Rescuing Democracy
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This afterword offers a theory of institutional design. As such, it may help those who want to devise a political institution themselves; perhaps one that holds more definite promise than the People’s Forum and other designs such as those described in Chapter 6.

As Rothstein (2011, 216) points out, institutions may be formal, such as legal and administrative structures, or they may be informal, such as generalized trust and other elements of social capital. It is the formal type that we focus on here, and it is suggested that these may be most effectively designed by the method that was used to invent the People’s Forum. Acceptance of this suggestion may of course be influenced by whether the Forum is shown to work in the real world of democratic politics. If it cannot be established, or if it is and fails to be effective, then more attempts are needed to design an institution of similar purpose, and the method set out here may help. In that situation, however, experience will have indicated that the method is deficient, so it will need improving or replacing. Nevertheless, it does at least provide a starting point. And if the People’s Forum is successful, then the method may be seen as beginning to prove itself and might therefore be used to try to devise other formal institutions.

In contemplating the Forum’s design and the following description of how it was produced, one might be prompted to
ask: ‘Why wasn’t that particular design suggested long ago?’ This question becomes obvious when it is recognized that the diagnosis guiding that prescription is basically well-known. Its first part, ambiguous delegation, is merely a reinterpretation of the old dilemma of whether political agents are delegates or trustees. The second part, excessive political competition, is widely recognized, but tends to be seen as necessary for the energy, transparency and accountability that it generates. And the third dysfunction, excessive compromise, has worried democrats at least since Plato emphasized in *The Republic* that democracy could never uphold the kind of knowledge that was needed to cure the world’s evils (Shapiro 2012, 193–94). So the diagnosis has been broadly apparent for a long time and the prescription might therefore be expected to have been concocted decades ago. It did not have to await the arrival of technology, such as television, the internet and the smart phone, as the People’s Forum could conceivably work across large jurisdictions with technology no more advanced than print media, radio and snail mail post.

The very small number of national-scale innovations described in §6.5.2 may provoke a similar question: ‘Why has political science come up with so few practical designs for improving democratic function across provinces and nations?’ Part of the answer may be that as government failure has always been experienced by most people it is seen as normal, rather than as a problem that urgently needs solving. Moreover, a particular policy or action that is seen by some citizens as government failure might be seen by others as good government, so what’s the problem? And if there is a problem, then it appears to have already been tackled, for many attempts have been made to design better democratic government, as shown by the great variety of these around the world (e.g. Lijphart 2012). However, these different forms may instead be considered fundamentally alike, which is the approach tried in this book. Further reason for the rarity of fundamental reform in democratic government is that it seems impossible to implement because most incumbent politicians support the current system, as this is where they have
been successful. And finally, the crafting of such reform seems too difficult as there is no recognized method for designing political institutions (see Goodin 1996, 31 — and quoted below). It is this deficiency that I now attempt to remove, by describing the process used to devise the People's Forum.

This method was only recognized in hindsight, after the design was produced. But it may derive merit in being deduced from the actual experience of devising an institution. The deduction began by recognizing that several distinct strategies had been used. These are quite unremarkable as they are widely and routinely used in everyday life and research, so their specification in a method may seem unnecessary. However, it is largely the manner in which these strategies were applied that produces the method. As this method is, in effect, a theory of how to design institutions, it may fill the gap described by political theorist Robert Goodin (1996, 31) as a ‘paucity of literature specifically on design issues’, with ‘little analysis as to what it means for institutions (or anything else, for that matter) to be designed and still less analysis of what principles might properly guide such design attempts’. That gap was clearly demonstrated by the book — *The Theory of Institutional Design* — in which Goodin made those comments, as none of its ten articles provided a theory of how to design an institution, despite the apparent promise of its title. Instead, those articles discussed functions that institutions should perform and what might be expected from institutions in practice, including what might happen to them. That book was an early contribution to the series ‘Theories of Institutional Design’, published by Cambridge University Press. Many years and volumes later, this series has yet to publish a basic ‘theory of institutional design’, in other words, a theory of how to design an institution.

Such a theory is now offered by proposing that five strategies are useful and perhaps both necessary and sufficient for designing an institution, provided that they are applied in a particular sequence.
The five strategies of the method

The first strategy: Utilize existing information
In looking for greater progress in his discipline, political scientist Jon Bond (2007, 904–5) offered the following advice.

The kind of revolution necessary to propel political science to the next level of development is a revolution in theory. Sir Isaac Newton’s contribution to the science of physics was not the basic research he did, but rather it was his recognition of how to put what physicists already knew together into a new overarching theory. I believe it is possible that political science has accumulated enough information about how and why politics work as they do to support such a synthesis.

The first strategy follows this suggestion. Only preexisting information was used to identify the problem and to try to solve it. I did no systematic empirical research and relied largely on the interpretations of experts in whatever research appeared relevant. The project was therefore multidisciplinary, ranging across history, biology, ethology, political science, public choice, ecological and neoclassical economics and evolutionary, social and cognitive branches of psychology. These sources have been augmented by nonacademic sources of information and my own political and psychological experience, where these appeared reliable and relevant.

There is one exception to this approach, and this is part of the fourth strategy, in which potential solutions are tested to provide new information that show whether the design process needs to be continued and, if so, to guide it. One of the ways in which this may be done is by the second strategy of design: defining or redefining the problem.

The second strategy: Define the problem
The second strategy is that the problem to be solved must be carefully defined. With political problems this may produce a definition that implies not only whether the solution requires
a new institution, but also the function of this institution and therefore possible outlines of its design. Definition may thereby indicate the solution. Perhaps the first people to state this strategy in the field of governance were two experts in design and city planning, Horst Rittel and Melvin Webber. They had recognized a widespread confusion about goals in public policy and suggested that it was happening because citizens in democracies had started ‘asking for a clarification of purposes’ (Rittel and Webber 1973, 157). As public infrastructure planners they recognized that

goal-finding is one of the central functions of planning… Goal-finding is turning out to be an extraordinarily obstinate task … Planning problems are inherently wicked … The information needed to understand the problem depends on one’s idea for solving it … To find the problem is thus the same thing as finding the solution. (Rittel and Webber 1973, 160–61, emphasis in original)

Political scientist Michael Harmon and public administration expert Richard Mayer (1986, 9) subsequently endorsed this view of ‘wicked’ problems: ‘the choice of a definition of such a problem, in fact, typically determines its ‘solution’”. Others have expressed a similar assessment of how to tackle any problem, whether it could be classed as ‘wicked’ or not. For example, Albert Einstein observed that the

mere formulation of a problem is far more essential than its solution, which may be merely a matter of mathematical or experimental skills. To raise new questions, new possibilities, to regard old problems from a new angle requires creative imagination and marks real advances in science. (QuoteWorld 2008)

Political scientist Ian Shapiro (cited in Topper 2007, 574) has noted that surprisingly little attention is given to problem specification in the study of politics. Therefore, ‘one central task for political theorists should be to identify, criticize, and suggest plausible alternatives to … the specifications of problems … and
to do it in ways that can spark novel and promising problem-driven research agendas’ (Shapiro 2005, 180).

Part 1 of this book is devoted to defining the problem, first by working from evidence to observe its existence and describe its essence, then by using more evidence to check the accuracy of that definition.

The third strategy: Utilize only necessary and sufficient information

My third strategy was to try to identify and use only the information that is necessary and sufficient for the task at hand. This is important because the task — to devise a better form of government — is to solve a very broad problem. If the problem was narrowly specialized, this strategy may need little emphasis, for solutions of narrow problems are likely to present fewer opportunities for fruitless digression. Researchers of narrow problems usually need to look for details within a specialized field, whereas those who tackle broad problems may need to look for large-scale patterns or general effects across several fields. Specialization might be expected to cultivate not only skills in focusing on detail, but also a preoccupation with this, so that the capacity to take an interest in large problems and to regularly back off from the details to assess their relevance to a big picture may be neither valued nor developed. This may explain the observation of political theorist David Held (1991, 4) that, while specialization need not always lead to the fragmentation of knowledge, this seems to have happened in the case of politics and related disciplines … we seem to know more about the parts and less about the whole; and we risk knowing very little even about the parts because their context and conditions of existence in the whole are eclipsed from view.

Fung (2007, 443) subsequently emphasized this problem, observing that specialization ‘has become a segregation of thought that now poses a fundamental obstacle to progress in democrat-
ic theory’. Specialization has produced many theoretical models of democracy, but as Graham Smith (2009, 10) observes:

No practical design [for a democratic institution] can realistically hope to meet all the rigorous demands of any particular theoretical model… While theoretical work often proceeds as if it were an exhaustive account of democratic politics, theories offer only a partial analysis of our democratic condition… we tend not to develop fully-fledged theories of democracy (whatever they would look like)…

An article on page C1 of The New York Times on October 20, 2009 reported several leading political scientists as recognizing that their discipline was finding it increasingly difficult to appear relevant to broader social and political discourse. One of these scholars was Joseph Nye, University Distinguished Service Professor at Harvard, who warned that the motivation to be precise had overtaken the impulse to be relevant: ‘the danger is that political science is moving in the direction of saying more and more about less and less’ (quoted in Holmberg and Rothstein 2012, 1). In 2010, this problem was addressed by panels in the annual meetings of both the British and American political science associations. Doubt about the relevance of much political theory parallels Goodin’s concern at the absence of a fundamental theory of institutional design — that is, the lack of specification of a method of design. The irrelevance of much political theory provoked Mark Warren (2002b, 683) to comment that, near the turn of the century, innovations in democratic participation were demonstrating ‘that reality is, once again, ahead of democratic theory’.

In executing the third strategy of only using information that is necessary and sufficient, the second strategy of carefully defining the problem was used to indicate which information is necessary and sufficient. In combining the first strategy of using existing information with the third strategy, the presentation of details in data from experiment and observation was minimized so that patterns are not missed because of a narrow focus. Pattern recognition, in the form of generalizations and conclusions
by experts who know the technical details, was used as much as possible.

The fourth strategy: Pushing and pulling
Two scholars of public management and policy, Erik-Hans Klijn and Joop Koppenjan (2006, 158) have observed that institutions for public administration must be designed by careful experimentation.

Institutional design is not a simple activity. The nature of institutional design, its process and its impact are not very well understood. Institutional design is a process of pushing and pulling with uncertain results … research into institutional design is still in its infancy.

Klijn and Koppenjan (2006, 155–56) explain that ‘pushing and pulling’ is partly needed to accommodate the power relations between the parties affected by the institution, and this is because how the ‘formal decisions in institutional arenas aimed at changing network rules will work out in the games played within networks is highly uncertain.’ This means that institutional ‘designs are by definition imperfect and should be seen rather as the start of a trajectory of institutional change than as a definitive design’ (Klijn and Koppenjan 2006, 156). ‘Pushing’ is taken here to be the design phase and ‘pulling’ to be the testing of a design, so that design-and-test must be followed by more design-and-test until a design is produced that performs well. The repetition of this design-and-test cycle is minimized by making the attempts at design as effective as possible. To this end, the second strategy is crucial — the problem must be carefully defined.

The fifth strategy: Thought experiment
Nobel laureate in economics Paul Krugman (1998, 19) has declared that you
can't do serious economics unless you are willing to be playful. Economics…is a menagerie of thought experiments — parables, if you like — that are intended to capture the logic of economic processes in a simplified way. In the end, of course, ideas must be tested against the facts. But even to know what facts are relevant, you must play with those ideas in hypothetical settings.

My fifth strategy was to follow such advice and use thought experiments. This ‘play’ may be described as applied imagination or attempts to mentally simulate reality, and it may not only save much time and expense, but encourage endeavours that otherwise would never be undertaken. Some of its use in this study was for the ‘pulling’ or testing required by the fourth strategy, because testing by experimental trials in the real world of politics is not possible at this early stage of the design process. As discussed in §6.4 and §7.2.12, such operational trials require considerable political commitment and/or funding. Part 1 mainly defines the problem that is being tackled as ambiguous delegation, which leaves public opinion largely in control of public policy but too disengaged to do a good job with its tactical and strategic components. That description of government failure shapes both the mission of the design and the two strategies it employs to accomplish its mission, which thereby implies a broad outline for the design. In §9.3 it is indicated that this design cannot execute its strategies for a small group, because it uses motivations to execute those strategies that can only arise in very large groups. However, the analysis and synthesis of Parts 1 and 2 are the first steps towards a large-scale trial, because they describe why an institution is needed and the type that appears to have the best chance of being able to perform the required functions. Thought experiment or imagination is crucial for getting to this stage, because if it can, to some extent, simulate the ability of a design to solve a problem, or identify weaknesses in a design that point to a ‘better’ one, then it should expedite the negotiation of Klijn’s and Koppenjan’s ‘trajectory of institutional change’. This is because much of the ‘pushing and pulling’ is done conceptually before it is necessary to do any of
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It should be emphasized here that a ‘better’ design is one that is not only likely to work better than others, but is also more feasible to implement.

‘Pushing and pulling’ as science

The push (design) and pull (test) of designing institutions is essentially theorizing to try to explain data followed by more data collection to test the theory, then re-theorizing and re-testing, and so on. The cycle begins when existing data indicates a problem that is serious enough to try to solve. To start doing that, the problem is defined. As this is a type of explanation and theories are explanations, the definition of a problem may be virtually a theory of its cause. If the definition, together with the theory it implies, indicates a design solution that is judged impossible to implement, this judgment constitutes additional data that instructs us to try to redefine/re-theorize the problem in a more helpful way. Redefinition may require more data to be gathered. If the new definition/theory implies a design solution that could possibly be implemented, then testing it becomes the next step, which is another act of data collection. Such a test might be any or all of three types: first, a thought experiment, which may be inconclusive as it has limited capacity to produce new data that are reliable; second, an inspection of the performance and operating environment of existing institutions that resemble this solution and its anticipated environment; and third, establishing the institution that appears to be the solution in the environment it is designed for and running it to see if it works. Such tests may indicate that the design is flawed and, if so, that data may be used to guide another redefinition of the problem.

Klijn and Koppenjan’s ‘pushing and pulling’ may thus be seen as a cycle of gathering data (pulling), then defining/theorizing/solution design (pushing), then gathering more data by testing the design (pulling) and then re-defining/re-theorizing/re-designing (pushing) and so on until a design is found that works well enough. This observing–theorizing–observ-
ing–theorizing iteration is, of course, the scientific method, the process of ‘scientific inquiry’ (Pigliucci 2010, 303–4). According to theoretical physicist Lee Smolin (2006), neglect of this method over the last thirty years is causing particle physics to stagnate. He suggests that the neglect arises because most physicists are preoccupied with string theory, which minimizes the formulation of different theories and thus the competition between them. This blocks the observation–theorizing–observation–theorizing iteration in two ways. One is that the paucity of competition between theories limits the variety of observational tests that might provide information leading to better theories (2006, 304–5). The other way is that string theory itself is not a real theory as it is not falsifiable — and also is not ‘confirmable’ (it is not possible to confirm a prediction that only that theory makes) (Smolin 2006, xiii–xv). String ‘theory’ is therefore disparaged by some of its sceptics as ‘not even wrong’ — it does not make predictions that could falsify it.

The need for both pushing and pulling was stressed by the eminent astrophysicist of the early twentieth century Sir Arthur Eddington (n.d.) when he pointed out that observation is not enough for effective science: ‘It is also a good rule not to put overmuch confidence in the observational results put forward until they are confirmed by theory.’ So pushing and pulling is the scientific method, in which the meaning of observation is formulated with theory (pushing) and this is then tested with more observation (pulling). As such, it was the basis of my method of designing an institution. Pushing and pulling organized the implementation of the other four strategies. However, as this is being written the pulling (testing by observation) remains to be completed with the third mode noted above, that of operational scale trials in the real world of politics.

The very real difficulty of pulling or testing theories DESIGNS in political science means that ever finer details are often investigated instead, producing theory without it being tested in the real world to see not only whether it works, but whether it does so for problems that are serious enough to warrant explanation and solution. This appears to be the unproductive specialization
that raises a ‘fundamental obstacle to progress in democratic theory’ (Fung 2007, 443), as discussed above in the description of my third strategy of institutional design. To some degree, Bond (2007, 905) has also noticed this: ‘it is possible that political science has accumulated enough information about how and why politics work as they do.’ Particle physics appears to have run into the same problem: theory has specialized to the stage where its experimental testing tends to be impossible or extremely expensive, for example, the 2010 budget for the Large Hadron Collider was US$9 billion. Lack of testing may obscure the insignificance of a theory by allowing it to be developed in order to employ a problem-solving method favoured by the theorist. If that theory is tested, it may show that the favoured method of theorizing is inadequate, forcing a search for other methods that are more suited to the task. Political scientists Donald Green and Ian Shapiro (1994; Shapiro 2005) contend that rational choice theorists have made this mistake. Strong preferences for particular methods, such as mathematical analysis or the use of assumptions from neoclassical economics, may draw the scholar’s attention to problems that appear amenable to these methods. However, this may mean that problems are studied that are of little significance, or that alternative explanations of significant problems are ignored. Such bias may be revealed by testing a proposed solution, for if the test shows it to be either a failure or an inconsequential success, then the problem may be re-evaluated, so that it is more constructively defined or replaced with a problem that is more significant. Green and Shapiro’s stipulation that research must be problem-driven and Shapiro’s subsequent stipulation that it should also be ‘theory-driven’ (Shapiro 2005; Topper 2007, 574) is followed by my second strategy of carefully defining the problem. The fourth strategy of ‘pushing and pulling’ assists that strategy by checking and rechecking that the problem is well-defined. In turn, the second strategy assists the third strategy because constructive definition of the problem points to the information that is necessary and sufficient to solve it.
The completion of pushing and pulling
To make the ‘pushing’ or designing phase as effective as possible, it might seem that collective action theory would be crucial, as it should help define the problem and thereby indicate the type of institutional design that would solve it. However, as Nobel Laureate in economics Elinor Ostrom (2007, 203) has pointed out, experimentation is necessary because collective action theory is in need of considerable further development.

[A] key lesson of research on collective action theory is recognizing the complex linkages among variables at multiple levels that together affect individual reputations, trust, and reciprocity as these, in turn, affect levels of cooperation and joint benefits. Conducting empirical research is thus extremely challenging… The reason that experimental research has become such an important method for testing theory is that it is a method for controlling the setting of many variables while changing only one or two variables at a time… Instead of looking at all of the potential variables, one needs to focus in on a well-defined but narrow chain of relationships… One can then conduct analysis of a limited set of variables that are posited to have a strong causal relationship… the theory of collective action is not only one of the most important subjects for political scientists, it is also one of the most challenging.

Ostrom’s view emphasizes the need for the fourth strategy: we must experiment, designing and testing (pushing and pulling) until we get it right. But as discussed in §9.3, careful definition of the problem in Part 1 of this book has produced a design for reforming democratic government that is unlikely to work with politically uninfluential experimental groups such as random samples or small communities. So until the resources can be mustered to test it on whole polities, this must be attempted by thought experiment (or possibly by computer simulation). At the stage of the project achieved here, purely conceptual pushing and pulling that relied on thought experiment has yielded a design that appears promising enough to warrant the cost of a trial (or ‘pull’) in the real world, at the scale of a province or a
nation. Such an experiment offers the possibility of completing the application of the fourth strategy of pushing and pulling, but in order to do this the necessary financial resources must be raised.

Applying the five strategies

I applied the five strategies by commencing with the first. Existing information suggested that democratic government tends to malfunction. In the late 1980s this seemed personally apparent to me from involvement in two types of work. One was my occupation as a public servant. For twenty-five years I was a government forester engaged in planning and managing commercial and non-commercial uses of natural resources. My other area of work was a private pursuit of environmental concerns, which included helping NGOs with conservation campaigns. As serious democratic dysfunction became more obvious from these two perspectives, I used the second strategy of defining and redefining the problem. In doing this I applied the first strategy of using existing information that might further illuminate the problem, together with the third strategy of trying to make sure that the information I looked for was necessary and sufficient for this purpose. This took many years as a part-time pursuit and in retrospect I expect that I would also have started to employ my fourth strategy, by testing (pulling) the definition and the type of solution that it implied (the push) with the fifth strategy of thought experiment. That is, I would have tried to imagine whether my current idea of the solution appeared both feasible to implement and likely to work. It was in this mode that Bob Brown and I (as noted in Acknowledgments) discussed the need for something like Citizen Initiated Referendums. If mental simulation had indicated that my current idea of a solution was either extremely difficult to implement or unlikely to work, then I would have made another attempt (following the fourth strategy of repeated design-and-test) at a definition (the second strategy) that would imply a more effective design. The evidence hinting that I had worked in this way was that I simul-
taneously produced a definition of the problem and a design for the solution. But this was not apparent for many years, for it was only the design that seemed to have been devised. This was written down (now elaborated as Part 2) about fifteen years before the problem was fully defined on paper (see Part 1). However, as that definition was written out, the ease of doing it showed that, whether it was right or wrong, it was well-formed in my mind. Thus the fourth strategy of repetitive ‘pushing and pulling’ had coordinated the use of the other four strategies to produce both a definition of the problem and a design for its solution. The definition included a classification of those characteristics of policy issues that appeared to be part of the problem, as set out in §4.1. As it is merely thought experiment that indicates this design may be effective, the next step must be a real-world experiment. As indicated in §9.3, this ‘pulling’ requires the design to be operated in a polity such as a province or a nation for the period that it was expected to need in order to begin producing results.

My explanation that I had used these five strategies is, of course, not thoroughly supported by evidence, and it assumes that unconscious thinking played a large part — for example, in defining and redefining the problem; in inferring a particular design from a particular definition; and in judging what information is necessary and sufficient for ‘pushing and pulling’. However, the idea that unconscious thought is efficacious when driven by strong desires to solve problems is well established. A century ago, the French polymath Henri Poincaré included it as a part of his four-stage description of how he solved tough problems. Those stages are: conscious thought, unconscious thought (incubation), illumination and then verification. Historian of science Arthur Miller (2010) considers that this model of creativity is the best we have. He notes that psychologist Edouard Toulouse, who was an expert on creativity, was satisfied with that view after personally psychoanalysing Poincaré. Other scientists such as Einstein, Helmholtz and Heisenberg have described their problem-solving in the same way. Einstein emphasized the importance of both creative imagination and relentless persistence, and this has been endorsed by Howard Eves, an
eminent historian of mathematics, who observed that an ‘expert problem solver must be endowed with two incompatible qualities — a restless imagination and a patient pertinacity’ (quoted in Singh 1997, 225).

The method as a theory of institutional design

We have seen that ‘research into institutional design is still in its infancy’ (Klijn and Koppenjan 2006, 158); that there is a ‘paucity of literature specifically on design issues’ (Goodin 1996, 31); and that the theory of collective action, which should guide the design of political institutions, ‘is not only one of the most important subjects for political scientists, it is also one of the most challenging’ (Ostrom 2007, 203). Graham Smith’s (2009) Democratic Innovations is a recent contribution to the Cambridge University Press series ‘Theories of Institutional Design’, and although it does not produce a theory of how to design political institutions, it moves a little in this direction by offering a framework for assessing their capabilities. Such a framework is useful, perhaps essential, for the testing (pulling) phase of the cycle of define/theorize/design — test, re-define/re-theorize/re-design — retest, and so on. This may be so whether the testing is a real-world trial of an institution or just the thought experiment of subjectively assessing the capabilities of a design, as is done here for four designs in Chapter 6 with a framework adapted from Graham Smith’s.

One of several desirable capabilities of institutional designs that are recommended by Goodin (1996, 40) is that we should ‘design our institutions in such a way as to be flexible … to admit of ‘learning by doing’ and to evolve over time. Thus, we might say revisability is one important principle of institutional design’. This may be a very useful criterion for preferring one design over another, but my fourth strategy of defining/theorizing/designing, then testing followed by re-defining and so on may be seen as more fundamental because it could point towards several designs that may then be ranked for suitability according to attributes such as their revisability. Another way of
understanding the fundamental nature of the fourth strategy is that it invites the replacement of a design that performs poorly with another that is produced by redefining the problem it is intended to correct. This may mean that the new design is so different that it could not have been produced by revising the design it replaces. Goodin’s ‘revisability’ conflates the desired capability of designs with the method of producing them whereas my fourth strategy (along with the other four) focuses only on method.

My method for designing institutions may also help solve the problem observed by David Held, Archon Fung, Graham Smith and others, that specialization may obstruct theoretical progress in some social sciences. The iterative process of the method may solve it by each cycle checking the usefulness of narrow focus. This is likely because the method is to produce a definition/theory/solution that is then tested in some sense so that deficiencies may be discovered, which may then elicit a new definition/theory/solution that is also tested and so on until sufficient progress is achieved. Theorizing is thus continually checked for significance, relevance and effectiveness. The cycle starts with the recognition of a problem (indicated by existing information, which is my first strategy) and proceeds to a careful definition of the problem (my second strategy), which should clarify whether the problem is significant and also indicate a theory of cause that may point to a solution — in other words, a theory that appears relevant and effective. Definition also helps to show what information is necessary and sufficient to solve the problem (so definition assists the implementation of my third strategy). This phase of defining/theorizing/solution design is followed with a testing phase that will initially be restricted to thought experiment (my fifth strategy). The two phases are repeated (which is my fourth strategy) until testing indicates that an adequate solution has been devised. Each repetition should improve the relevance and effectiveness of the definition of the problem, the theory of its cause, the design of the solution and the information gathered to help produce these improvements.
Thought experiment may seem to be too subject to the imaginative idiosyncrasies of the researcher to be useful in this procedure, but it should provoke her to look outside her specialization to see whether her definition, theory, design-solution and information gathering appears relevant and effective in the ‘context and conditions of existence in the whole’ (Held 1991, 4). And, of course, thought experiment may be made more reliable by comparing the thoughts of several people who are well-informed on the problem. This may apply more knowledge and subject different idiosyncrasies to critical review. However, as emphasized in the previous section, this method, or theory of institutional design, finally requires the design to be tested by operating it in the environment in which it was intended to function.