling term T_2. For each I, this term is independent of J within our rotor model. Furthermore, T_2 does not enter in our two-channel approximation employed in Ref. [2]. Oddly Matveenko, as stated in his Reply, would replace the $J(J+1)$ term in Eq. (1) by $J(J+4)$, a procedure which has not been justified by numerical evidence.

In summary, we stand to the results of our paper [2]. The two criticisms of Matveenko have no substance. We also take this opportunity to point out that attempts have been made to carry out ion-atom collisions using reaction coordinates [7], but the method has not been widely adopted. The hyperspherical approach presented in our paper [2] appears to be the best way to address ion-atom collisions involving one or one-active electron systems.

Akinori Igarashi[1] and C. D. Lin[2]
[1]Department of Applied Physics, Faculty of Engineering
Miyazaki University
Miyazaki, 889-2192, Japan
[2]Department of Physics, Cardwell Hall
Kansas State University
Manhattan, Kansas 66506

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