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## Book Reviews

Jandl, Thomas (2013), *Vietnam in the Global Economy – The Dynamics of Integration, Decentralization and Contested Politics*

Plymouth: Lexington Books, ISBN 978-0-7391-7786-0, 312 pages

I found this book very hard to review as its methodology is highly unorthodox. It is developed from a PhD thesis defended at an American University. As such, one would expect it to follow standard rules of research at that level: respect for, and awareness of, situations where its views are contentious; respect for facts; and an awareness of the specificity of its theoretical positions. All of these are, however, lacking.

Any student of Vietnam studies knows that there is a considerable extant literature on the history of the country since reunification in 1975–1976 that discusses her politics, policy histories, social change and so on. This literature is often rather solipsistic, lacking reference to experiences elsewhere, yet it exists and is surprisingly large. It contains a series of important debates and differences. Jandl makes no attempt to survey this literature, nor to situate his own views within it, and so to better contribute to it. This is a pity.

Jandl’s main thesis is that much can be learnt about Vietnam from examining the incentive structures that influenced important choices, such as investments and patterns of governance, especially those at provincial level. He sees incentive structures as importantly influenced by governance, and is concerned to argue that good governance, when it happens, in part comes because local officials avoid rent-seeking behaviour and benefit from this by securing better outcomes locally as higher investment supports growth, better workers’ incomes, better public services and so on. He marshals data to try to show that this is often the case, and treats this as evidence for “harmony of interest” between rulers and ruled.

This is a familiar metaphor, arguing that private capital and a politics that is not really democratic can interact to generate good results for the mass of the population.

For Vietnam, this line of argument owes much to Malesky’s work in the early/mid 2000s, which fed into his construction of the Provincial Competitiveness Index (PCI), which was funded by aid donors. The central point of this is the presence of a positive correlation between variations in levels of foreign investment by province or city and variations in the PCI. The PCI is a composite index made up of a series of

sub-components that are weighted together. This means that how it moves depends upon how this happens arithmetically. It is taken by Jandl to be a proxy for good governance. As such, the PCI needed to have been deconstructed to see how it was made up and how the proxy variable worked in empirical terms. This is qualitative research methodology ‘101’, and Jandl does not do this. There is a discussion in Chapter 4 of the elements of the PCI, but there is no analysis that I can see of how the arbitrary weights used and the particular settings of the PCI’s sub-indices influence how it moves. This is crucial to judging its value as a proxy.

Jandl’s argument pivots on a range of devices, such as the view that much of Vietnam’s post war history is best written as about the factors that influence policy, which of course assumes much: that policy matters, a lot; that policy is decided through relatively rational processes; and that policy outcomes, measured through devices such as Provincial Competitiveness Index, are well-known and cause–effect relationships between policy and outcomes well-established. Much of this is contentious, and given that much of what he states appears as assertion. And much of this argument is to do with just where the *Doi Moi* ideology of the 1986 VI<sup>th</sup> Party Congress came from, and how and why. Anybody familiar with the Vietnam studies literature, or indeed who has argued with a well-informed Vietnamese, should be well aware of the histories that argue contrarily that much happened to support market development before 1986, that much of this was not policy-driven, and that there is no persuasive simple “policy drives change” rationale to what was happening. Jandl does not mention any of this.

Since I myself disagree with Jandl this situation is more than a little difficult to manage in a review. Many statements made are challengeable. This should have been picked up by his supervisors, his examiners or the publisher’s reviewers. Naturally, therefore, I will try to curb my retorts and interjections and look to the footnotes and sourcing to see whether ‘his sources are better’. But usually I find no such sources. For example, the reactive policies towards the commercialisation of the state sector that happened before 1986 can be tracked through the *Official Gazette* (the *Cong Bao*). The introductions to these Decrees are very informative. There is no mention of such sources. Chapter 3 is entitled ‘From roving to stationary bandit – a history’. Its 19 footnotes contain 6 references, all of them singular and there is no discussion of alternative positions. Footnote no. 1 states that “Profit-loss accounting, for example, was not practiced in Vietnam prior to *doi moi*, and accountants had to be brought in from abroad or trained from scratch” (78). No source is given. Stand-

ard Soviet accounting produced profit and loss statements, under different names. Any reading of the Soviet literature on SOE (State Owned Enterprise) management would tell this story. I think that by 1986 many SOE managers had already acquired considerable experience managing commercial profit and loss information through their commercial activities. I can source this through references. Jandl gives no reference for his contrary assertion. Most of his footnotes simply support assertions made in the main text, without adding sources to support the position he takes.

We are told (119) that “Vietnam followed the Chinese model in initially limiting economic reforms to a small part of the country, in Vietnam’s case to Ho Chi Minh City”. The policy record as I know it does not show this, and there is more to policy than simply the allocation of investment licenses (120). Indeed, Jandl’s own account shows how significant investment went to Hanoi and Haiphong, like Ho Chi Minh City possessing significant ‘voice’ in Hanoi’s corridors of power. The question as to whether, under corrupt conditions, allocation of investment licenses reflects ‘policy’, is also contentious. A similar stance towards belief in policy as a key explanatory factor can be found throughout the book. For example, “Ho Chi Minh City’s government has a vision of how to maintain the attractiveness of the province” (187). One could argue that this vision may be articulated, but in reality takes second priority to maintaining corrupt earnings, as education policy shows clearly.

We read that “[...] China serves as Vietnam’s reform model” (227). There are citable published studies (not by me) that strongly disagree with this point. They are not cited.

Data issues are also rather worrying. On page 236 we read that Le Duan was Party General Secretary from 1986 to 1991 and was a Northerner and a *Doi Moi* leader. He was from central Vietnam and died in 1986 before the 1986 VI<sup>th</sup> Party Congress. Again, this should have been picked up by supervisors, examiners and publisher’s reviewers.

Apart from issues of data, Jandl’s theoretical position is also contentious, though he does not engage with alternatives. Thus he states on page 98 “Rent-seeking is always a disincentive to invest in a jurisdiction”. One could argue that the presence of large rents, created in some way or another, increases incentives to invest, and could point to the high levels of investments of human capital in modern financial centres, to the state of the US economy during the robber-baron periods ... and so on. Indeed Jandl pays very little attention to the possibility of rent-creation as a way to create incentives to attract investors, such as through investments in public goods production, infrastructure etc. In technical economic

terms, these also create “profits that cannot be competed away” and are therefore rents.

This book is mainly of interest for what it tells us about its own provenance. Its arguments are driven by provincial thinking that is as certain of its own validity as it is unaware of its own limitations, and cannot be bothered to accept the challenges that come from going out into the wide world outside its own borders. The book is driven by the need to preserve the idea that policy, based upon beliefs and analyses of which it is itself an example, can, should and does drive change. To make these arguments, it ignores the basic rules of scholarship, which are to locate one’s arguments in the wider field of which they are part, a problem that much of the Vietnam studies literature faces, to be careful with facts, and to discuss other theories than those of one’s professors. This is to be somewhat charitable. Jandl writes on page 265 that “Vietnam initiated its reforms out of rational considerations”. A better argument is that the Vietnamese and their politics managed to cope with a transition from plan to market, and then failed to cope with the issues that arose as the market evolved into a form of capitalism. Politics is not political science.

A final observation is to ask what value is being added here by the publisher. Under today’s conditions, where it is possible for scholars like Jandl and myself to distribute work directly, to self-publish, why use a publisher? Marketing is easy, and the potential audience is not large. The valid reason is because a publisher, through their review process, endorses the value of the text. The invalid reason is because it is necessary ‘to publish’ for an academic career. The endorsement function is valuable if done properly. There is food for thought here.

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