Research

Systematic review: Intra-aortic balloon counterpulsation pump therapy: a critical appraisal of the evidence for patients with acute myocardial infarction

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Introduction

Intra-aortic balloon counterpulsation pump (IABP) therapy has been used in several clinical situations, predominantly in critically ill patients, since 1968 [1]. In acute myocardial infarction (AMI) patients who are experiencing continued ischemia, IABP therapy may be used in an attempt to improve patency of an infarct-related coronary artery (IRA) and reduce the rates of recurrent myocardial ischemia and its sequelae. The mechanism for this benefit is thought to be a combination of reduced oxygen demand [2], increased coronary artery blood flow velocity [3], and augmentation of diastolic arterial pressure enhancing thrombolysis, leading to faster reperfusion [4]. IABP therapy may also be used in patients with ventricular septal rupture, severe mitral regurgitation, and cardiogenic shock.

The technique for IABP therapy involves insertion of an 8 or 9.5 Fr helium-filled balloon via the femoral artery into the descending aorta. The device is preferably inserted through an existing vascular access site in an attempt to reduce the rate of vascular and hemorrhagic complications. It is crucial that the tip be positioned distal to the left subclavian artery, but proximal to the renal arteries. The balloon is synchronized to deflate during early systole, thus decreasing left ventricular (LV) afterload. In turn, LV ejection fraction (EF) and stroke volume (SV) are enhanced, leading to reduced myocardial oxygen consumption. The balloon inflates during early diastole, thus increasing coronary blood flow and peripheral perfusion. The IABP is usually commenced at a rate of 1 : 1. Once the benefit of IABP therapy is thought to be concluded, patients are usually gradually weaned from the pump at rates of 1 : 2 to 1 : 3 over 6-12 h.

Following the procedure, one must ensure that the patient has adequate radial artery pulses, suggesting no IABP interference with the subclavian arteries. A chest roentgenogram should be inspected for the location of the IABP marker, which should be 1-2 cm below the aortic arch knuckle. The patient's serum creatinine and urine output should be followed for evidence of IABP interference with the renal arteries. When used to prevent recurrent ischemia post-AMI, all patients who receive IABP therapy should also be prescribed daily aspirin and systemic heparinization with 1000-2000 U/h infused for at least 48 h to maintain activated partial thromboplastin time (aPTT) between 50 and 84 s.

Contraindications to IABP use include severe peripheral vascular disease (PVD), defined as diminished femoral pulses or absent pedal pulses; aortic valve regurgitation (AVR); aortic dissection; tortuous or aneurysmal descending thoracic or abdominal aorta; and patients unable to be systemically heparinized. IABP therapy does not prohibit the use of other medications often used in AMI patients, including aspirin, systemic heparinization, angiotensin-converting enzyme inhibitors, intravenous nitroglycerine, and beta blockers.

Complications of IABP therapy may include limb ischemia and hemorrhage to the femoral access site. A recently developed technique of sheathless insertion may reduce the rate of limb ischemia [5].

Review methods

We performed a computerized search of MEDLINE to identify any randomized trials or economic analyses of IABP

treatment. Terms used were `prospective studies, randomized controlled trial, clinical trial, economic analysis, intra-aortic balloon counterpulsation pump'. We also scanned reference lists for English language studies evaluating IABP treatment *vs* control treatment using randomization methods.

Results from randomized clinical trials of IABP therapy in AMI patients

As with all analyses of interventions used to treat AMI patients, IABP therapy must be subdivided into the pre-and post-thrombolytic eras. Two small randomized trials in the prethrombolytic era, enrolling a total of 50 patients, failed to show a survival benefit, reduction in infarct size, or improvement in global LV function [6,7]. Although thrombolytic therapy has greatly improved the prognosis of patients with AMI, the inhospital mortality rate of AMI patients with cardiogenic shock remains as high as 55% [8]. The main cause for early mortality (within 48 h of onset of AMI symptoms) in thrombolytic-treated patients is LV failure [9]. In the post-thrombolytic era there have been three randomized trials of prophylactic IABP therapy in AMI patients.

In 1994, the Randomized IABP Trial, which enrolled 182 patients from 11 centers, evaluated the benefit-risk ratio of IABP during the early phase of AMI [10]. Inclusion criteria were an emergency cardiac cathererization within 24 h of an AMI which demonstrated an occluded IRA at first angiography, and restored IRA patency by primary angioplasty ($n = 106$), intracoronary thrombolysis ($n = 25$), or rescue angioplasty following failed thrombolysis ($n = 51$). Exclusion criteria were hemodynamic instability necessitating the use of an IABP (it was considered unethical to withhold IABP therapy from such patients), severe PVD, bleeding diathesis prohibiting the use of extended intravenous heparin therapy, or IRA patency at first angiography. This study randomized patients at the end of cardiac catheterization or angioplasty to receive 48 h of prophylactic IABP therapy ($n = 96$) or standard care ($n = 86$). The primary endpoints was IRA reocclusion at 5- to 7-day repeat quantitative coronary angiography (QCA), which was performed in 162 (89%) of the patients. Only 8% of patients randomized to IABP had IRA reocclusion during this short-term follow-up, compared to 21% of controls ($P < 0.03$). The secondary endpoint was a composite inhospital clinical endpoint of death, stroke, reinfarction, emergency revascularization, or recurrent ischemia. This composite secondary endpoint occurred in 13% of IABP patients, compared with 24% of controls ($P < 0.05$). No significant differences were observed between the two groups with regards to severe bleeding complications, vascular repair, or thrombectomy.

In 1996, Kono *et al*[11] published the results of a trial which enrolled 45 patients with AMI from one center in Japan. Patients were included if their AMI was unsuccessfully treated with thrombolysis (tPA within 12 h of onset of symptoms). Failed thrombolysis was identified by coronary angiography, performed 1 h after initiation of intravenous thrombolysis, which revealed persistent occlusion or partial reperfusion of the IRA. Rescue angioplasty was not attempted in any of these patients. This study randomized patients at the end of initial coronary angiography to 48 h of prophylactic IABP therapy ($n = 23$) or standard care ($n = 22$). The primary endpoint was IRA patency (defined as Thrombolysis in Myocardial Infarction flow grade 3) 3 weeks after AMI. Significantly more patients in the IABP therapy group had IRA patency than patients randomized to standard care (74% *vs* 32%, $P < 0.05$). The secondary endpoints were recurrent ischemia, malignant ventricular dysrhythmias, death, stroke, or need for coronary artery bypass grafting. There were no statistically significant differences in any of these clinical endpoints between the IABP and standard therapy groups at 3-week follow-up. As with the Randomized IABP Trial, there were no significant differences observed between the two groups with regard to severe bleeding complications, vascular repair, or thrombectomy.

In 1997, the Second Primary Angioplasty in Myocardial Infarction (PAMI-II) trial determined the role of prophylactic IABP therapy after primary angioplasty in AMI patients [12]. This trial enrolled 437 high-risk, but hemodynamically stable patients from 34 centers worldwide. Inclusion criteria were ongoing chest pain up to 12 h in duration, electrocardiographic (ECG) evidence of AMI, an occluded coronary artery with regional LV dysfunction, and high-risk status (one or more of age > 70 years, three-vessel coronary artery disease, LV EF < 46%, saphenous vein graft occlusion, persistent malignant ventricular dysrhythmias, or a suboptimal angioplasty result). Exclusion criteria were hemodynamic instability necessitating the use of an IABP, cardiogenic shock, bleeding diathesis prohibiting the use of aspirin or heparin, precatheterization administration of thrombolytic therapy, PVD, aortic aneurysm, and AVR. This study randomized patients at the end of primary angioplasty to receive 36-48 h of prophylactic IABP therapy ($n = 211$) or standard care ($n = 226$). The primary endpoint was a composite predischarge clinical endpoint of death, stroke, reinfarction, IRA reocclusion, new onset congestive heart failure (CHF), or sustained hypotension. There was no statistically significant difference in this composite endpoint between IABP (28.9%) and standard therapy (29.2%) groups ($P = 0.95$). However, significantly more IABP therapy patients had an inhospital stroke than those treated with standard therapy (2.4% *vs* 0, $P = 0.03$). PAMI-II concluded that major benefits or hazards of routine prophylactic IABP therapy are unlikely to exist.

The reason why the Randomized IABP Trial demonstrated a beneficial effect of IABP therapy, but PAMI-II did not, may

be the fact that the control group in PAMI-II had a better than expected outcome. Most notably, the IRA reocclusion rate in the standard therapy group in PAMI-II was 5.5%, compared to 20.8% in the Randomized IABP Trial control group. This was an unexpected finding, since patients in PAMI-II were generally older and had poorer LV function than those in the Randomized IABP Trial.

Critical appraisal of randomized clinical trials of IABP therapy in AMI patients

Thorough critical appraisal involves determining the validity of the study design, understanding the results and deciding whether and how the results may be applied in practice. The factors to consider when critically appraising articles on therapy are summarized in Table 1[13–15].

In determining the validity of a therapeutic article, we ask whether the results represent an unbiased estimate of the treatment effect [13]. First, we ask if the assignment of patients was randomized? In the Randomized IABP Trial [10], randomization took place at the end of the initial emergency catheterization, before the patient was transferred to the critical care unit (CCU). The randomization was stratified by clinical site, and utilized permuted block randomization within each center to maintain chronological balance in the number of patients allocated to each treatment arm. In the trial by Kono *et al*[11], patients were enrolled using a predetermined randomization list along with sealed envelopes. The PAMI-II trial [12] did not state their exact method of randomization. Although there is no direct evidence in either of these three trials of lack of concealment, a situation where a clinician may know in advance that a patient will be allocated to either the treatment or placebo group, this possibility cannot be excluded.

The second question to address when determining validity is whether all patients who entered the study were accounted for and attributed at its conclusion. In the Randomized IABP Trial [10], only 162 (89%) of the 182 randomized patients had 5- to 7-day follow-up QCA. The reasons for lack of angiographic follow-up were death (two), coronary artery bypass graft (CABG) surgery before follow-up QCA (five), patient refusal (seven), and medical contraindication (six). Follow-up for the composite clinical endpoint, however, was 100%. In the trial by Kono *et al*[11], 3-week follow-up for QCA and clinical endpoints was 100%. In PAMI-II [12], only 330 (85%) of 389 eligible patients had predischarge follow-up QCA, although the composite predischarge clinical endpoint was determined on all patients. It is crucial that clinical endpoint follow-up is complete, otherwise bias may be introduced, since patients who are lost often have different prognoses from those who are retained. Neither the Randomized IABP Trial nor PAMI-II performed a sensitivity analysis on the QCS follow-up data.

Another important aspect of follow-up relates to whether patients were analyzed in the groups to which they were randomized. In the Randomized IABP Trial [10], although seven (8%) standard therapy patients crossed-over to the IABP group, and nine (9%) IABP therapy patients required premature termination of the IABP within 24 h, analysis was conducted on an intention-to-treat basis. Similarly, in PAMI-II [12], although 26 (12%) standard therapy patients received an IABP, and 29 (14%) patients randomized to IABP did not receive the device, analysis was also conducted on an intention to treat basis.

An important secondary guideline in determining validity relates to whether patients, clinicians, and study personnel were blinded, thus preventing observer bias and cointervention. In the Randomized IABP Trial [10], it was not stated if the physicians analyzing the QCA films were blinded. Kono *et al*[11] stated the QCA results were analysed in a blinded manner, but did not provide specifics of this blinding process. In PAMI-II [12], the independent core laboratory angiographic analysis was performed by a technician in a single-blinded manner with regard to the randomization scheme.

We also need to determine if the study groups were similar at the start of the trial, since randomization does not always produce groups balanced for known prognostic factors. In the Randomized IABP Trial [10], the baseline clinical and angiographic characteristics in the two groups were similar, except that the IRA was more frequently the left anterior descending coronary artery in IABP therapy patients (49% *vs* 35%; *P* value not stated). Conversely, more standard therapy patients had the right coronary artery as the IRA (54% *vs* 34%; *P* value not stated). In both the trial by Kono *et al*[11] and PAMI-II [12], patient groups were well matched following randomization.

Another assessment of validity related to whether the groups were treated equally, aside from the experimental intervention. In the Randomized IABP Trial [10], angioplasty was used to restore patency in 90% of patients later randomized to IABP therapy, and 83% later assigned to standard care. Intracoronary thrombolysis was used in 42% of patients later randomized to IABP therapy compared to 46% of patients in the standard therapy arm. Intravenous heparin was used for a mean of 5 days in both the IABP and standard therapy arms. In Kono *et al*'s trial [11] and PAMI-II [12], thrombolysis and primary angioplasty, respectively, were used exclusively. In none of these three trials was there evidence of contamination, a situation where control patients accidentally receive experimental treatment, or cointervention, a circumstance where additional diagnostic or therapeutic procedures are performed on experimental, but not control, patients.

Once the validity of a clinical trial is established, we can then focus on the results. The first consideration is the size of the treatment effect. Results from the Randomized IABP trial [10] suggest that eight patients needed to be treated with prophylactic IABP therapy for 48 h to prevent one patient from developing IRA reocclusion 5- to 7-days post-AMI. Furthermore, 13 patients needed to be treated with an IABP to prevent one patient from sustaining an inhospital death, stroke, reinfarction, emergency revascularization, or recurrent ischemia. Kono *et al*'s trial [11] suggested better results, where only two patients needed to receive IABP therapy for 48 h to enable the IRA to be patent in one patient 3 weeks post-AMI. PAMI-II [12] demonstrated equivalence between IABP and standard therapy, although for every 42 patients treated with an IABP, one extra stroke resulted ($P = 0.03$). Not only is the size of the treatment effect important, but so too is the precision. Unfortunately, neither of these three trials reported 95% confidence intervals.

In determining the clinical application of an article on therapy, we ascertain if the results are useful in practice. The results of the Randomized IABP Trial [10] are applicable to patients with AMI undergoing immediate cardiac catheterization. However, the Randomized IABP Trial confirmed previous observational data [16,17] that IABP therapy is particularly important in situations in which IRA patency is critical for survival, such as in patients with cardiogenic shock. In addition, both the Randomized IABP [10] and PAMI-II [12] trails did enrol patients in numerous centers ranging from community hospitals to large academic centers, suggesting their respective results may be generalizable to most hospitals commonly using IABP. Only high-risk patients were included in PAMI-II, and no patients in Kono *et al*'s trial received rescue angioplasty. The therapeutic maneuver in these trials, specifically insertion and use of an IABP, is described in sufficient detail and is available, acceptable and affordable in many centers caring for critically ill patients.

These three trials generally met other important considerations when determining the clinical application of a therapeutic article; namely, both statistical and clinical significance were considered, and all clinically important outcomes (both beneficial and adverse), other than quality of life issues, were assessed objectively and reported. Whether the resources required to use IABP are better spent pursuing this, rather than some other intervention, will be discussed below when considering the economics of IABP therapy.

Economic analysis of IABP therapy in AMI patients

The economic implications of prophylactic IABP in sustaining IRA patency are worth considering. A reduction in recurrent ischemia and repeat revascularization procedures may initially lead to reduced costs, but the expense of inserting an IABP and potential peripheral vascular and hemorrhagic complications arising from its use may offset these initial cost savings.

An economic analysis of the Randomized IABP Trial was recently performed on 102 patients (from three centers) (56%) of the 182 patients from the original 11 participating centers [18]. Hospital bills for this subset of patients randomized to either 48 h of IABP or standard therapy were assessed using each hospital's Medicare cost report billing data and correction factors to convert charges to costs. Thus, the specific subtype of economic appraisal performed was a form of cost-benefit analysis, since it measured both resources used and health effects in monetary units. The results are expressed in dollar values, rather than as costs per quality adjusted life year (QALY). However, unlike traditional cost-benefit analyses, this paper did not value health consequences by asking patients what they would be willing to pay for health services that achieve outcomes of particular types. By expressing both costs and health benefits in monetary units, Talley *et al*[18] facilitate the calculation of IABP therapy's net benefit, which gives policy decision-makers a single measure of its desirability from an efficiency perspective.

Table 1

Factors to be considered when critically appraising articles on therapy

Validity
Primary guides
Was assignment of patients to treatments really randomized?
Were all patients who entered the study accounted for and attributed at its conclusion?
Secondary guides
Were patients, clinicians, study personnel blinded?
Were the groups similar at the start of the trial?
Aside from experimental intervention, were the groups treated equally?
What were the results?
How large was the treatment effect?
How precise was the treatment effect?
Clinical application
Are the results applicable to my patients?
What is the net impact of the treatment?

Critical appraisal of the economic analysis of IABP therapy in AMI patients

The factors which we consider when critically appraising an economic analysis are summarized in Table 2[19,20]. In determining the validity of an economic analysis, we ascertain if the results yield an unbiased assessment of the costs

and outcomes, and whether the economic analysis truly determines which of the clinical strategies provides the most benefit for the available resources. The first consideration in assessing validity is whether the analysis provides a full economic comparison of healthcare strategies. Talley *et al*.'s cost-benefit analysis [18] compared all relevant clinical strategies by determining total index hospitalization costs from admission to discharge, including all diagnostic and therapeutic procedures, as well as all outcomes, including angiographic and clinical complications. The viewpoint adopted by this analysis, however, was not broad since data for this economic analysis was only based on inhospital billings. It may be argued that from an economic standpoint, the advantages or disadvantages of IABP therapy may be eroded if the costs incurred during a longer duration of postdischarge follow-up were analyzed. Furthermore, quality of life issues were not estimated.

The next question to address is whether all relevant costs and outcomes were properly measured and valued. It is questionable whether this analysis established the true clinical effectiveness, since the design of the Randomized IABP Trial involved mandatory 5- to 7-day repeat QCA. Some patients with occluded coronary arteries remain clinically silent, and because repeat QCA does not always occur in clinical practice as it did in this trial, Talley *et al*[18] should have adjusted their analysis to assume that incomplete efficacy is actually achieved in clinical practice. Talley *et al*'s costs were measured accurately by applying correction factors to convert charges and costs.

There are two secondary guides to consider when determining the validity of an economic analysis. Talley *et al*[18] did not perform a sensitivity analysis, and thus appropriate allowances were not made for uncertainties in the analysis. Costs and outcomes were, however, related to different baseline risks within the treatment population, and these results are discussed below.

After the validity of the economic analysis has been established, determining the incremental costs and outcomes of each strategy is the first component one assesses when examining the results. The overall inhospital costs for patients who received an IABP were not significantly increased ($22,367 ± $14,369) compared to those who did not ($19,211 ± $8414; $P = 0.45$). The authors therefore concluded that IABP provided a better clinical outcome without substantially increasing hospital costs. Talley *et al*[18] also determined whether the incremental costs and outcomes differed between subgroups. Patients who received an IABP and also developed recurrent ischemia had significantly higher inhospital costs ($23,125 ± $7690) compared to patients who had recurrent ischemia but were not randomized to IABP therapy ($20,416 ± $12,449; $P = 0.02$). Similarly, patients who experienced an adverse composite clinical secondary endpoint and received an IABP had significantly higher costs ($25,598 ± $10,024) than similar patients not randomized to IABP therapy ($19,790 ± $12,045; $P = 0.002$). However, a sensitivity analysis was not used to assess the robustness of the conclusions.

Table 2

Factors to be considered when critically appraising an economic analysis

Validity
Primary guides
Did the analysis provide a full economic comparison of health care strategies?
Were all relevant costs and outcomes properly measured and valued?
Secondary guides
Was appropriate allowance made for uncertainties in the analysis?
Are estimates of costs and outcomes related to the baseline risk in the treatment population?
What were the results?
What were the incremental costs and outcomes of each strategy?
Do incremental costs and outcomes differ between subgroups?
How much does allowance for uncertainty change the results?
Clinical application
Are the treatment benefits worth the costs and potential harms?
Could my patients expect similar health outcomes from using the intervention?
Could I expect similar costs?

In determining the clinical application of an economic analysis, clinicians determine whether the results are useful in practice. Given that IABP therapy produces higher inhospital costs, but a better clinical outcome (at least in the Randomized IABP Trial), we ask if the added treatment effect is worth the added costs? Talley *et al*[18] did not calculate incremental cost-effectiveness ratios of IABP therapy, thus leaving the issue of the trade-off between increased costs and possibly increased effectiveness unresolved. Given that the Randomized IABP Trial was undertaken in a variety of community hospitals and academic centers, it is reasonable to assume that similar health outcomes and costs could be expected from using IABP therapy, provided one's center has competent clinicians to insert the devices.

Conclusion

The potential benefits of careful use of IABP therapy are unlikely to be offset by vascular and hemorrhagic complications. In the final analysis of IABP cost-effectiveness, clinicians and institutions need to decide what health benefits will be foregone from other treatments or programs if resources are diverted instead to routine IABP prophylaxis post-AMI.

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