Organic Process Research & Development **1999**, *3*, 379

Response to Comment on "Kinetics of Catalyzed Liquid-Phase Oxidation of *p*-Nitrotoluene by Air in Basic Medium"

To the Editor:

Dr. Felix Sirovski has raised some interesting inquest on our article.[1] We are gratified to answer his queries and are quite sure that the following points are sufficient to remove the trepidations Dr. Sirovski has.

First of all, it is quite a recognized fact that under highly basic conditions, $CoCl_2$ and $CoBr_2$ transform to $Co(OH)_2$, leading to heterogeneity. Keeping this truth in mind, we executed the reactions under a very high flow rate (3 L/min) to maintain a high superficial linear velocity for adequate mixing and a high air pressure, (16 atm) to eliminate the diffusional mass transfer factor. It is perceived that, primarily, with an increase in air-flow rate and air pressure, the rate of reaction also increased, but after a certain air-flow rate (Table 2) and air pressure (Table 3), the reaction rate is independent of further increase in air-flow rate or air pressure. Our operating conditions are well above that distinctive flow rate and pressure. Thus, it is clear that under the reaction conditions, the diffusional mass transfer is quite negligible and the data exhibit the true kinetics of the process.

The observed low activation energy does not mean that the reaction is controlled by diffusion. It is shown from the data that under our operating conditions, diffusional mass transfer is insignificant, and thus it is transparent that the reaction is intrinsically a very slow reaction.

The enhanced rate of oxidation with Co-phthalocyanine is not fully for the homogeneity of the catalyst system but predominantly for its structure. For the specific molecular structure of metal phthalocyanines and porphyrins, they are well-known oxygen receptors. In this case, the reaction is a catalytic gas−liquid interfacial reaction; the activity of the catalyst site to bind free oxygen plays an important role in such kinds of reactions.

We performed a reaction under the identical reaction conditions without any catalyst, and after the same reaction period, no conversion to the product was attained, and it quite agrees with the published literature.[2] It clearly argues that the reaction is absolutely a catalytic reaction. We showed the linearity only under the operating domain of catalyst loading, and we are quite assured that if we would have interpolated our data, we could secure a best-fit straight line passing through the origin. Therefore it does not evince that the reaction is noncatalytic or that a nonlinear rate depends on the catalyst loading.

We agree with Dr. Sirovski that there is an error concerning the effect of solvent. We should not have used the word homogeneous. We regret that mistake.

In conclusion, we relish the positive and interesting comments made by Dr. Sirovski and presume that our exegesis will appease his oddity.

Sincerely yours

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Received for review April 21, 1999.

OP990046G

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(2) Sasson, Y.; and Neumann, R. *J. Chem. Soc., Chem. Commun.* **1985**, 616.

10.1021/op990046g CCC: $18.00