In the last ten years, much progress has been made in the study of meeting real-time application requirements. The five-year research initiative of the United States Office of Naval Research, which ended a few years ago has yielded a plethora of new results and paradigms for real-time resource scheduling problems, for example, results extending the analysis of the classic paper by Liu and Layland, imprecise computation, and communication scheduling. Some of these results are beginning to be used in practice, (for example, RMA and related theories). Paradigms proposed by the real-time systems community have also found their way into other disciplines such as rate-control communication protocols. The so-called linear-bounded processes in multimedia traffic are analogous to the concept of sporadic processes in real-time computing. There is also a world-wide resurgence of interest in formal and other aspects of real-time programming.

On the other hand, extant work in real-time systems research has been criticized for being fixated on static models. However, static models are not appropriate for next-generation distributed real-time applications that are likely to be adaptive in nature, (for example, to provide a high degree of fault tolerance). During the last few years, the real-time systems community has started to counter this criticism by extending traditional work to cover newer application domains. The central problem remains, however, that the concept of adaptivity is often domain-specific and sometimes ill-defined in the context of
bringing distributed real-time systems concept into better focus. Accordingly, be it resolved that future distributed embedded and real-time applications will be adaptive and that meanings, challenges and research paradigms await discovery. The charge to the panel is to defend (or to dismiss as fluff) the above resolution.